The Cleanup of US Manufacturing through Pollution Offshoring*

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Abstract

We study the role of offshoring in understanding long-run environmental impacts of trade liberalization and the cleanup of US manufacturing. Leveraging detailed establishment-level data and a change in US trade policy toward China in the early 2000s, we show that US establishments decrease toxic emissions in response to a reduction in trade policy uncertainty. Emission abatement is more pronounced for establishments that are more likely to engage in offshoring activities. We provide comprehensive evidence that supports the pollution offshoring hypothesis: US manufacturers, especially those that emit pollutants intensely, source from abroad and establish more subsidiaries in China following the event.

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1 Introduction

Since the late 20th century, toxic manufacturing emissions have declined in many developed countries, in stark contrast to a noticeable increase in industrializing middle- and low-income countries. The *pollution offshoring hypothesis* offers a compelling explanation for this global trend, which posits that trade liberalization induces firms in developed countries to relocate high-polluting activities to developing countries with laxer environmental regulations (Copeland and Taylor, 2004; Copeland, Shapiro, and Taylor, 2022). Despite the plausibility of the mechanism and active public discussions on coordinating trade and environmental policies across borders (e.g., the Carbon Border Adjustment Mechanism), however, the empirical evidence on whether and how trade liberalization causes such global relocation of pollution-intensive tasks still remains elusive.[1] Importantly, less is known about the role of offshoring as an explanatory factor for the observed global trends, let alone the decline of pollution emitted by US manufacturers in the same period.

When it comes to the cleanup of US manufacturing, prior studies have mainly attributed the decline in emissions to changes in environmental regulations and technology (see, e.g., Greenstone, 2002; Levinson, 2009; Shapiro and Walker, 2018). The role of offshoring, however, has been surprisingly underexplored in this context, despite the fact that environmental regulations are fundamentally linked to competitiveness of firms in global trade (Greenstone, List, and Syverson, 2012).[2] The dearth of empirical evidence may originate from the lack of plausibly exogenous trade liberalization episodes, micro-level datasets, identification strategies, or any combination thereof, needed to establish a causal linkage between trade liberalization and the cleanup of US manufacturing via offshoring.

In this paper, we draw on arguably the most significant trade liberalization episode—the US granting permanent normal trade relations (PNTR) status to China—and find compelling evidence of the pollution offshoring hypothesis. A priori, the impact of reducing trade policy uncertainty on US establishments' toxic emissions remains ambiguous. For example, the surge of imports from China could drive US manufacturers to prioritize cost-saving measures over environment-friendly practices, which may lead to increased emissions. Conversely, the same competitive pressure can lead to a reduction of production scale or closures, potentially resulting in decreased emissions. In addition, as the trade policy change facilitates the offshoring of pollution-intensive tasks, emissions may fall. Our results show that (i) US manufacturers

[1]While there is ample theoretical and empirical support on the causal linkages between *environmental regulation changes* and cross-regional movement of pollution-intensive tasks, whether *reductions of trade barrier* triggers such a movement has relatively received scant support (Copeland and Taylor, 2004; Copeland, Shapiro, and Taylor, 2022).

[2]Offshoring is frequently recognized as one of the most important drivers of global integration during the last few decades (Feenstra, 1998; Hummels, Ishii, and Yi, 2001).

decreased toxic emissions in response to a reduction in trade policy uncertainty and that (ii) offshoring, rather than competitive pressure, played a pivotal role in improving the environmental performance of US manufacturing.

To better understand the role of international trade, especially offshoring, in the reduction of pollution emissions in US manufacturing, we exploit rich longitudinal data from the Toxics Release Inventory (TRI), together with the National Establishment Time Series (NETS) database. We begin by studying aggregate patterns of the data over the sample period (1997-2017) which we summarize as three stylized facts. First, US manufacturing exhibits a decline in aggregate levels of pollution emissions with increased effort in waste management. Second, a decomposition exercise shows that the aggregate decline in manufacturing toxic emissions is primarily driven by within-industry adjustments through surviving establishments. Third, such within-establishment decreases in pollution emissions are more pronounced in industries comprising establishments that engaged intensively in imports, but not in exports.

Motivated by the importance of within-establishment adjustments and their negative correlations with import intensities, our study employs a generalized difference-in-differences research design to further explore the causal effects of the trade liberalization on pollution emissions in US manufacturing. Our identification strategy follows Pierce and Schott (2016) and leverages the conferral of Permanent Normal Trade Relations to China in early 2000s, which generates exogenous variations in changes in the degree of US-China trade policy uncertainty across industries. 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. The outcomes of these votes were highly unpredictable. However, in October 2000, the US Congress granted China PNTR status, permanently eliminating such uncertainty and setting low NTR tariff rates on US imports from China. Our measurement of the reduction in trade policy uncertainty uses the *NTR Gap*, which is the difference between the non-NTR tariff rates to which tariffs would have risen had annual renewal failed and the low NTR tariff rates.

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. 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, dropping outliers, and incorporating various weighting schemes.

Further analyses delving into assessing the within-establishment emission reductions reveal that offshoring, rather than competition, is potentially an important channel through which adjustments occur. Specifically, the reduction in pollution emissions is not driven by establishments' exits nor reductions of the production scale—a channel through which the competitive pressure can directly affect emissions—, but instead, primarily driven by a decline in pollution emission intensity within an establishment. We further find that the reductions in pollution emissions are more substantial for US establishments that were more able and willing to offshore production. That is, US manufacturers—(i) having existing foreign business relationships, (ii) having more incentives to move away from stricter environmental regulations, (iii) operating in upstream industries along the supply chains, and (iv) belonging to a multi-sector firm—indeed show greater reductions in pollution emissions.

Finally, we provide direct evidence supporting the pollution haven hypothesis. Using time-varying establishment-level importing status to proxy for global sourcing activities, we find that US establishments initially associated with high-polluting tasks are more likely to engage in sourcing activities than other establishments after PNTR. Further merging with the Wharton Research Data Services (WRDS) Company Subsidiary Data, we find that PNTR induces US manufacturers to establish more foreign subsidiaries in China, but not in other countries, and that such effects are more pronounced for establishments with high-polluting activities. Additionally, using HS 10-digit product-by-year-level data from the UN Comtrade database, we demonstrate that such offshoring and FDI activities after PNTR resulted in increased reliance on imports from China, especially for products that are manufactured by dirty industries according to US standards. Lastly, we provide detailed discussions on (i) how the cleanup of US manufacturing has been achieved via *offshoring*, (ii) alternative channels, such as competition and PNTR-induced clean technology adoption, and (iii) the implications of pollution emissions in China.

Contributions to the Literature

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

First, we contribute to the deep and important line of studies examining the cleanup of US manufacturing (Copeland and Taylor, 1994; Grossman and Krueger, 1995; Antweiler, Copeland, and Taylor, 2001) and highlight the role of international trade as an important adjustment channel in pollution emissions. Prior studies have mainly attributed the decline in emissions to advancements in production or abatement processes (Levinson, 2009) and changes in environmental regulation (Greenstone, 2002; Shapiro and Walker, 2018). However, the role of trade, which had been primarily associated with a channel causing shifts in industry compositions, has received relatively less attention. Leveraging granular establishment-level data on emissions, we find consistent results with the existing studies (Holladay and LaPlue III, 2021) in highlighting that within-establishment adjustments predominantly contribute to the aggregate declines in emissions. Adopting the identification strategy of Pierce and Schott (2016), our research design further enables us to examine and quantify the long-run causal impact of trade on within-establishment emission reductions. This offers a new perspective on the role of trade as a driving mechanism behind the within-industry dynamics of pollution emissions in US manufacturing.

Second, we contribute to the literature studying the impact of trade on environmental outcomes by offering direct and comprehensive establishment-level evidence of offshoring as a mechanism for reducing emissions. A burgeoning strand of recent papers in the field use firm- or establishment-level data to look for causal effects of international trade on emissions in various countries: India (Martin, 2011; Barrows and Ollivier, 2021), the United States (Holladay, 2016; Cherniwchan, 2017), European countries (Akerman, Forslid, and Prane, 2021; Dussaux, Vona, and Dechezleprêtre, 2023; Leisner et al., 2023), Mexico (Gutiérrez and Teshima, 2018), and China (Bombardini and Li, 2020; Rodrigue, Sheng, and Tan, 2022). A few papers (partly) focus on offshoring either relying on tariff measures reflecting input-output linkages (Cherniwchan, 2017) or shift-share type of shocks using trade flow data to proxy exogenous shifts in offshoring (Dussaux, Vona, and Dechezleprêtre, 2023; Leisner et al., 2023). Our approach exploits detailed data on establishment-level imports, firm-level subsidiaries, and product-level imports to obtain information on offshoring, which allows us to measure offshoring activities in a comprehensive manner, and further examine its impact in explaining the within-establishment adjustments in emissions.

Note that these results offer evidence supporting the pollution offshoring hypothesis, thereby contributing to studies on the pollution haven effect, a notion that links changes in environmental regulations and cross-border shifting of pollution-intensive industries.[3] To date,

[3] Despite the frequent mixture of usage in previous studies, Copeland and Taylor (2004) formally distinguish the "pollution haven hypothesis" and the "pollution haven effect." The pollution haven hypothesis asserts that *a reduction in trade barriers* will lead to a shifting of the pollution-intensive industry from countries with stringent regulations to those with weaker regulations, while the pollution haven effect states that *a tightening of environmental regulation* will generate such a movement across regions—which is also referred to as "carbon leakage" in the context of greenhouse gas emissions. Copeland and Taylor (2004) and Copeland, Shapiro, and Taylor (2022) note that the pollution haven hypothesis has relatively scant theoretical and empirical support than the pollution haven effect because many other factors—in addition to environmental policy—can affect trade flows. The crucial distinction between the pollution haven hypothesis and the pollution offshoring hypothesis is that the former emphasizes trade-liberalization-induced "industry" specialization (i.e., developed countries specialize in clean industries while developing countries specialize in dirty industries), while the latter underscores

empirical evidence in this literature has been elusive and 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, Teshima, and Verhoogen, 2022; Bartram, Hou, and Kim, 2022), whereas others are broadly consistent with weak pollution haven effects (e.g., Eskeland and Harrison, 2003; Hanna, 2010).[4] Unlike previous studies, we leverage an episode of trade policy uncertainty reduction, neither variations in environmental regulations nor actual changes in tariffs, to study the pollution offshoring mechanism. To the best our knowledge, our paper is the first to provide comprehensive evidence of the pollution offshoring hypothesis by directly investigating offshoring-related activities of manufacturing establishments and analyzing imports of dirty products from China to the US.

Third, 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 (Handley and Limao, 2015; Handley and Limão, 2017, 2022; Caliendo and Parro, 2021). Despite its importance in explaining investment and trade dynamics in the post-2000 period, less is known about the impact on the cleanup of US manufacturing. By emphasizing the importance of uncertainty in investment decisions when countries open up to trade, our study enriches the ongoing discussion on the pollution offshoring hypothesis and shows that a decline in trade policy uncertainty could be a powerful force that leads to significant pollution abatement, even without changes in actual tariffs.

Finally, we contribute to the literature studying the China trade shock, which has significant impacts on labor market outcomes (Autor, Dorn, and Hanson, 2013; Pierce and Schott, 2016; Choi and Xu, 2020; Kim, 2022), innovation (Bloom, Draca, and Van Reenen, 2016; Autor et al., 2020b), political outcomes (Che et al., 2016; Autor et al., 2020a), health (Pierce and Schott, 2020), product scope adjustment (Choi et al., 2022), and internal migration (Greenland, Lopresti, and McHenry, 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.

The remainder of the paper is organized as follows. Section 2 describes the data. Section 3 presents stylized facts on US manufacturing emission trends. Section 4 details the empirical strategy. Section 5 presents the main results. Section 6 discusses mechanisms and reports results supporting the offshoring channel. Section 7 concludes the paper.

trade-liberalization-induced within-firm (and therefore, within-industry) relocation of dirty tasks across borders.

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

2 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 data sources, sample construction, and descriptive statistics.

2.1 Data Sources

Toxics Release Inventory (TRI) We obtain facility-chemical-level releases of toxic materials (1987-2020) provided through the EPA's TRI database. The program is mandatory for facilities that meet the following TRI reporting criteria: (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. Facilities that are non-compliant are subject to further investigation and possible enforcement actions by the EPA and TRI has several institutional features to optimize and maintain the quality of data (see Appendix A for details on the institutional background of TRI Program).

For each reporting facility, we observe detailed information on the chemical (including chemical name, acuteness in human health effects, carcinogenicity, and the severity of environmental effects) and the chemical-specific amount of production waste generated on-site and transferred to off-site locations. The data add breakdowns of how each facility manages this chemical waste. One is the amount "released" (or emitted) to the air, water, (or placed into) land, which directly affects the environment. The other is the amount recycled, treated, or combusted for energy recovery, which speaks to facility-level effort in effectively managing waste. In addition, we also have information on the various types of pollution prevention (P2) activities that facilities conduct to reduce waste at the source. Detailed descriptions of 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.[5]

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. These updates were intended to better provide data on

[5]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.

exposures to toxic chemicals and the environmental performances of US facilities.[6] From an empirical perspective, the increasing list of TRI-covered chemicals, a subset of which face lower thresholds, can mechanically increase the reported amount after these policy changes. Therefore, our analyses carefully address these issues in the sample construction, and we conduct a series of robustness checks, which we describe in later sections. After restricting to a list of chemicals of interest, we apply a crosswalk obtained from the National Emissions Inventory (NEI) to map relevant chemicals to PM_{10}.[7] Throughout our analyses, we collapse the data and focus on facility-level waste production of this major pollutant.

Our decision to use TRI as the primary data source is worth discussing, especially considering other available options such as the National Emissions Inventory (NEI). First, TRI provides a common establishment identifier (DUNS number) that is readily linkable to the near-universe of the US establishment panel dataset—the NETS data.[8] Second, the transparency of TRI data in providing chemical-level details on reporting criteria, release media, and amount, which aligns with its primary purpose to inform the public and policymakers about toxic emissions, facilitates a rigorous and consistent tracking of changes in emissions over a long time horizon.[9] Third, to the best of our knowledge, TRI-NETS is the only available data combination that provides the yearly frequency of establishment-chemical-level toxic emissions, together with a vast set of establishment and firm-level business characteristics.[10] This is a critical data requirement for the purpose of our paper—identifying a causality through a difference-in-differences-type design—compared to other analyses (e.g., model estimations and decomposition exercises). To address the often raised issue on the quality of TRI (Khanna, 2019), we ensure that our results are not driven by potential erroneous emission records (Table E.11), in addition to working with a consistent set of chemicals throughout the sample period (see Section 2.2).[11] Reassuringly, the aggregate data patterns from our sample are consistent with those documented in the literature (Shapiro and Walker, 2018).

[6]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.

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

[8]NETS manually and independently cross-checks the quality of linkage between TRI and NETS using various available information (e.g., name, address, etc.), which ensures the quality of TRI-NETS crosswalk.

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

[10]NEI is only available at the triennial frequency and can be matched with other establishment-level datasets through algorithm-matching techniques, which can introduce additional noise in the measurement.

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

National Establishment Time Series (NETS) To study establishment-level responses in waste production relative to size (employment and sales) as well as various heterogeneity in the effects, we obtain establishment-specific characteristics from the NETS database, which is an annual panel of a near universe of US establishments (1990-2020). In NETS, we observe establishment-level industry code, 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. A number of studies have demonstrated the accuracy of the information in NETS data (Neumark, Zhang, and Wall, 2006; Neumark, Wall, and Zhang, 2011; Barnatchez, Crane, and Decker, 2017).[12] 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 TRI-reported DUNS Numbers, 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.[13]

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.[14] 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).

[12]For example, Barnatchez, Crane, and Decker (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, Wall, and Zhang (2011) document the accuracy of entry and exit information of establishments. For recent studies that use the NETS database, see, e.g., Gray, Siemsen, and Vasudeva (2015); Asquith et al. (2019); Rossi-Hansberg, Sarte, and Trachter (2021); Behrens et al. (2022); Hyun and Kim (2022); Choi, Hyun, and Park (2022); Oberfield et al. (2022).

[13]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.

[14]We linked parent companies in WRDS Subsidiary data with headquarters companies in NETS data through a probabilistic record linkage algorithm. We use the company name and address information in the two datasets to perform record linking (using the Stata command RECLINK2), which we also manually verify.

U.S. Historical Tariff Rates We obtain NTR and non-NTR tariff rates provided by Pierce and Schott (2016), which sources data from Feenstra, Romalis, and Schott (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).

2.2 Sample Construction

The matching of the TRI-NETS data between 1987 and 2020 results in 2,809,810 observations with chemical-establishment-year level release amounts for 54,224 establishments covering 660 chemicals, 27 of which are mapped to PM_{10}. Below, we describe how 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 Hexabromocyclododecane (HBCD) chemicals—introduced in the subsequent years (see Appendix B for more details).[15]

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 the participation of Native Americans in 2012. To maintain consistency on this end, we keep establishments that are not located in *Indian country*.[16]

We exclude periods with prevailing impacts of major events (e.g., the US-China Trade War, the pandemic, the North American Free Trade Agreement (NAFTA) agreement) that might have confounded the effects of our treatment, and thus, restrict our sample period to years between 1997 and 2017. 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

[15]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

[16]Appendix Table E.1 provides further details related to these policy changes.

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.

2.3 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, including 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 E.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.[17] Another important feature is that the NTR gap, our measurement of the shock, 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—the within-firm employment share of establishments that engaged in import activities—in 1997 is 13.5 percent.[18] 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 TRI's reporting threshold of 10 or more full-time employees, the sample firms are relatively large compared to the entire distribution.[19] For the establishment-year-level observations, the average number of employees in 1997 is 21,655 with a median of 1,870, which indicates a highly right-skewed distribution.[20]

[17]Appendix Table E.11 shows that our results are not driven by these extreme observations.

[18]This measure captures the importance of import activities within a firm. We cannot weight by import values since the NETS only provides a binary indicator of whether an establishment engages in import activities.

[19]See Appendix Table E.4 for the comparison of our final sample distribution with the manufacturing sample distribution from the original NETS data. 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.

[20]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 E.4, the mean and median numbers of

Table 1: Summary Statistics

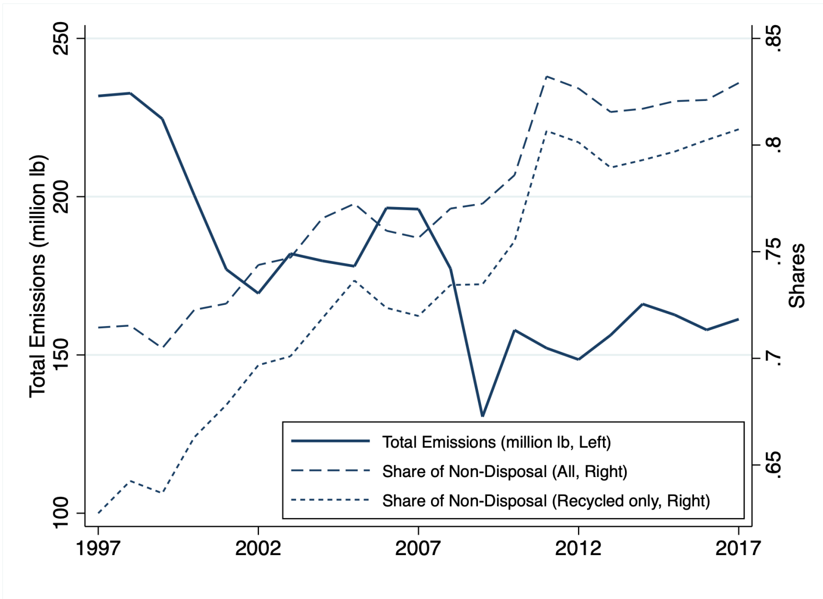
Establishment-Year Level						
Variable	Obs.	Mean	Std. Dev.	P10	P50	P90
PM Emissions$_{p,t}$ (lb)	46753	50838	450609	10	719	36605
NTR Gap$_{i,99}$	46753	0.294	0.119	0.138	0.304	0.424
NTR$_{i,t}$	46753	2.480	2.037	0.000	2.342	5.162
MFA Exposure$_{i,t}$	46753	0.098	1.493	0.000	0.000	0.000
NP$_{i,95}$/Emp$_{i,95}$	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 dollars)	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 dollars)	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. See Appendix Table E.3 for the summary statistics at various aggregation levels (industry-year, industry, firm, establishment, and county-level).

3 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 cleanup of manufacturing found in previous studies (e.g., Levinson, 2009; Shapiro and Walker, 2018; Najjar and Cherniwchan, 2021) is also present in our data. The solid line in Figure 1 shows the time series of the aggregate levels of PM_{10} waste released or emitted into the air from 1997 to 2017, where we find a 30 percent drop. Appendix Figure D.1 reveals that most of these aggregate changes are driven by establishments in 2-digit SICs 28 and 33, which are Chemicals and Allied Products and Primary Metal Industries, respectively.[21] In fact, these two industry categories represent a predominant share of the initial PM_{10} emissions from manufacturing establishments.[22] However, we also note that there is also an overall decline in PM_{10} emissions in other industries.

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.

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

[22] Appendix Figure D.2 shows the industry distribution of employment and PM_{10} emissions in our sample.

The detailed breakdown of waste management in TRI allows us to understand the cleanup 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.[23] 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). In sum, Figure 1 reveals that the aggregate emissions from manufacturing establishments declined during the past two decades, while the share of non-disposal, which captures waste management efforts, steadily increased over time.[24]

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

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

$$dP = \underbrace{\boldsymbol{\theta}'\mathbf{z}dV}_{\text{scale}} + \underbrace{V\mathbf{z}'d\boldsymbol{\theta}}_{\text{composition}} + \underbrace{V\boldsymbol{\theta}'d\mathbf{z}}_{\text{technique}} \tag{3.1}$$

Leveraging establishment-level data, we further decompose the within-industry channel to examine the magnitude of the intensive and extensive margins: 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. Identifying establishments that survive (s), enter (n), and exit (x) between the baseline year t_0 and year t, we characterize changes in the average emission efficiency for a given industry

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

[24] 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 E.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 (3.1) and (3.2). 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.

between year t_0 and year t as,

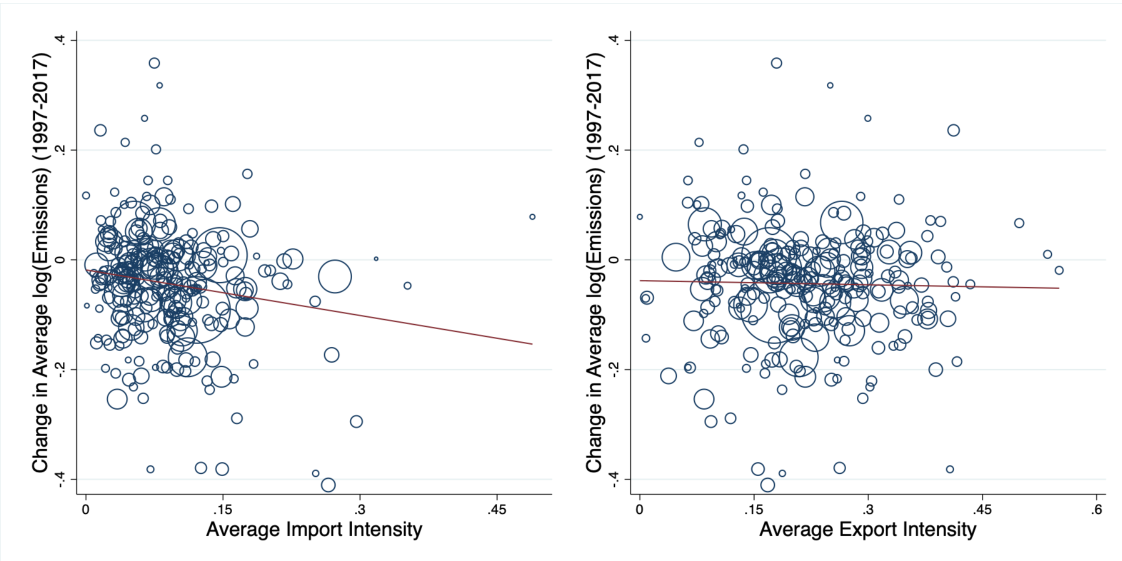
$$\begin{aligned}\Delta z = z_t - z_{t_0} &= (\theta_{s,t} z_{s,t} + \theta_{n,t} z_{n,t}) - (\theta_{s,t_0} z_{s,t_0} + \theta_{x,t_0} z_{x,t_0}) \\ &= \underbrace{z_{s,t} - z_{s,t_0}}_{\text{surviving}} + \underbrace{\theta_{n,t}(z_{n,t} - z_{s,t}) + \theta_{x,t_0}(z_{s,t_0} - z_{x,t_0})}_{\text{entry and exit}}\end{aligned} \tag{3.2}$$

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.

Figure 2 shows the decomposition results by tracking changes in total manufacturing emissions of PM_{10} relative to 1997 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), which is the sum of the scale and the *composition* factors, reveals that these two channels make limited contributions in the downward aggregate trends in PM_{10} Emissions. The inclusion of the *technique* factor in the remaining two lines accounts for the major portion of the observed decrease in PM_{10} emissions over time, a finding consistent with Levinson (2009). Moreover, in line with Holladay and LaPlue III (2021), the adjustments made by surviving establishments (intensive margin) have a more substantial impact than the entry and exit of establishments (extensive margin).

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

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



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

To understand the cleanup of manufacturing establishments in the context of globalization, we examine how the initial trade status relates to changes in PM_{10} emissions. Figure 3 plots the industry-level average growth of PM_{10} emissions against measures of industry-level import intensity (left panel) versus export intensity (right panel). Specifically, from the establishment-year-level data, we calculate for each industry (i) the growth in the average PM_{10} emissions between 1997 and 2017; (ii) the average initial within-firm employment share of importing establishments (*import intensity*); and (iii) the average initial within-firm employment share of exporting establishment (*export intensity*). We observe a stark asymmetry between import-intensive and export-intensive industries on their changes in PM_{10} emissions. 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.[25] A

[25] Appendix Figure D.3 robustly demonstrates a similar asymmetry between import-intensive and export-

possible interpretation of such asymmetry is that offshoring of manufacturing associated with importing activities led to emission declines. We revisit this discussion in Section 6.1.

4 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.[26]

The change in China's PNTR status generated heterogeneous implications across different manufacturing industries: The reduction in trade policy uncertainty had a greater impact on those who expected a larger drop in tariff rates. 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,

$$NTR\ Gap_i = Non\ NTR\ Rate_i - NTR\ Rate_i. \tag{4.1}$$

As summarized in Panel (B) of Appendix Table E.3, we observe sufficient variation in industry-level $NTR\ Gap$ in our sample.[27] Note that the differences in the $NTR\ Gap$ 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.

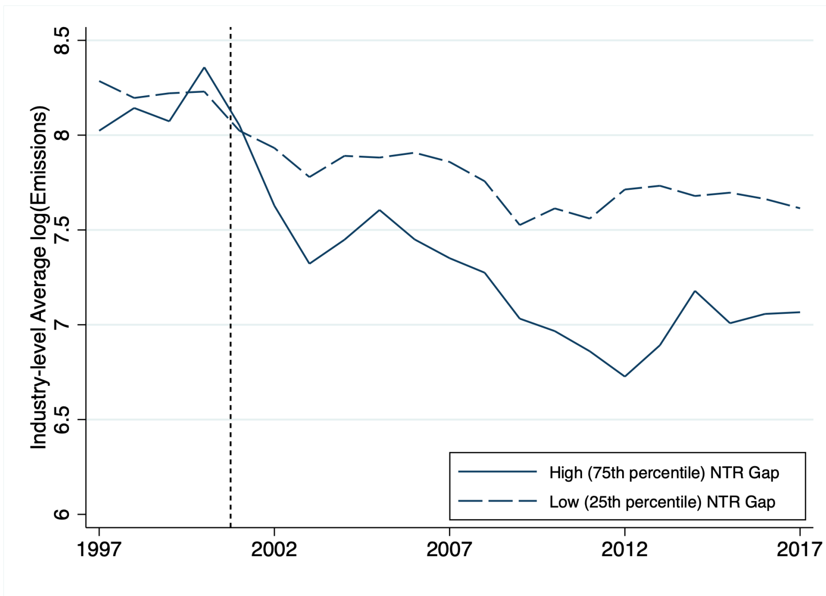
intensive industries by using industry-level import-to-value-added and export-to-value-added ratios as measures of import and export intensities.

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

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

Conceptually, the first difference compares establishments in high-$NTR\ Gap$ industries versus low-$NTR\ Gap$ industries. The second difference compares years before and after 2001 when Congress passed the bill that granted China's PNTR status and the change in US trade policy became effective. Figure 4 visualizes our identification strategy where we demonstrate trends in the log of average establishment-level PM_{10} emissions for industries in the 75$^{\text{th}}$ percentile (solid line) and the 25$^{\text{th}}$ 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$^{\text{th}}$ (dashed line) and 75$^{\text{th}}$ 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:

$$\mathrm{y}_{p,t} = \beta_0 + \beta_1 NTR\ Gap_i \times Post_t + \delta Z_i \times Post_t + \gamma X_{i,t} + \eta_p + \eta_{c,t} + \varepsilon_{p,t}, \tag{4.2}$$

where the dependent variable is the log of PM_{10} emissions from establishment p in industry i in year t.[28] The second term interacts the $NTR\ Gap_i$ with $Post_t$, an indicator for the post-PNTR

[28]Appendix Table E.6 considers emissions of sulfur dioxide (SO_2) and volatile organic compounds (VOC). We find a negative impact of PNTR on these emissions. However, we lack sufficient observations in the sample for

period (years from 2001 forward). The third term is an interaction of time-invariant industry-level characteristics (Z_i) with the post-PNTR period. As in Pierce and Schott (2016, 2020), these variables include Chinese policy variables—exposure to changes in Chinese import tariffs from 1996 to 2005 and exposure to changes in Chinese domestic production subsidies from 1998 to 2005—and initial industry characteristics, including capital intensity (capital-to-labor ratio) and skill intensity (the proportion of non-production workers in total employment) in 1997. The fourth term controls for time-varying industry characteristics ($X_{i,t}$)—the phasing out of Multi-Fiber Arrangement (MFA) quotas and the US import tariff rates.

We also include establishment fixed effects (η_p) to control for time-invariant establishment characteristics. We add county-by-year fixed effects ($\eta_{c,t}$), which is the most flexible way of controlling for any time-varying observed and unobserved common component at the county level. These fixed effects absorb any time-varying local environmental regulatory conditions and any common variation within a county-by-year pair that is due to a time-varying regional shock—e.g., local labor market shocks, regional housing market shocks. They also account for spillovers from one region to another (e.g., due to price or other general equilibrium effects) as long as such spillovers generate a common impact across establishments within a county-by-year pair. We allow for arbitrary correlations in the error term across establishments and years within the same 4-digit industry and county—thus, standard errors are two-way clustered at the industry level and the county level. The coefficient of interest is β_1, which captures the within-establishment effects of the change in trade policy on pollutant emissions.

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

$$\mathrm{y}_{p,t} = \beta_0 + \sum_{\tau} \beta_\tau \mathbb{1}\{\tau = t\} \times NTR\ Gap_i + \sum_{\tau} \delta_\tau \mathbb{1}\{\tau = t\} \times Z_i + \gamma X_{i,t} + \eta_p + \eta_{c,t} + \varepsilon_{i,t}, \quad (4.3)$$

where the second term interacts *NTR Gap* with a full set of year dummies excluding 2000. Therefore, each β_τ coefficient estimates the effect in year τ relative to 2000. The full sequence of the estimated β_τ's not only allows us to examine pre-existing trends but also to study the dynamics and persistence of the effects of the trade policy shock on PM_{10} emissions.

As discussed above, our sample period overlaps with major events that possibly confound the effects of the shock, which we address as follows. 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.[29] Second, we

SO_2, which limits the power of our estimates. Similarly, for VOC, there is insufficient variation in our shock measure, resulting in imprecise estimates.

[29]The EPA classifies US counties into attainment and nonattainment based on the ambient concentrations of

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, given its impact on the reductions of establishment-level pollutant emissions (Cherniwchan, 2017). The other is to directly control for the change in US import tariffs from Mexico (Hakobyan and McLaren, 2016); 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.

5 Main Results

5.1 Within-Establishment Emission Adjustment

Table 2 presents the estimates of Equation (4.2). 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. 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 PM_{10} emissions. 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 PM_{10} emissions within an establishment by 0.341 (= -1.191 $\times$ (0.424 - 0.138)) log points—or 34 percent.

Pre-Existing Trends and Dynamic Treatment Effects Figure 5 plots the coefficient estimates, along with their 95 confidence intervals, from the regression in Equation (4.3). The point estimates are statistically indistinguishable from zero leading up to 2000, which is in line with the parallel trends assumption, giving further validity 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 declines by -0.983 log points in 2002 (the first year PNTR became effective) and remains stable until 2005. There is an

pollutants where counties in the nonattainment category face stricter regulation (Hanna, 2010).

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

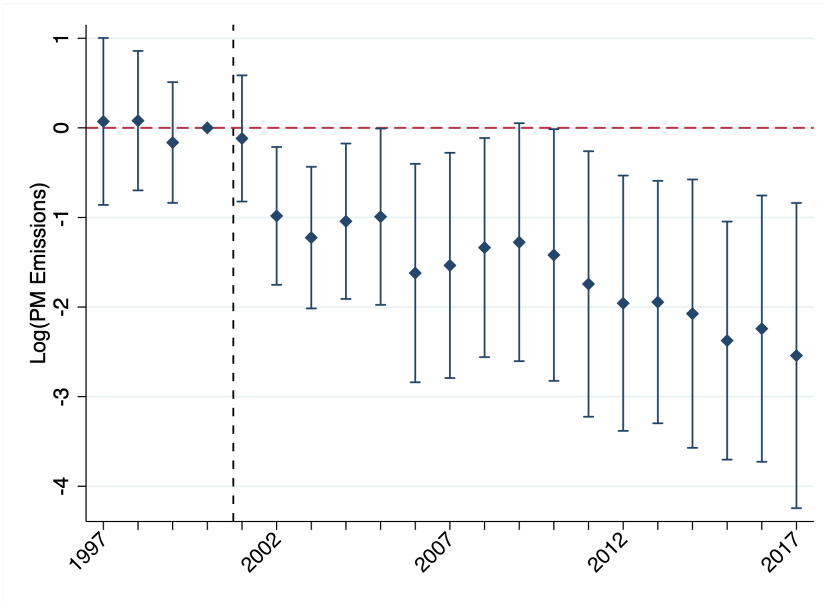
	(1)	(2)	(3)	(4)
	Log(PM Emissions)			
$\text{Post}_t \times \text{NTR Gap}_{i,99}$	-1.161***	-1.049**	-1.031**	-1.191***
	(0.428)	(0.422)	(0.425)	(0.387)
$\text{NTR}_{i,t}$			-0.019	-0.008
			(0.034)	(0.036)
$\text{MFA Exposure}_{i,t}$			-0.011	-0.009
			(0.016)	(0.016)
$\text{Post}_t \times \text{Log}(\text{NP}_{i,95}/\text{Emp}_{i,95})$				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 the 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.

overall downward trend in the estimated coefficients for the subsequent years, with an uptick from 2007 to 2009.[30] The magnitude of estimates increase over time from -1.419 log points in 2010 to -2.541 log points in 2017. Overall, the dynamic treatment effects highlight how PNTR had a prolonged effect on emission reductions in US manufacturing establishments.

[30]We cautiously interpret the upticks given how they may be attributed to the change in reporting criteria for chemicals and/or the global financial crisis. Section 5.2 includes robustness exercises related to this concern.

Figure 5: Dynamic Treatment Effects at the Establishment Level



Notes: This figure displays the estimated coefficients with their 95 percent confidence intervals for the interactions of year dummies with the NTR gap in Equation (4.3). 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.

Establishment Survival As the change in trade policy allows Chinese firms to gain greater access to US markets, which accompanies greater import competition, the less competitive domestic manufacturers are forced to exit the market (Pierce and Schott, 2016). It is then possible that our main results are driven by the extensive margin where the firm exits have caused emissions to decrease. To assess this possibility, we proceed in two ways. First, we examine the importance of the adjustments that occur at the intensive margin by repeating the baseline analysis but restricting to establishments that show positive employment throughout our sample period. Appendix Table E.7 shows the estimated coefficients are all negative with statistical significance at the one percent level. The magnitudes of the coefficients are larger, ranging from -1.57 to -1.43.

Second, we compare the evolution of establishment survival rates in industries facing large NTR gaps to those facing smaller NTR gaps,[31]

$$y_{p,t} = \beta_t NTRGap_i + \alpha V_p + \gamma X_i + \delta Z_i + \eta_c + \varepsilon_{p,t}. \tag{5.1}$$

We restrict our sample to establishments that release positive amounts of PM_{10} in 2000 (the

[31]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.

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 PNTR on establishment survival by year t. V_p captures establishment- and firm-level initial characteristics (measured in 2000), including the log of establishment employment, the log of firm employment, and firm age. X_i and Z_i capture industry-level characteristics, which are analogous to $X_{i,t}$ and Z_i, respectively, in our baseline specification in Equation (4.2). η_c is the county to which establishment p belongs in 2000. Each β_t captures one point on the empirical impulse response function describing the cumulative effects of PNTR as of each post-PNTR year.

Appendix Figure D.4 plots the coefficients on $NTRGap_i$ for each year. The survival rates initially increase in the early 2000s; show a downward trend until the year 2008; rebound during the period 2008-2010; and then slightly decline thereafter. However, all the coefficients are statistically insignificant, which suggests that PNTR did not induce US manufacturers that initially report positive amounts of PM_{10} to exit the market. Hence, establishment exits are not the primary factor behind the reduction in pollution emissions in US manufacturing.[32] Note that this result does not necessarily mean that the PNTR did not induce US manufacturers to leave the market *in general*. In Appendix C, we show that manufacturing establishments that generate positive amounts of emissions are fundamentally different from those with zero or negligible emissions, and our result that attributes emission abatement to surviving establishments is not a spurious result driven by the restriction of sample induced by TRI-reporting criteria.

Emission Intensity Adjustment The emission reduction effects could be explained simply by a scale effect—reduction in production—within an establishment. To check for this possibility, we repeat the baseline analysis using establishment-level emission intensities (the ratio of PM_{10} emissions to sales) as the dependent variable. Appendix Table E.8 reports the estimation results.[33] 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 we obtain in Table 2, which suggests a limited role

[32]In Appendix Table E.15, we accommodate observations with zero reported emission using PPML regressions and show that the estimated impact of PNTR on PM_{10} emissions are more or less stable across (i) accommodating establishment entry and exit margins, (ii) restricting the analysis to surviving establishments but allowing zero emission, and (iii) restricting the analysis to observations with positive emissions. This further shows that our results are not particularly driven by the extensive margin of establishment exits.

[33]The results are robust to defining emission intensities using the ratio of PM_{10} emissions to employment instead of sales (Appendix Table E.16, Column (4)). Also, we find robust results when we consider emission intensities at the firm-year level (Appendix Table E.16, Columns (2)-(3)).

of the within-establishment scale effect in reducing emissions.[34] 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. Appendix Figure D.9 plots the dynamic treatment effects using emission intensity as the dependent variable. Reassuringly, we do not detect any differential pretrends. As in Figure 5, we find lingering effects of PNTR on emission intensity, but without any upticks during the period between 2007 and 2009: The emission intensity declines substantially in 2002 and thereafter exhibits a smooth, downward trend until 2017.[35]

Non-Disposal Activities As discussed in Section 2, 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 E.19 (off-site non-disposal) and E.20 (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 suggest that establishments are responding by potentially reducing waste production at the source. We revisit this discussion in Section 6 where we study mechanisms in detail.

5.2 Robustness Checks

In this section, we conduct several robustness tests to corroborate our main difference-in-differences results: (i) alternative sample periods; (ii) controlling for NAFTA; (iii) dropping outliers; and (iv) weighted regressions and toxicity-weighted emissions. At the end of this

[34]Appendix Figure D.10 is generally consistent with this pattern such that sales within establishments increased after PNTR but they are almost statistically indistinguishable from zero. While most of the coefficients are statistically insignificant, sales within surviving establishments that release positive amounts of emissions can actually increase in response to PNTR, which can be due to capital deepening of continuing US firms allowing them to expand their production capacity and better compete with Chinese firms, as shown in Pierce and Schott (2016). Furthermore, Appendix C demonstrates how those releasing emissions are fundamentally different from typical manufacturing establishments, and thus, respond differently in terms of sales (at the intensive margin).

[35]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 D.9.

section, we briefly list additional robustness exercises that we conducted in Appendix E.

Alternative Sample Periods Here, we examine our baseline specification using alternative sample periods. First, we add the years 1995 and 1996, which were initially excluded to avoid the potentially confounding effects of NAFTA—which came into force on January 1, 1994. Column (1) of Appendix Table E.9 shows the estimated results, which are quantitatively similar to our baseline. Next, we check whether our results are robust to excluding the years 2007, 2008, and 2009 from our sample. As discussed above, there was a major change in the TRI reporting criteria in 2007, which was revoked in 2009. Moreover, these years include the Great Recession, which can possibly accompany unobserved demand or supply shocks. It is particularly concerning if these shocks are correlated with our shock and are not adequately addressed through the set of control variables and the county-year fixed effects. Column (2) of Appendix Table E.9 repeats the baseline analysis dropping years from 2007 to 2017; Column (3) drops from 2007 to 2017 and adds 1995 and 1996; Column (4) drops from 2007 to 2009. We find robust results.[36]

Controlling for NAFTA A more direct way to address concerns related to the lagged responses to NAFTA is to control for changes in US tariffs on imports from Mexico in our baseline regression. In particular, we include an interaction term of industry-level changes in US tariffs on imports from Mexico from 1990 to 2000 and the post-PNTR dummy variable.[37] Appendix Table E.10 presents the estimation results. Column (1) of Appendix Table E.10 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 D.8 plots the dynamic treatment effects after controlling for the NAFTA tariff changes. Again, we obtain quantitatively similar effects to our main results.

[36]Appendix Figures D.5 -D.7 present corresponding dynamic treatment effects for the sample periods (i) from 1995 to 2017, (ii) from 1995 to 2006, and (iii) from 1997 to 2006, respectively. Once again, the results confirm the robustness of the baseline results in Figure 5. In addition, the observed emission reductions are noticeable from 2002 onward, the timing of which aligns with China joining the WTO on December 11, 2001, and with PNTR becoming effective on January 1, 2002.

[37]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.

Dropping Outliers As discussed in Section 2.3, the distribution of PM_{10} emissions is highly skewed with a small number of establishments generating extreme amounts of 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, Jarmin, and Miranda 2013). To ensure that these extreme observations are not driving our main results, we run our baseline specification without these outliers. Specifically, Columns (1)-(3) of Appendix Table E.11 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 robust to dropping these exercises.

Weighted Regressions and Toxicity-Weighted Emissions We show that our results remain robust to alternative weighting schemes. Column (1) of Appendix Table E.12 considers a weighted regression using the establishment's initial PM_{10} emissions as weights. Column (2) weights each observation by the establishment's initial employment. Column (3) uses the log of toxicity-weighted PM_{10} emissions as the dependent variable using initial emissions as weights.[38]

Additional Analyses We show the robustness of our main results with respect to (i) controlling for the impact of PNTR through input-output linkages—i.e., upstream- and downstream-specific NTR gaps (Table E.13), upstream-specific time trends (Table E.14); (ii) accommodating observations with zero reported emission using PPML regression (Table E.15); and (iii) considering firm-year-level regressions (Table E.16).

5.3 Heterogeneous Adjustments Across Establishments

We extend Equation (4.2) 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.[39] Finally, we consider industry-level upstreamness

[38]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.

[39]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 (O_3), sulfur dioxide

(constructed using Input-Output tables on US production linkages as in Antras et al. (2012)). Following Burchardi, Chaney, and Hassan (2019), we use a binary indicator that is equal to one if the upstreamness index is larger than 2. Table 3 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.[40]

There are four notable results in Table 3, which, for visibility, we place in the first four columns (i.e., Columns (1)-(4) and (10)). First, the estimated coefficient for import intensity (Import $\text{Intensity}_{f,97}$) is negative and statistically significant, while that of export intensity (Export $\text{Intensity}_{f,97}$) is negative yet lacks statistical precision. Consistent with Fact 3 of Section 3, establishments of firms that are initially more engaged in import activities have substantially more reduced emissions than others.[41] 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.[42]

Second, the estimated coefficient for the initial nonattainment status of the county in which each establishment is located ($\text{Nonattainment}_{c,95-97}$) 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.

(SO_2), 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.

[40]We use the log of PM_{10} emissions as the dependent variable in Table 3; however, we also find consistent results using the log of pollution emission intensity. See Appendix Table E.17.

[41]In this exercise, we condition on firms being importers (i.e., Import $\text{Intensity}_{f,97}>0$) to capture the intensive margin of intensity. In Appendix Table E.18, we consider the unconditional import intensity that includes non-importers. We obtain negative coefficients, but the estimate is less precise. The importance of intensive margin is consistent with Martin, Mejean, and Parenti (2021), who find that products with higher relationship stickiness—in particular, industrial (specialty) chemical and pharmaceutical products—have larger intrafirm trade and exhibit stronger trade dynamics in response to uncertainty shocks. In our data, we find that chemicals and allied products (SIC 2-digit 28), which include industrial chemical and pharmaceutical products, are among the industries with the highest emission share in the US (Figure D.2) and exhibit a strong response to PNTR (Table E.5).

[42]We find qualitatively similar results using the log amount of PM_{10} processed through off-site non-disposal methods as the dependent variable (Column (11) of Appendix Table E.19). While we do not find any significance in the main effects, the results imply potentially important complementarities between an establishment's access to foreign sourcing networks and off-site non-disposal activities. Appendix Table E.20 presents the results for on-site non-disposal, where we do not find analogous patterns.

Table 3: Heterogeneous Treatment Effects:
PNTR and Establishment-level Pollution Emissions, 1997 - 2017: Log(PM Emissions)

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

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

In fact, Hanna (2010) finds that stricter environmental regulations in the US, as indicated by the nonattainment county status of establishment locations, induce US-based multinationals to increase their FDI activities. Consistent with this finding, a possible interpretation is that establishments in nonattainment counties, seeking to lower abatement costs, capitalize on the PNTR-induced opportunities for FDI, including offshoring high-polluting tasks abroad.

Third, the estimated coefficient for upstream industries ($\text{Upstream}_{i,97}$) is negative and statistically significant. In response to PNTR, establishments operating in industries that produce intermediate goods reduce emissions by a greater amount than those operating in more downstream industries. This result is consistent with the notion that offshoring activities are more likely to occur in intermediate goods industries.

Finally, the estimated coefficient for multi-sector establishments ($\text{Num. 4-digit Sectors}_{f,97}$) 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 possibly more resilient to shocks through flexible reallocation of resources across establishments (Hyun, Park, and Smirnyagin, 2022). It is possible that such flexibility allows these multi-sector firms to easily offshore dirty production and reallocate their resources toward cleaner production, resulting in reduced emissions.

6 Mechanisms

Motivated by the suggestive evidence in support of the offshoring mechanism in the heterogeneous treatment effect analyses, we directly assess the importance of the offshoring channel—global sourcing and FDI—in explaining our main findings on the PNTR-induced reductions in the PM_{10} emissions within US manufacturing. We further examine whether consistent patterns are found when studying the PNTR-induced US imports of high-polluting products from China. Lastly, we discuss alternative explanations, such as import competition and clean technology adoption, and implications for pollution emissions in China as a result of PNTR.

6.1 Pollution Offshoring Hypothesis

Global Sourcing and FDI Activities Offshoring occurs when parts of the multi-stage production process are performed abroad. Such offshoring activities involve sourcing foreign intermediate inputs (Hummels, Ishii, and Yi, 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 (Garetto, 2013; Tintelnot, 2017). In the data, however, it is challenging to construct a single measure that comprehensively captures these offshoring activities (Monarch, Park, and Sivadasan, 2017), let alone relocation of dirty tasks.

Our analysis relies on two separate measures of offshoring activities, which leverage detailed data from NETS and WRDS Company Subsidiary data, respectively.[43] Specifically, we first use time-varying establishment-level importing status from the NETS database. Given our focus on manufacturing establishments, we consider these sourcing activities to be more pertinent to offshoring than purchases intended for direct resales to domestic consumers. Next, we use WRDS Company Subsidiary data linked to our main dataset and count the number of foreign subsidiaries in China (and other countries) to measure US multinationals' FDI activities at the establishment-year level. Using these two measures—importing status and FDI—as dependent variables, we estimate Equation (4.2).

In each exercise, we use the triple difference-in-differences framework to test whether such offshoring and FDI activities are more pronounced for establishments associated with high-polluting tasks—measured as whether establishments were initially located in nonattainment counties or whether establishments show higher initial pollution intensity. Note that establishments in counties with a nonattainment designation are likely heavy emitters during the initial period, as this designation is granted to counties with air pollution concentrations that exceed federal standards.

Note that our approach allows us to capture whether establishment- or firm-level offshoring and FDI activities respond to the reduction in industry-level *NTR Gap* (i.e., trade policy uncertainty). If this is the case, it suggests that offshoring occurs within the same industry category and possibly allows US manufacturers to concentrate their in-house production activities toward low-polluting tasks or products. That is, our analysis captures establishment-level responses to PNTR through (i) fragmenting the production process into multiple stages and sourcing dirty intermediate tasks within the same industry—previously all carried out in-house—from abroad; and (ii) establishing new foreign subsidiaries that conduct high-polluting tasks in China in the same industry.[44]

Table 4 reports the estimates for global sourcing activities. We begin by focusing on firms with at least one foreign sourcing network (Import $\text{Intensity}_{f,97} > 0$) because the emission reduction effects were most pronounced at the intensive margin of importing activities in Table 3.[45] Column (1) of Table 4 indicates that non-importing establishments that belong to

[43]While the coverage of the WRDS Company Subsidiary Data is confined to publicly listed companies, it is the best available dataset that covers two decades of the sample period and enables us to directly test the offshoring mechanism via FDI activities. See Section 2.1 for more details on WRDS Company Subsidiary Data.

[44]A complementary approach in the literature is to use the industry-level input-output tables to proxy plant-level production and sourcing structure. We conduct robustness checks incorporating industry-level upstream- and downstream-specific NTR gap measures and discuss the result. See Section 6.2 for further details.

[45]As noted in footnote 41, the importance of intensive margin of the relationship is broadly consistent with

Table 4: PNTR and Import Status, 1997 - 2017

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

Notes. This table investigates the average and heterogeneous treatment effects of the conferral of PNTR to China on establishment-level import status. The dependent variable, Import, is a dummy variable that equals 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., $\text{Import Intensity}_{f,97} > 0$). 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.

a firm with foreign sourcing networks began to source from abroad after PNTR. Conversely, Column (1) of Appendix Table E.21 shows that PNTR had no such effect for establishments that did not initially belong to an importing firm (i.e., $\text{Import Intensity}_{f,97} = 0$). Both results underscore the importance of the intensive margin adjustments where establishments with existing foreign networks play a major role in global sourcing activities following PNTR.[46] Columns (2) and (3) of Table 4 further show that establishments that are most likely to be involved in high-polluting tasks engage more in importing activities than other establishments after PNTR. The results collectively suggest that offshoring dirty tasks is an important channel through which US manufacturers reduce emissions.

Table 5 reports the estimates for FDI activities. 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),

the findings in Martin, Mejean, and Parenti (2021).

[46] Similarly, we examine whether exporting activities respond to PNTR. Columns (2) and (3) of Appendix Table E.21 reveal that PNTR does not cause new exporting activities at the establishment level.

Table 5: PNTR and FDI into China vs. Other Countries, 1997 - 2017

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

we measure the log of firm f's number of subsidiaries in China (or other countries) in year t, thereby capturing the intensive margin. We find that the PNTR induces US manufacturing establishments to set up *more* subsidiaries, mainly at the intensive margin.[47] 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.

Table 6 further shows that establishments that are most likely to engage in high-polluting tasks increase their subsidiaries in China more than other establishments, an effect that mainly operates through the intensive margin (Columns (2) and (4)), not the extensive margin. That is, the number of subsidiaries in China increase more for establishments that initially faced stricter environmental regulation and for those that had higher initial pollution emission intensity.

Dirty Product Imports from China to US Next, we test whether US manufacturers, in fact, have increased imports of dirty products from China. If US manufacturers shifted high-polluting activities to China after PNTR, and such a shift was driven by the offshoring

[47] Due to the reduced number of observations in Column (4), we lose statistical power; nevertheless, statistically significant at the 21 percent level.

mechanism, we would anticipate an increase in dirty product imports from China relative to other countries. To test this hypothesis, we use HS 10-digit product-by-year-level data from the UN Comtrade database and examine whether the share of US imports from China increase to a greater extent for products categorized under high-polluting industries using the same definition of dirtiness as the previous exercises. Table 7 shows the result. Column (1) confirms that, following PNTR, the share of US imports from China increased. Column (2) shows the heterogeneity across products in terms of dirtiness: the effects are more pronounced for products that are produced by high-polluting industries. Column (3), albeit a p-value of 0.258, shows that

Table 6: 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 5, 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.

the increase in the share of US imports from China is more noticeable in upstream industries.

Table 7: Dirty Industries and Heterogeneity in Product-level Response of US Import Share from China, 1997 - 2017

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

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

6.2 Discussions

Discussion 1: Offshoring vs. Import Competition and the Cleanup of US Manufacturing The set of findings in Section 6.1 collectively provides direct evidence supporting offshoring as an important mechanism through which US manufacturers shifted high-polluting activities to China after PNTR, resulting in a reduction in domestic emissions. These results are consistent with the *pollution offshoring hypothesis*, which posits that progress toward trade liberalization induces firms in developed countries to avoid stringent environmental regulations by locating production in countries with laxer environmental standards. Despite the extensive evidence supporting the offshoring mechanism, one may still argue that our results are potentially confounded by the import competition channel at the level of the final goods market. While we do not claim that offshoring is the only contributor driving our results, we emphasize the ample evidence we find in favor of offshoring, but not import competition, as we illustrate below.

To reiterate, PNTR is related to a reduction in trade policy uncertainty, not actual tariffs. Hence, a conventional tariff reduction channel—cheaper Chinese imports replacing US products—is not directly applicable to our main analysis while we do control for the NTR tariff rates in our baseline specification. Our findings in Figure D.4, Table E.8, and Figure D.10 further show that the observed emission reductions are not driven by these competitive forces leading to downsizing or exits. The offshoring channel, on the other hand, is strongly supported by both direct and indirect evidence shown in previous sections of the paper. Specifically, emission reductions are more pronounced for establishments in upstream industries (Table 3). Furthermore, establishments with existing foreign sourcing networks, including those in China, experience more substantial emission reductions along with their increased sourcing and foreign direct investment (FDI) activities. Furthermore, we observe a weak, but statistically insignificant, increase in sales after PNTR among establishments with positive emissions (Figure D.10). This finding is again inconsistent with the competition channel but instead supports the productivity effect of offshoring—the cost-saving nature of offshoring allows firms to boost their productivity and increase sales (Grossman and Rossi-Hansberg, 2008).

From a measurement perspective, a complementary approach to capture the offshoring channel is to use *NTR Gap* reflecting input-output linkages. Note that constructing establishment-specific measures of input uncertainties is infeasible in our study as plant-level data on detailed input purchases are not accessible. Thus, as done in many other studies, we rely on the industry-level input-output tables to proxy plant-level production and sourcing structure. Following Pierce and Schott (2016), we construct industry-level upstream- and downstream-specific *NTR Gap* measures, respectively, and examine our baseline specification including both measures. The first row of Appendix Table E.13 shows the main estimate of our interest, which remains negative and statistically significant. Consistent with the "within-industry" offshoring discussed in Section 6.1, establishments in our sample do not respond to changes in input uncertainties in other industries in adjusting their emissions. Our finding is broadly consistent with the "produced-goods imports" channel articulated in Bernard et al. (2020), which shows that offshoring primarily involves importing products in the same detailed six-digit HS category of goods that they continue to produce domestically.[48]

[48] Note that this is entirely compatible with our finding in Section 5.3 that establishments operating in upstream sectors show stronger responses in pollution abatement: Establishments, which produce intermediate goods, can import the same category of intermediate goods during the offshoring process and supply them to other establishments (owned by different firms or own parent firms).

Finally, we discuss recent studies that document important evidence on the firm-level adjustments in response to import shocks, which is also informative of how the cleanup of US manufacturing has been possibly achieved via offshoring. For example, Bernard et al. (2020) find that, besides "produced-good imports," offshoring allows firms to reallocate their tasks toward producing high-quality varieties within the same product category. Similarly, Hombert and Matray (2018) find that the China trade shock induced US firms, especially R&D-intensive firms, to increase product differentiation, and climb up the quality ladder. As PNTR facilitates offshoring (or FDI) activities to China, multi-stage (or multi-product) establishments, mostly producing differentiated steel or chemical products (Figure D.2), may have differentiated their products (or production stages) by substituting high-polluting activities with low-polluting ones within establishments. Given that we find stronger responses for establishments located in upstream industries and owned by multisector firms (Table 3), it is possible that these establishments are intermediate goods producers that shifted their tasks from dirty to clean activities and imported dirty products from China.

Discussion 2: Pollution Offshoring and Pollution Emissions in China What are the implications of our findings on the overall pollution level in China? While our findings support the pollution offshoring hypothesis, indicating that US manufacturers shift high-polluting tasks to China after PNTR, it is important to note that this does not necessarily mean an increase in the overall level of pollution in China. First, it is possible that high-polluting tasks offshored by the US to China may be less pollution-intensive compared to tasks performed by local Chinese firms. In this case, the overall pollution level would depend on whether the presence of US subsidiaries in China leads to the displacement of local Chinese firms that have higher pollution intensity.[49] Second, 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, Sheng, and Tan (2022) find that Chinese exporters are significantly less

[49] This idea is reminiscent of Feenstra and Hanson (1996) where outsourcing by Northern multinationals to the South leads to an increase in the South's capital stock relative to the North. This can potentially increase the relative wage of skilled labor and average skill intensity of tasks in both North and South simultaneously. In their model, the activities outsourced to the South rely more on unskilled labor from the North's perspective, but rely more on skilled labor from the South's perspective. One can apply the same mechanism in the context of offshoring high-polluting tasks from North to South. In addition, Davis and Kahn (2010) find that the NAFTA-led exports of used cars from the US to Mexico resulted in a decrease in vehicle emissions per mile in both countries. Although the exported vehicles were dirtier than the average US vehicles, they were still cleaner than Mexican vehicles.

emission-intensive compared to non-exporters. Our results suggest that PNTR—followed by the offshoring of high-polluting tasks to China—resulted in increased reliance on imports from China, especially for products that are produced by dirty industries according to US standards (Table 7).

Discussion 3: PNTR and Clean Technology Adoption We now examine the importance of the technology channel in understanding the PNTR-led emission reductions. Levinson (2009) finds that the majority of the pollution emission reductions in the US from 1987 to 2001 were attributable to technology adoption. If US manufacturers have adopted clean technologies in response to PNTR, the observed decline would reflect trade-induced advances in production or abatement processes rather than offshoring activities.

To test for this possibility, we estimate Equation (4.2) 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 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 E.22 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 associated with these P2 activities.[50] We find that neither the extensive nor intensive margin of P2 activities respond to PNTR.

The null results provide further support for the pollution offshoring hypothesis, indicating that offshoring, rather than clean technology adoption, is the primary response to PNTR. These findings may also be interpreted as offshoring acting as a substitute for clean-tech innovation. Bena and Simintzi (2022) show that a policy change, which allows US firms to produce in China at lower costs, such as PNTR, deters process innovation aimed at reducing production costs because sourcing labor across borders becomes cheaper. Consequently, instead of costly innovation to reduce pollution emissions, US manufacturers can opt to offshore dirty tasks (especially to China) after PNTR. Nevertheless, it is important to note that this result does not necessarily contradict the existing literature that highlights the role of technology in explaining the cleaning-up of manufacturing (i.e., the *technique effect* in Section 3). In addition to the trade-induced channel, it is still possible that a broader trend of nationwide green technology adoption contributes significantly to the reduction of emissions in US manufacturing.

[50] Appendix Table E.23 shows similar results using overall P2-related activity as the dependent variable.

7 Conclusion

Using the conferral of PNTR status to China as a quasi-natural experiment, we investigate the long-run environmental impacts of trade liberalization and provide support for the pollution offshoring hypothesis in US manufacturing. Our finding suggests that reduced trade barriers, particularly trade policy uncertainties, drive US manufacturers to engage in offshoring activities. The extent to which differential environmental regulations between developed and developing countries generate pollution havens depends on additional economic factors, beyond regulatory differences alone. Thus, our work highlights the importance of nontrivial interactions among trade policy uncertainty, environmental regulations, and offshoring in teasing out the pollution offshoring hypothesis.

While our work exploits a specific trade liberalization episode between the US and China, it also provides broader implications for jointly explaining two salient global patterns since the late 20th century: (i) the divergent paths of emissions between developed and developing countries; and (ii) offshoring production tasks from developed to developing countries. Our research also carries important implications for trade policy, highlighting the need to carefully consider the environmental impact in light of recent empirical studies documenting substantial detrimental effects of pollution on both health and productivity (Chang et al., 2016; Deryugina et al., 2019). In this regard, our paper contributes to the existing literature by taking a significant step forward in providing novel insights into understanding the various effects of the China trade shock and, more broadly, trade liberalization.

Lastly, our findings on PNTR-led offshoring pollution shed light on the implications of broader implementation of policies aimed at reducing the gap in environmental regulation between developed and developing countries such as the Carbon Border Adjustment Mechanism (CBAM), a policy tool that imposes tariffs on carbon-intensive products across borders. On the one hand, such coordinated policy can deter offshoring of dirty tasks from developed to developing countries, which helps mitigate offshoring-led exacerbation of environmental outcomes through carbon leakages. On the other hand, considering the positive spillovers of multinationals on transfers of technology, knowledge, and institutions, which has been recognized as an important source of growth in host countries, the introduction of policies such as CBAM can discourage offshoring activities altogether, which impedes important positive spillovers that benefit both ends through global integration of production activities.

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Appendix

FOR ONLINE PUBLICATION

The Cleanup of US Manufacturing through Pollution Offshoring

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Appendix A Institutional Background on TRI 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.[1] Facilities that are noncompliant are subject

[1]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

to further investigation and possible enforcement actions by the EPA.[2] Furthermore, TRI has several institutional features to optimize and maintain the quality of data, for example, through "built-in data quality alerts," "data quality call processes (ad hoc data quality calls)," and enforcement actions.[3] 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.

"full-time employees" includes "all persons employed by a facility regardless of function (e.g., operational staff, administrative staff, contractors, etc.)."

[2]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]See https://www.epa.gov/toxics-release-inventory-tri-program/tri-data-quality for further details.

Appendix B Sample Construction: Details

The matching of the TRI-NETS data between 1987 and 2020 results in 2,809,810 observations with chemical-establishment-year 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.[4]

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 the participation of Native Americans in 2012. To maintain consistency on this end, we keep establishments that are not located in *Indian country*.[5]

Applying the above process results in 636,985 establishment-year level observations with 27,695 unique establishments for the period 1995-2017. 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. By additionally restricting the sample to manufacturing establishments yields 495,765 establishment-year level observations (21,555 unique establishments), and by keeping those with non-missing NTR gap results in 432,860 establishment-year level observations (18,820 unique establishments). Then, by further restricting the sample to establishments with at least one year of positive PM emissions, we have 191,751 establishment-year level observations with 8,337 unique establishments for the period 1995-2017.

In our baseline analysis, we also exclude a few years after the North American Free Trade

[4]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

[5]Appendix Table E.1 provides further details related to these policy changes.

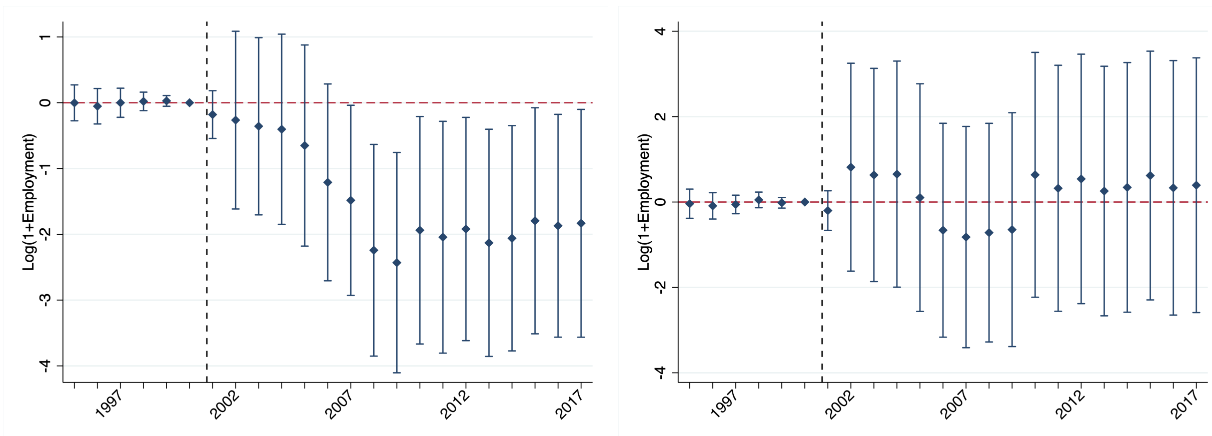
Agreement (NAFTA) agreement (1994), given its impact on the reductions of establishment-level pollutant emissions (Cherniwchan, 2017). Hence, we restrict our sample period to years between 1997 and 2017, which yields 175,077 establishment-year level observations (8,337 unique establishments). Note that including a few years (i.e., 1997, 1998, 1999, and 2000) before the US trade policy change in 2001 allows us to examine the pre-existing trends in our analysis. We address any remaining concerns related to the lagged responses of NAFTA by directly controlling for changes in the US tariffs on Mexican imports following Hakobyan and McLaren (2016).

Finally, our baseline regression restricts analyses to observations with positive PM emissions.[6] After dropping zero emission observations and singleton observations, we arrive at the final sample: 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.

[6]We accommodate observations with zero reported emission using PPML regressions in Table E.15 and show that the results are robust. The number of observations in Table E.15 is lower than 175,077 because singleton observations are dropped during the estimation process.

Appendix C PNTR and Employment Responses

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



Notes: These figures display the estimated difference-in-differences (DID) coefficients with their 95 percent confidence intervals, where we use log of one plus employment as the dependent variable. The sample consists of establishment-year-level observations from NETS-TRI matched data, where each establishment has at least one year of positive emissions reported to TRI. In the left panel, we restrict the sample to establishments that had ten or more workers during the initial period (1995-2000). In the right panel, we further restrict the sample to those that had positive initial emissions. All other specifications are identical to those in Equation (4.3).

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

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

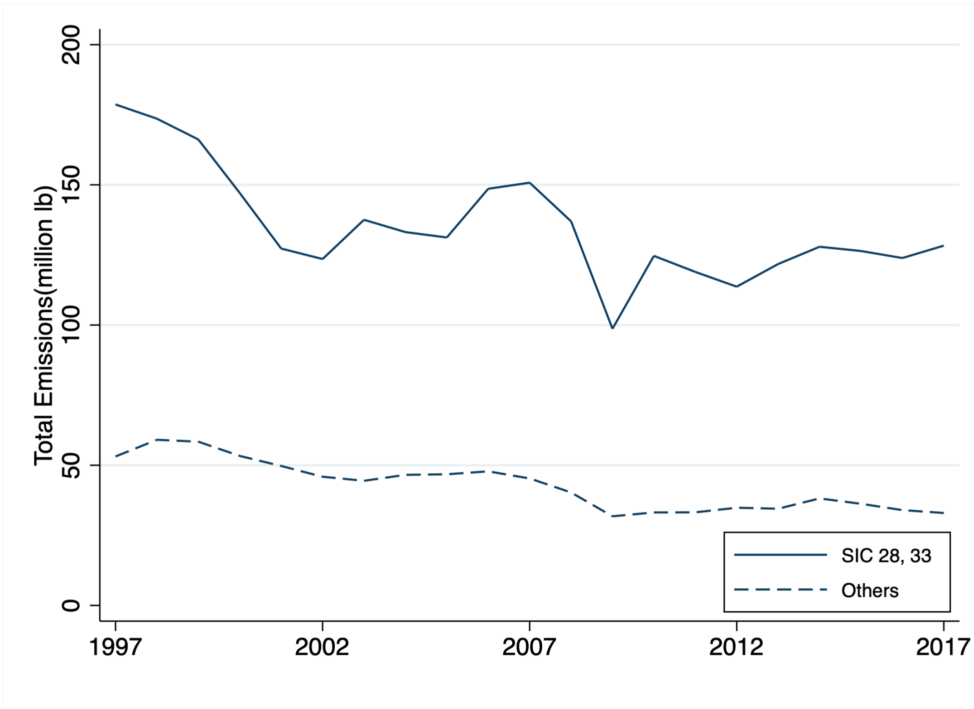
The result exhibits a clear contrast with the right panel of Figure C.1, where we restrict the sample to establishments that had positive initial emissions. Once we restrict the sample to those with positive initial emissions, we find an insignificant response of employment. This is also broadly consistent with Figure D.10, where we observe a mild—but insignificant—increase of sales following the PNTR when we focus on establishments with positive emissions.[7]

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

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

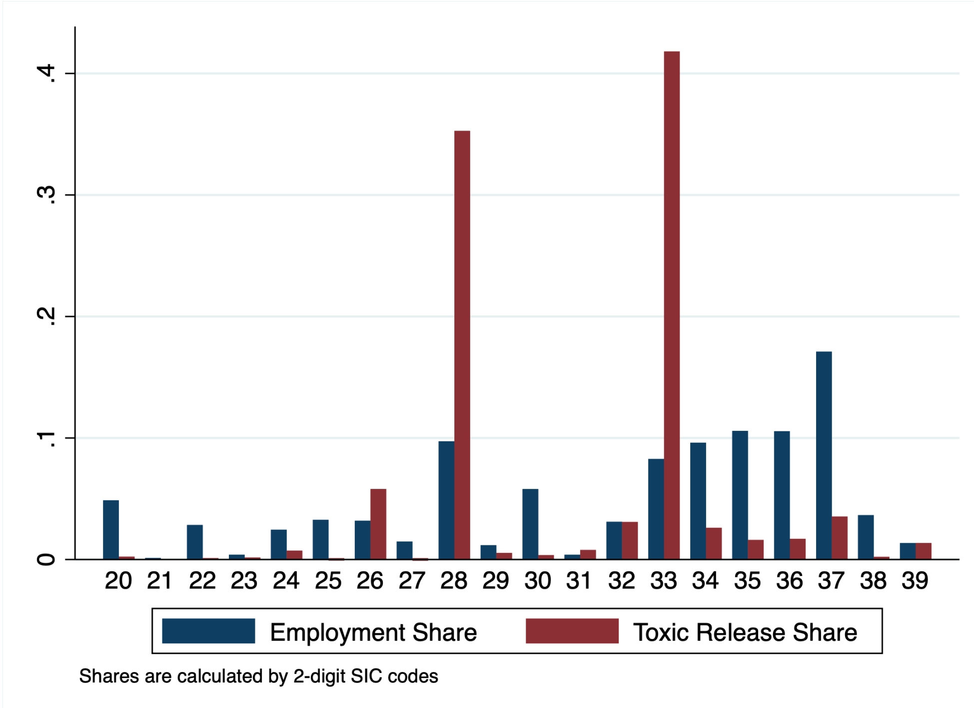
Appendix D Additional Figures

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



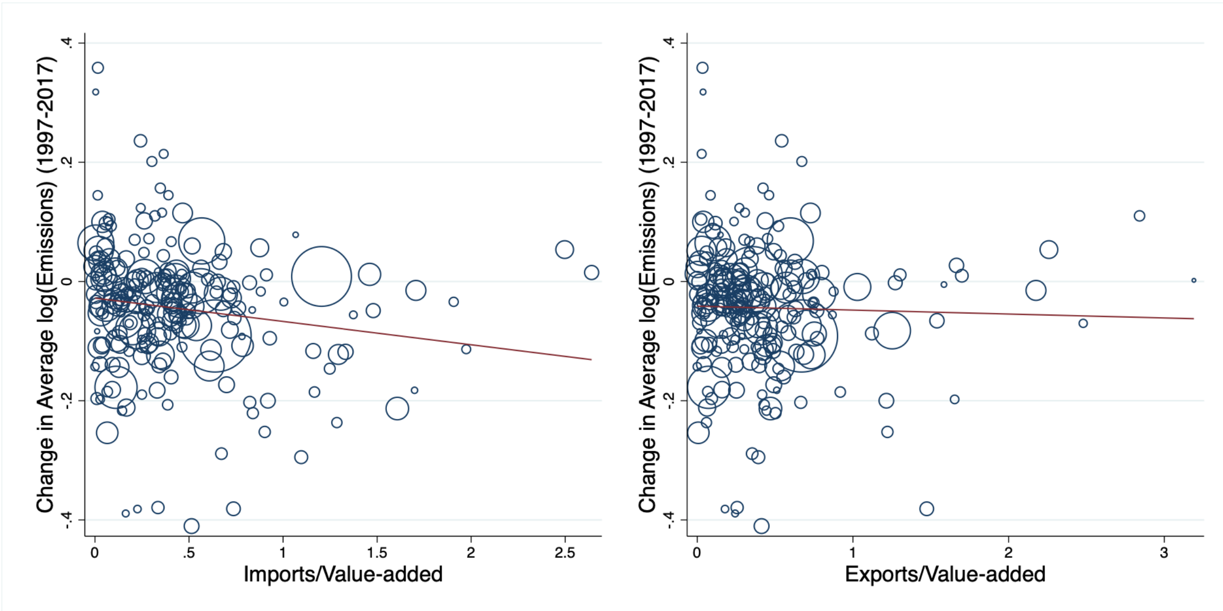
Notes: This figure displays PM_{10} emissions trends for (i) 2-digit-SIC 28 and 33 and (ii) all other industries for 1997-2017.

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



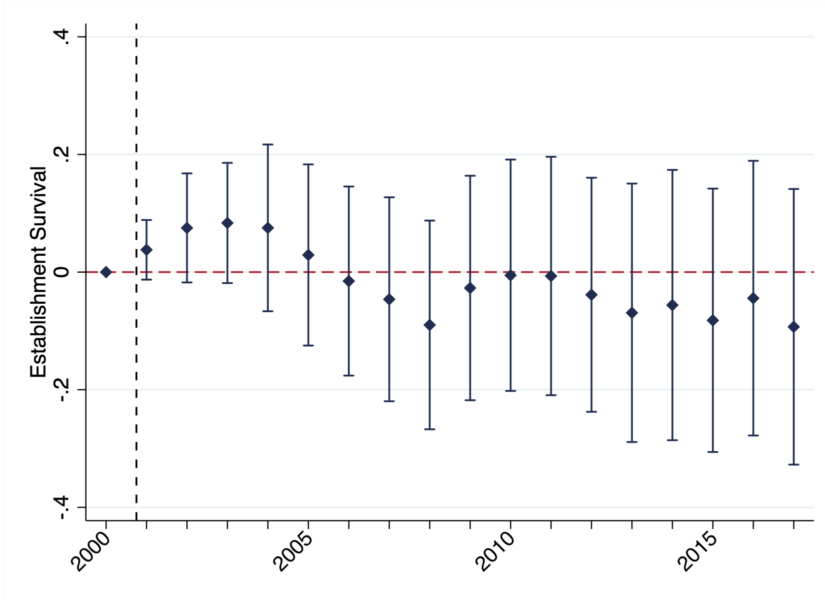
Notes: This figure displays employment (navy bars) and PM_{10} emissions (red bars) shares in 1997 by 2-digit-SIC industry. SIC 2-digit 28 indicates "Chemicals and Allied Products"; SIC 2-digit 33 indicates "Primary Metal Industries".

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



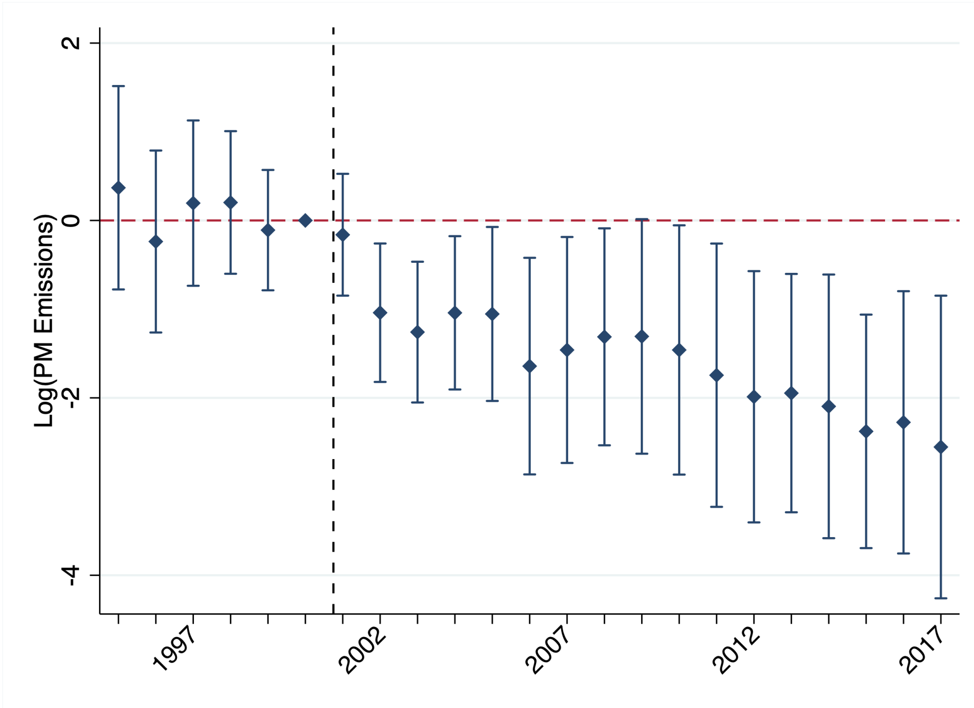
Notes: The graph on the left (right) illustrates the correlations between the industry-level averages of changes in the within-establishment log(emissions) of PM_{10} from 1997 to 2017 and the industry-level import (export) intensity constructed using the value of imports (exports) relative to value-added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

Figure D.4: PNTR and Establishment Survival, 2001 - 2017



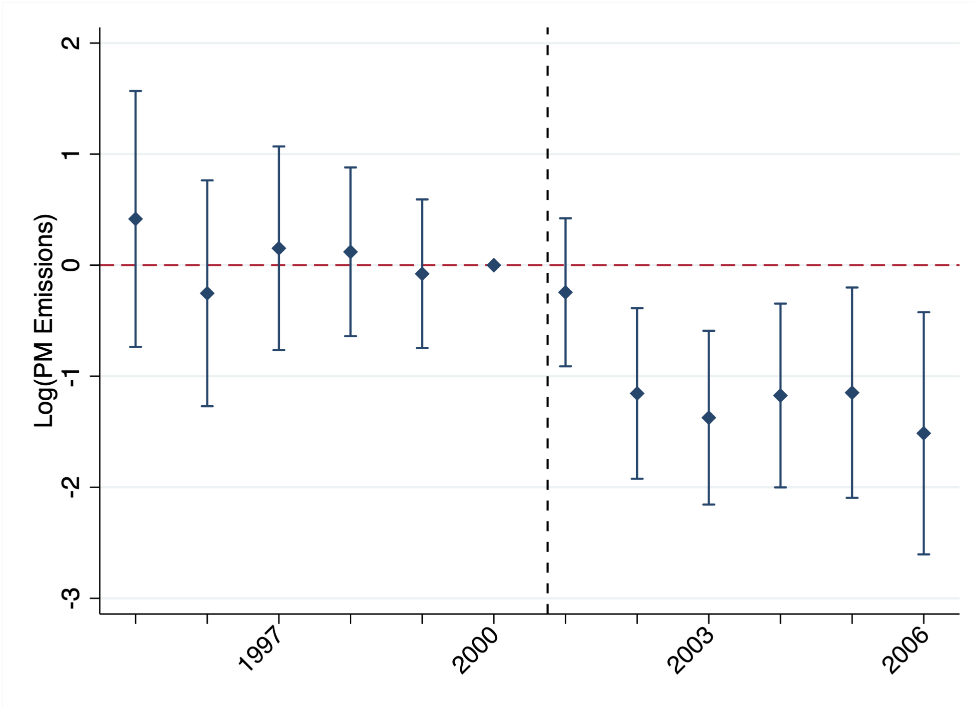
Notes: This figure shows the cumulative effect of the imposition of PNTR on establishment survivals, conditional on positive PM_{10} Emissions in 2000. Each point reflects an individual regression coefficient, β_t, following Equation (5.1). The estimated coefficients are displayed with their 95 percent 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 D.5: Robustness: Dynamic Treatment Effects at the Establishment Level, 1995-2017



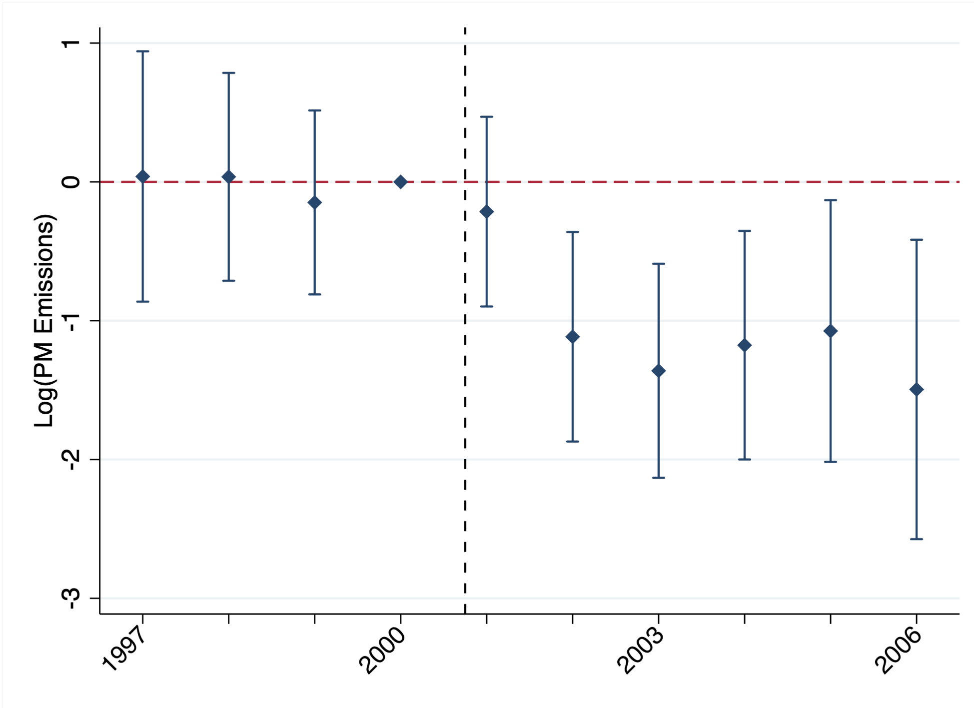
Notes: This figure displays the estimated difference-in-differences (DID) coefficients with their 95 percent confidence intervals, where we consider an extended sample period from 1995 to 2017. All other specifications are identical to those in Equation (4.3).

Figure D.6: Robustness: Dynamic Treatment Effects at the Establishment Level, 1995-2006



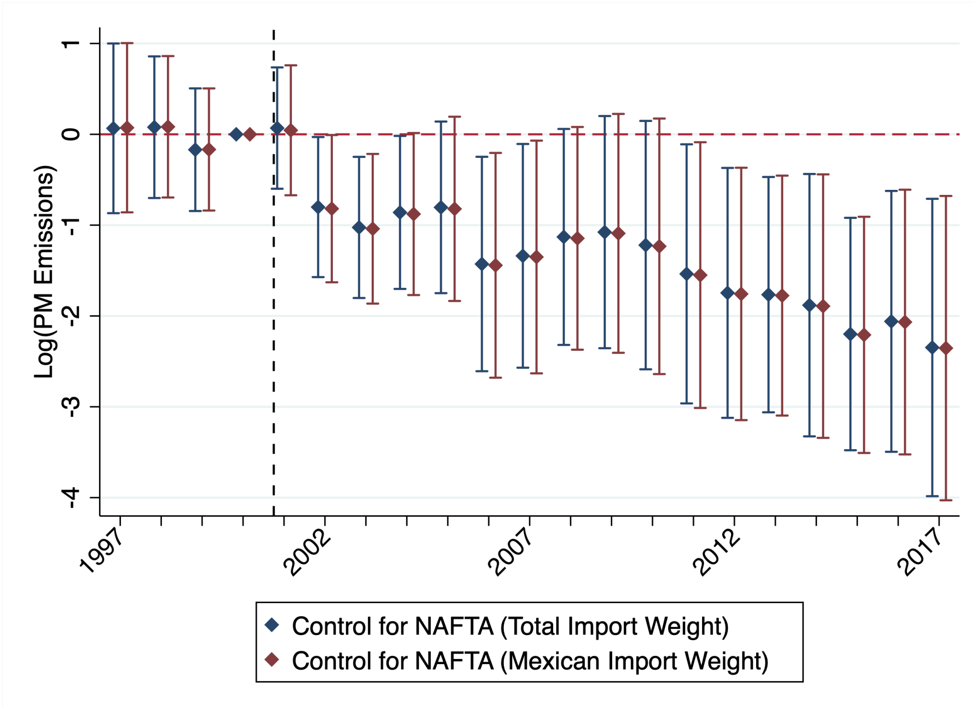
Notes: This figure displays the estimated difference-in-differences (DID) coefficients with their 95 percent confidence intervals, where we consider the sample period from 1995 to 2006. All other specifications are identical to those in Equation (4.3).

Figure D.7: Robustness: Dynamic Treatment Effects at the Establishment Level, 1997-2006



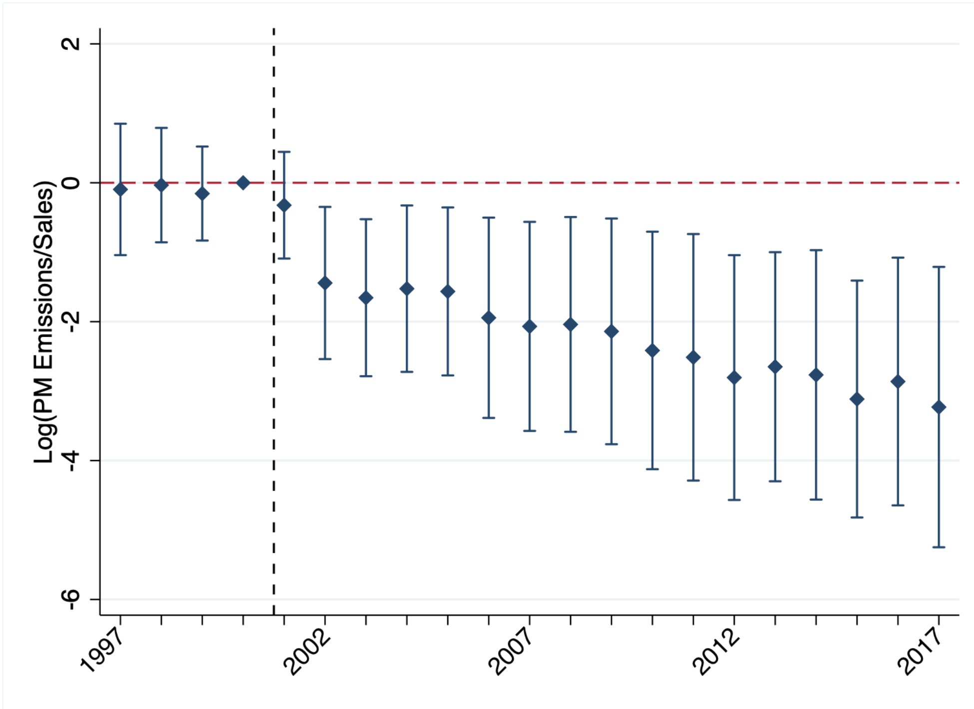
Notes: This figure displays the estimated difference-in-differences (DID) coefficients with their 95 percent confidence intervals, where we consider an extended sample period from 1997 to 2006. All other specifications are identical to those in Equation (4.3).

Figure D.8: Controlling for NAFTA: Dynamic Treatment Effects at the Establishment Level



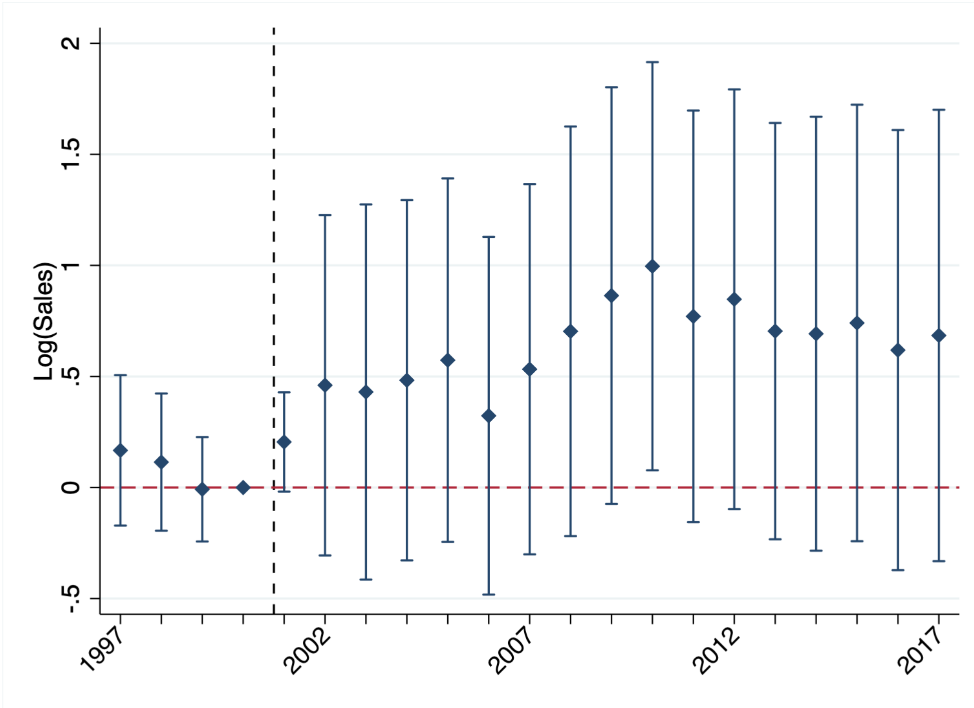
Notes: This figure displays the estimated difference-in-differences (DID) coefficients with their 95 percent confidence intervals, where we additionally control for the interaction of the post-PNTR indicator and the industry-level NAFTA tariff changes. Blue dots use US total imports as trade value weights 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 (4.3).

Figure D.9: 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 the dependent variable. All other specifications are identical to those in Equation (4.3).

Figure D.10: Dynamic Treatment Effects of Sales at the Establishment Level



Notes: This figure displays the estimated difference-in-differences (DID) coefficients with their 95 percent confidence intervals, where we use a measure of establishment-year-level sales—measured by log of sales—as the dependent variable. All other specifications are identical to those in Equation (4.3).

Appendix E Additional Tables

Table E.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 E.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 E.3: Additional Summary Statistics

Variable	Obs.	Mean	Std. Dev.	P10	P50	P90
(A) Industry-Year Level						
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						
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						
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						
PM Emissions$_{p,97}$	3858	41262	472714	0	15	17422
PM Emissions$_{p,97}$/Sales$_{p,97}$ (lb/million dollars)	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						
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 E.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$_{p,97}$ (million dollars)	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 the 50th percentile (median).

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

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

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

Table E.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 E.7: 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 E.8: PNTR and Establishment-level Pollution Emission Intensity, 1997 - 2017: Log(PM Emissions/Sales)

	(1)	(2)	(3)	(4)
	Log(PM Emissions/Sales)			
$\text{Post}_t \times \text{NTR Gap}_{i,99}$	-1.743***	-1.621***	-1.594***	-1.635***
	(0.514)	(0.597)	(0.544)	(0.535)
$\text{NTR}_{i,t}$			0.013	0.041
			(0.042)	(0.045)
$\text{MFA Exposure}_{i,t}$			-0.010	-0.008
			(0.018)	(0.018)
$\text{Post}_t \times \text{Log}(\text{NP}_{i,95}/\text{Emp}_{i,95})$				0.311**
				(0.155)
$\text{Post}_t \times \text{Log}(\text{K}_{i,95}/\text{Emp}_{i,95})$				0.172***
				(0.062)
$\text{Post}_t \times \Delta\text{Chinese Tariff}_i$				-0.852
				(0.572)
$\text{Post}_t \times \Delta\text{Chinese Subsidies}_i$				-74.698**
				(30.635)
Establishment FE	✓	✓	✓	✓
Year FE	✓	-	-	-
County x Year FE	-	✓	✓	✓
Observations	46753	46753	46753	46753

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 the dependent variable. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table E.9: 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 E.10: 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 E.11: 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 E.12: 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 the 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 E.13: Controlling for the Indirect Impact through Input-Output Linkages: Own, Upstream, and Downstream PNTR and Establishment-level Pollution Emissions, 1997 - 2017

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

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

Table E.14: Controlling for Upstream-Specific Time Trends:
PNTR and Establishment-level Pollution Emissions, 1997 - 2017

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

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

Table E.15: Accommodating Observations with Zero Emission by using PPML Regression: PNTR and Establishment-level Pollution Emissions, 1997 - 2017

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

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

Table E.16: Firm-level Pollution Emissions and Alternative Definition of Emission Intensity, 1997 - 2017

	(1)	(2)	(3)	(4)
	Firm-Year			Establishment-Year
	Log(PM Emissions)	Log(PM Emissions/Sales)	Log(PM Emissions/Emp)	Log(PM Emissions/Emp)
$\text{Post}_t \times \text{NTR Gap}_{i,99}$	-1.187***	-1.052**	-0.996**	-1.678***
	(0.416)	(0.441)	(0.415)	(0.526)
$\text{NTR}_{i,t}$	0.020	0.006	0.019	0.040
	(0.033)	(0.036)	(0.034)	(0.044)
$\text{MFA Exposure}_{i,t}$	-0.008	0.006	-0.002	-0.016
	(0.008)	(0.009)	(0.008)	(0.017)
$\text{Post}_t \times \text{Log}(\text{NP}_{i,95}/\text{Emp}_{i,95})$	0.185	0.056	0.104	0.349**
	(0.169)	(0.196)	(0.186)	(0.153)
$\text{Post}_t \times \text{Log}(\text{K}_{i,95}/\text{Emp}_{i,95})$	0.054	0.083	0.078	0.153**
	(0.062)	(0.072)	(0.065)	(0.062)
$\text{Post}_t \times \Delta\text{Chinese Tariff}_i$	-1.040	-0.903	-0.964	-0.892
	(0.717)	(0.884)	(0.850)	(0.550)
$\text{Post}_t \times \Delta\text{Chinese Subsidies}_i$	8.278	-4.534	-6.482	-65.190*
	(27.367)	(39.286)	(36.454)	(34.216)
Firm FE	✓	✓	✓	-
Year FE	✓	✓	✓	-
Establishment FE	-	-	-	✓
County x Year FE	-	-	-	✓
Observations	33416	33416	33416	46753

Notes. This table repeats the specifications in Column (4) of Table 2 (for Column (1)) and Column (4) of Appendix Table E.8 (for Columns (2)-(4)), where Columns (1)-(3) consider firm-year-level regression and Column (4) considers establishment-year-level regression with an alternative definition of emission intensity. Specifically, Column (1) uses firm-year-level pollution emissions as the dependent variable; Column (2) considers firm-year-level emission intensity, where emissions are divided by sales; Column (3) considers an alternative definition of firm-year-level emission intensity, where emissions are divided by employment. Column (4) repeats Column (3) at the establishment-year level. In Columns (1)-(3), we assign each firm with industry-level variables using the firm's primary industry. Standard errors (in parentheses) are either clustered at the industry level (Columns (1)-(3)) or two-way clustered at the industry level and county level (Column (4)). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table E.17: 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 3, where we use an establishment-year-level pollution emission intensity—measured by the log of PM_{10} emissions-to-sales ratio—as the dependent variable. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table E.18: 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 3, where we consider unconditional import intensity that incorporates non-importers. The regression includes all controls in Column (1) of Table 3, 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 perfect multicollinearity). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table E.19: 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)										
$Post_t \times NTR\ Gap_{i,99}$	-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)
$Post_t \times NTR\ Gap_{i,99} \times Import\ Intensity_{f,97}$		10.484**									14.160**
		(5.106)									(6.850)
$Post_t \times NTR\ Gap_{i,99} \times Nonattainment_{c,95-97}$			1.003								2.778
			(2.039)								(3.099)
$Post_t \times NTR\ Gap_{i,99} \times Upstream_{i,97}$				2.143							-1.696
				(1.348)							(2.704)
$Post_t \times NTR\ Gap_{i,99} \times Log(Num.\ 4\text{-digit}\ Sectors_{f,97})$					0.136						4.180*
					(0.697)						(2.171)
$Post_t \times NTR\ Gap_{i,99} \times Export\ Intensity_{f,97}$						0.398					6.454
						(2.067)					(6.732)
$Post_t \times NTR\ Gap_{i,99} \times Log(Num.\ Establishment_{f,97})$							-0.001				1.286
							(0.296)				(2.009)
$Post_t \times NTR\ Gap_{i,99} \times Log(Firm\ Employment_{f,97})$								0.028			-4.346***
								(0.327)			(1.553)
$Post_t \times NTR\ Gap_{i,99} \times Age_{p,97}$									0.013		0.030
									(0.016)		(0.026)
$Post_t \times NTR\ Gap_{i,99} \times I(Num.\ P2_{p,95-97} > 0)$										-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 3 (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 E.20: 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 3 (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 E.21: 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 the sample to establishments that did not belong to importing firms in 1997 (i.e., Import $\text{Intensity}_{f,97} = 0$). Column (2) focuses on the extensive margin adjustment of exporting activities within a firm by restricting the sample to establishments that did not belong to exporting firms in 1997 (i.e., Export $\text{Intensity}_{f,97} = 0$). Column (3) focuses on the intensive margin adjustment of exporting activities within a firm by restricting the sample to establishments that belonged to exporting firms in 1997 (i.e., Export $\text{Intensity}_{f,97} > 0$). 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 E.22: 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)
$\text{Post}_t \times \mathit{\Delta}\text{Chinese Tariff}_i$	0.117*	-0.026
	(0.068)	(0.881)
$\text{Post}_t \times \mathit{\Delta}\text{Chinese Subsidies}_i$	-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 E.23: 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.