Trade Liberalization and Pollution: Evidence from the China Shock

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Abstract

This paper exploits an exogenous shock to import competition, U.S. granting China Permanent National Trade Relations (PNTR) in 2000, to determine the effect of trade liberalization on industrial pollution. I find that increased geographic exposure to PNTR significantly reduced industrial pollution at the county level. A one unit standard deviation increase in exposure reduces PM2.5 concentration by 0.7 micrograms per cubic meter per year relative to counties that aren't exposed. I find similar results for NO2 and SO2 concentration. These results confirm a classic prediction of trade models–that increased trade (import competition) reduces domestic pollution. They also help explain the decline in aggregate pollution from 1990-2015. These estimates directly inform trade and environmental policy as Executive Order 13141 requires careful assessment and consideration of the environmental impacts of trade agreements. My results also contradict some of the previous literature on this topic.

Keywords: Air Pollution, Trade Liberalization, China Shock

JEL Codes: F13, F16, Q53, Q56

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1 Introduction

The environmental consequences of globalization are widely debated, yet, there is little consensus on what exactly this relationship looks like. The role of trade liberalization on the environment is an important part of this debate. There has been a large decline in U.S. manufacturing pollution since the early 1990s as emissions of "criteria" pollutants fell by 60% between 1990 and 2008 while aggregate output rose by 35% (Shapiro and Walker, 2018). During the same time period, Levinson (2015) shows that aggregate PM_{10} and SO_2 emissions fell by 3.55% and 3.61% yearly.

There are many possible explanations for why aggregate pollution has declined this significantly including advances in technology (Levinson, 2009), trade liberalization (Cherniwchan, 2017a) and domestic environmental policy (Levinson and Taylor, 2008). This paper focuses on the impact of trade liberalization and industrial pollution by exploiting the U.S. granting of Permanent Normal Trade Relations (PNTR) to China in 2001. The PNTR did not change tariffs with China but simply removed uncertainty in trading with China by ending the need for politically contentious annual renewals of tariffs on Chinese imports Pierce and Schott (2016). This strengthens import competition and suppressed U.S. employment in manufacturing as firms were incentivized to move production to China and establish relationships with Chinese suppliers.[1]

This policy change allows us to construct a measure of its effect–the "NTR gap" which is the difference between NTR tariff rates ($\approx$ 4% in 1999) to non-NTR rates ($\approx$ 36% in 1999). These gaps vary substantially across industries as well with a standard deviation of 15 percentage points. Consequently, it is possible to compute U.S. counties' exposure to the PNTR as the employment-share weighted average NTR gap across industries (Pierce and Schott, 2020). I exploit this geographic variation in exposure to PNTR to measure the impact of trade liberalization/Chinese imports on county-level pollution using a differences-in-difference identification strategy.

[1]For a more complete discussion of PNTR's possible effects, see Pierce and Schott (2016)

The intuition for why rising imports from China would lead to changes in domestic pollution is fairly clear. Since the U.S. has stronger environmental policy, pollution intensive production would relocate to China. This is the Pollution Haven Hypothesis which has also been studied in a variety of contexts. The evidence for this is fairly mixed which makes this China example compelling (Eskeland and Harrison, 2003). Moreover, since the shock is very large, we should expect to see significant changes in domestic pollution patterns (Autor et al., 2016a). This identification strategy allows us to compare counties that are more-exposed to Chinese imports while controlling for time-varying county demographics and exposure to other policies. Moreover, there is strong evidence that the moving of manufacturing jobs to China increased pollution in areas that are more exposed to these industries (Bombardini and Li, 2016).

This paper makes four primary contributions. First, it exploits within-country variation to produce causal estimates for the relationship between trade liberalization and pollution. Second, it provides strong causal evidence for a key theoretical prediction of international trade models–that increase import competition reduces domestic pollution. It also contradicts some of the previous empirical literature that finds no such relationship. Third, it provides some of the first evidence for the effect of Chinese import competition on U.S. pollution. Fourth, it contributes to a nascent literature that studies the health and wellness effects of Chinese import competition by offering pollution an an explanation for differential changes in health outcomes across US counties.

2 Related Literature

There is a large literature that studies the impact of rising Chinese import competition, starting in 1990 to present, on a variety of outcomes. Autor et al. (2013) exploits cross-market variation in import exposure and finds that rising imports cause higher unemployment, lower labor force participation and reduce wages. This depression in wages hasn't been temporary either and adjustments have been remarkably slow as wages and labor-force participation remain depressed for at least a full decade after the China trade shock begins (Autor et al., 2016b). The impact of import competition on domestic industries has also been studied in other contexts as well. For instance, Utar and Ruiz (2013) shows that Chinese imports negatively impact Mexican plants and employment especially in unskilled labor intensive industries. Bloom et al. (2015) finds that competition from China reallocated employment to more technologically advanced firms in Europe.

This paper focuses on the relationship between international trade and the environment. The previous literature tried to understand this relationship via decomposing the effect of trade on pollution into three primary channels–the scale of economic activity, across-industry changes, and within-industry changes (see e.g.: Grossman and Krueger (1991) and Copeland and Taylor (1994)). This type of work typically focusing on industry-level outcomes (Cherniwchan et al., 2017). However, further decomposition shows that changes attributed to industry-level emissions originate from within-industry, within-firm, and plant/task-level changes in emission intensities (Cherniwchan et al., 2017).

Cherniwchan (2017b) presents strong empirical support for this "micro-foundations" theory. The paper finds that trade liberalization, specifically the passage of NAFTA, significantly reduced plant-level emissions. Moreover, most of this decrease was driven as firms now had access to intermediate goods from Mexico. However, these are not causal estimates as tariff changes via NAFTA are far from exogenous and the "treatment" group is treated due to underlying industry trends. Moreover, there are other policy changes that occur around the same time as NAFTA which threatens identification. This paper, however, exploits plausibly exogenous geographic variation in exposure to the China shock to produce causal estimates.

Bombardini and Li (2016), perhaps the closest paper to mine, exploits variation in initial industry composition within Chinese prefectures and finds that export shocks significantly increase pollution and infant mortality. They identify two primary channels for how export shocks affect pollution. First, the "scale" effect is captured via the interaction between polluting industries and regional exposure to export shocks as prefectures that are more exposed to polluting industries and export shocks will simply pollute more. Second, the "technique" effect is captured by the dollar per worker associated with export expansion. This is essentially a measure of the income effect as higher incomes lead to higher demand for clean air. These channels relate directly to those identified in Copeland and Taylor (1994) and Grossman and Krueger (1991) and discussed earlier. They find strong evidence for the first channel as one unit increase in import exposure increases infant mortality by 2.2 deaths per thousand live births. They find some evidence for the technique/income effect using the value of export shocks, however, this is not significant. This paper is essentially the opposite set-up of this paper as I examine the impact of import shocks by exploiting geographic variation in exposure to Chinese imports.

Josh et al. (2004) uses a industry-level approach to examine the effect of trade liberalization on pollution between 1972 to 1994. It finds that a shift towards cleaner industries did occur but pollution-intensive industries are not disproportionately affected by the tariff changes. As I discuss below, my results somewhat contradict this as I do find a link between trade liberalization and pollution. However, with this empirical specification, it is hard to tell if the changes occur through a shift towards cleaner industries or if pollution-intensive industries are more likely shift production overseas.[2]

There is a recent literature that studies the health and wellness effects of the China shock. Pierce and Schott (2020) finds counties that are more exposed to the China shock exhibit higher rates of suicide and measures its impact on other health outcomes. Interestingly, it finds that the rates of death by heart attack and cancer go down. One of the channels for improving physical health could be reduction in pollution and my results can possibly directly answer this question. My identification strategy is exactly the same as this paper. However, I use different data sources to construct geographic exposure to PNTR.

[2] A future draft of this paper will use a slightly different identification strategy that takes into account the pollution intensity of each industry to compute a pollution-intensity-weighted NTR gap.

There is also a small literature that studies the effects of international trade and pollution using within-country variation (for e.g.: Dean (2002) and Chintrakarn and Millimet (2006)). However, both of these papers rely on within-country trade flows which are inherently less important relative to trade between countries, both in terms of policy interest and value. Since this paper exploits plausibly exogenous geographic variation to the China shock, stemming from initial exposure to industries via employment, I am able to avoid this problem while exploiting within-country variation. More specifically, this paper studies the impact of import competition stemming from China on industrial pollution using a differences-in-difference empirical strategy.

3 Data

This paper utilizes recently released CACES data which provide estimates of outdoor concentrations for six pollutants (four gases: O3, CO, SO2, NO2; Two aerosols: PM10, PM2.5) throughout the contiguous U.S. These estimates utilize publicly available concentration measurements from U.S. EPA regulatory monitors, and use information about land use (for example, locations of major and minor roads; elevation; and whether an area is urban or rural) and satellite-derived estimates of air pollution to predict concentrations at locations without measurements. These estimates are available at the national, state, county, census tract, and census block group levels but I use counties as that is the lowest level of resolution for which employment data is available. This data is available from 1990-2015 for most pollutants mentioned above. PM2.5 is only available starting 1999 which is still before the treatment comes into effect.

To construct geographic exposure to PNTR, I utilize replication files from Pierce and Schott (2020) for the 1997 4-digit SIC level exposure to PNTR which is simply the difference between the Non-NTR Rate and the NTR-Rate. The underlying data for these gaps come from tariff data provided by Feenstra et al. (2002) which report *ad valorem* equivalent NTR and non-NTR rates for 1989-2001 at the eight-digit Harmonized System (HS) level. Pierce and Schott (2016) use the U.S. Bureau of Economic Analysis' (BEA) HS-NAICS concordance to create time-consistent NTR gaps for each 4-digit SIC industry.

The county exposure to the PNTR is the employment-share weighted average NTR gap across the sectors in which they are active using employment shares from 1990 to deal with endogeneity. These are calculated as follows,

$$NTRGAP_c = \sum_j \frac{L_{jc}^{1990}}{L_c^{1990}} NTRGap_j$$

where c indexes counties and j indexes industries. This is the exact same measure as that of Pierce and Schott (2020). This is available from 1990 to 2015.

The employment-by-industry data comes from the Census Bureau's County Business Patterns data which is publicly available from 1946-present. I utilize recently released imputations by Eckert et al. (2020). This data previously difficult to use as the Census Bureau suppressed a majority of county-industry to protect confidentiality; and industry classifications change over time. Eckert et al. (2020) develop linear programming methods to exploit the implicit hierarchical arrangement of the data to impute missing cells. They also produce a time-consistent concordance.

Figure 1 provides some motivation for the paper. Aggregate pollution has decreased from 1990-2015 for a variety of industrial pollutants. This motivates the core idea of this paper as I'm trying to explain this decline. Table 1 shows descriptive statistics for all counties. Table 2 & 3 compares counties affected by PNTR to those that are not. To make this analysis simple, I compare counties with a positive NTR gap with zero NTR gaps. These tables preview my results as the counties affected by NTR gaps are, on average, more polluted prior to PNTR. However, this gap reduces significantly for the post period. This is suggestive evidence that PNTR reduced pollution as manufacturing shifted overseas.

In order to isolate the effect of PNTR on pollution, I control for exposure of each county to

Chinese tariffs and domestic production subsidies. These are computed in a similar fashion to NTR exposure. However, instead of utilizing industry employment structure, these measures utilize tariffs/subsidies associated with the goods produced by each county. Future drafts will also contain additional time-varying policy controls that track changes in NTR rates, Multi-Fiber Arrangements (MFA) and Chinese policy in response to it's ascension to the WTO. [3]

4 Identification Strategy

The baseline difference-in-differences (DID) specification examines whether counties with higher NTR gaps experience differential changes in pollution after the change in U.S. trade policy versus before. I estimate the following equation,

$$Y_{ct} = \alpha + \beta NTRGap_c \times Post_t + X_c \times Post_t + \delta_c + \delta_t + \epsilon_{ct}$$

where c indexes counties, and t indexes time (years). Y_{ct} represents the outcome variables which are a variety of pollutants that capture industrial pollutoin including PM2.5, NO2, and SO2. NTR Gap is the time-invariant county-level exposure to the PNTR. Post is a dummy that is 1 in years after 2000. The first term is the primary DD term of interest. δ_j and δ_t represent county and year fixed effects respectively. These controls for time invariant differences in counties and macro shocks that affect the entire country. I also run a specification with state-in-year fixed effects (δ_{st}) to control for state-level policy changes that could affect pollution. However, its unclear if there is enough variation within each state to pick up this effect.

In order to see the trajectory of changes in pollution, I also estimate a flexible event study framework where I estimate a set of betas, each corresponding to one year. This also allows

[3] For a more complete discussion of these controls, see Pierce and Schott (2020). Estimates from other studies with different outcomes show that these additions do not change estimates and are mostly a robustness exercise.

me to check for pre-trends and verifying the key assumption of a differences-in-difference empirical design. I estimate the following equation,

$$Y_{ct} = \alpha + \sum_t \beta_t NTRGap_c + \sum_t \theta_t X_c + \delta_c + \delta_t + \epsilon_{ct}$$

where I estimate coefficients for each year from 1991-2015 where 1990 is the base year. Results for this specification are presented via graphs with 95% confidence intervals.

5 Results

In this section, I report baseline results for differences-in-difference estimates to compare counties with exposure to PNTR to those that aren't.A one unit increase in the NTR Gap exposure reduced pollution by 0.11 micro-grams per meter cubed (See Table 4). One unit increase in standard deviation corresponds to a reduction of 0.7 micro-grams per meter cubed. This translates into a 10% increase in NTR Gap exposure reduces pollution by 8% annually. This estimate doesn't change when we add controls Chinese tariffs and subsidies. The estimates for PM2.5 are consistent across various specifications as well. It is also robust to clustering at the state level (this result isn't reported).

The graph of the flexible DD suggests that this effect happens consistently over the treatment period (See Figure 3 & Figure 4). Since PM2.5 data isn't available before 1999, it is impossible to test pre-trends for PM2.5. These estimates are also consistent with and without the Chinese subsidy and tariff controls.

This result, like the others, is however not robust to state-by-year fixed effects. This FE is included to deal with state level time varying changes via policy changes as states can independently enact environmental protections that reduce industrial pollution. This FE allows me to compares counties within each state. However, the result essentially disappears as there are very few counties in the average state which makes it hard to pick up the effect.

Table 5 finds a similar effect for S02. This result is consistent across every specification except the one with state-by-year FEs. They are also robust to clustering at the state level. The flexible DD picture also tells a similar story for SO2 but the effect takes longer to show up (See Figures 5 & 6). These estimates are also consistent with and without the Chinese subsidy and tariff controls. Moreover, there are essentially no pre-trends for SO2 as the most of the yearly estimates are statistically zero which supports the identification strategy.

The NO2 results don't fit this story and are more finicky (See Table 6 & Figure 7). The Chinese Tariffs and Subsidies explain a lot of variation in this case and are both statistically significant and have very large coefficients. This is interesting and could be specific to NO2. After controlling for the Chinese tariffs and subsidies, the event study pictures become way less finicky and tell a more consistent story (See Figure 8).

6 Discussion

This paper studies the effect of trade liberalization on industrial pollution. It contributes to a nascent literature that studies the health and wellness effects of trade liberalization (see e.g.: Pierce and Schott (2020), Hummels et al. (2016), McManus and Schaur (2016)). These papers find similar overall results that import competition reduces risk of workplace injury, but, can increase "deaths of despair" that are related to mental health due to job loss. This paper contributes to that literature by offering a channel for these outcomes. A reduction in pollution can improve health outcomes in the long run.

I also find partial evidence for the pollution haven hypothesis. As the U.S. increased environmental protections, firms moved manufacturing abroad which reduced domestic pollution. Moreover, we know these emissions don't just disappear as Bombardini and Li (2016) finds.

These results also partially contradict previous papers that explore the relationship between trade liberalization and pollution. For instance, Josh et al. (2004) finds that U.S. manufacturing shifted towards cleaner industries between 1972-1994. They use industry-level data on imports to examine if this compositional change was due to the significant trade liberalization that occurred over that period and find no such link. My paper directly contradicts this result as I provide strong causal evidence for exactly this relationship by comparing counties that are more severely by the trade shock to those that aren't. This difference in results could be due to different time periods. It could also be that Josh et al. (2004) failed to pick up this relationship as it utilized industry-level outcomes.

7 Appendix

Table 1: Summary Statistics

	Mean	St. Dev.	N	Min	Max
O3	48.54	6.32	80782.00	21.72	67.91
NO2	6.66	3.33	80782.00	0.17	40.88
PM10	19.57	4.62	80782.00	5.76	61.14
PM2.5	9.41	2.70	52819.00	2.35	22.12
SO2	2.68	1.73	80782.00	0.11	21.61
Median Income (1990)	31122.29	8427.06	80782.00	11213.00	77345.00
NTR Gap (1990)	7.40	6.47	80782.00	0.00	44.44
Observations	80782				

Table 2: Comparing Counties Affected by China Shock (Pre/1990-2000)

	Control		Treat		T-Test
	Mean	St. Dev.	Mean	St. Dev.	Difference
O3	47.34	6.29	50.17	6.76	-2.84***
NO2	5.45	1.68	8.12	3.52	-2.66***
PM10	18.46	4.36	21.42	4.19	-2.95***
PM2.5	7.44	2.59	11.51	3.29	-4.07***
SO2	2.48	1.04	3.83	1.85	-1.35***
Observations	627		33550		34177

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3: Comparing Counties Affected by China Shock (Post/2001-2015)

	Control		Treat		T-Test
	Mean	St. Dev.	Mean	St. Dev.	Difference
O3	47.81	5.35	47.37	5.71	0.44**
NO2	3.85	1.92	5.66	2.77	-1.82***
PM10	17.24	3.77	18.28	4.47	-1.05***
PM2.5	6.30	1.68	9.19	2.47	-2.90***
SO2	1.39	0.59	1.87	1.06	-0.47***
Observations	855		45750		46605

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4: PM2.5 (Differences-in-difference Estimates)

	PM25	w/ Controls	State-by-year FE
NTR Gap * Post	-0.115***	-0.116***	-0.00907***
	(-30.54)	(-18.71)	(-2.79)
Chinese Tariffs		-0.171	-1.869***
		(-0.13)	(-2.87)
Chinese Subsidies		-6.166	-31.07
		(-0.05)	(-0.57)
Year FE	Yes	Yes	Yes
County FE	Yes	Yes	Yes
State-by-Year	No	No	Yes
Adjusted R-squared	0.875	0.875	0.965
N	52819	52819	52802

Standard Errors are clustered at the county level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5: SO2 (DiD Estimates)

	SO2	w/ Controls	State-by-year FE
NTR Gap * Post	-0.0188***	-0.0271***	0.00141
	(-7.59)	(-7.57)	(0.49)
Chinese Tariffs		-2.322***	-2.025***
		(-3.03)	(-3.63)
Chinese Subsidies		17.46	70.17
		(0.26)	(1.46)
Year FE	Yes	Yes	Yes
County FE	Yes	Yes	Yes
State-by-year FE	No	No	Yes
Adjusted R-squared	0.808	0.808	0.914
N	80782	80782	80756

Standard Errors are clustered at the county level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6: NO2 (DiD Estimates)

	no2	w/ Controls	State-by-year FE
NTR Gap * Post	-0.00875***	-0.0306***	0.00754**
	(-2.91)	(-7.27)	(2.02)
Chinese Tariffs		-6.587***	-5.369***
		(-7.19)	(-7.91)
Chinese Subsidies		363.1***	290.2***
		(3.81)	(4.00)
Year FE	Yes	Yes	Yes
County FE	Yes	Yes	Yes
State-by-year FE	No	No	Yes
Adjusted R-squared	0.893	0.894	0.939
N	80782	80782	80756

Standard Errors are clustered at the county level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

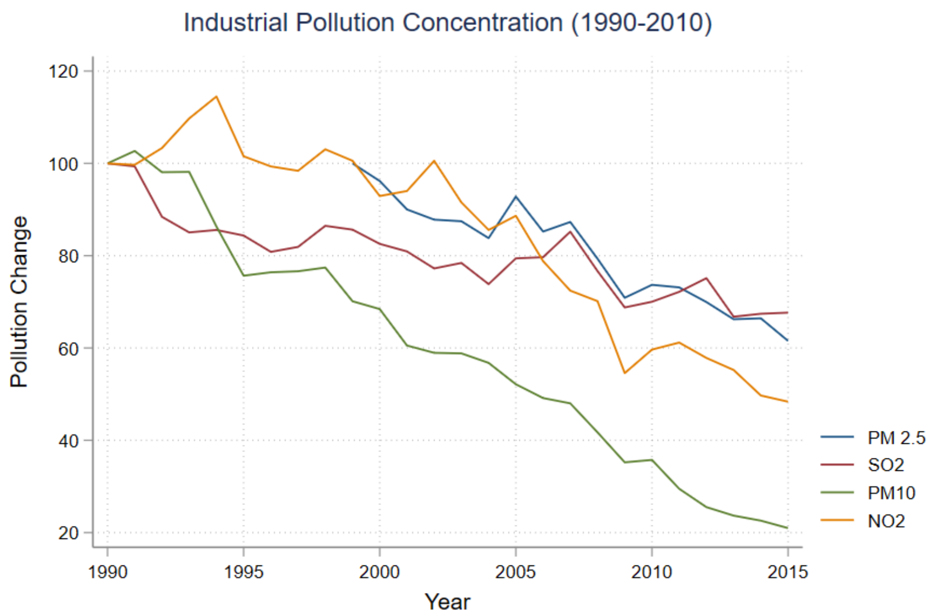

Figure 1: Aggregate Pollution from 1990-2015

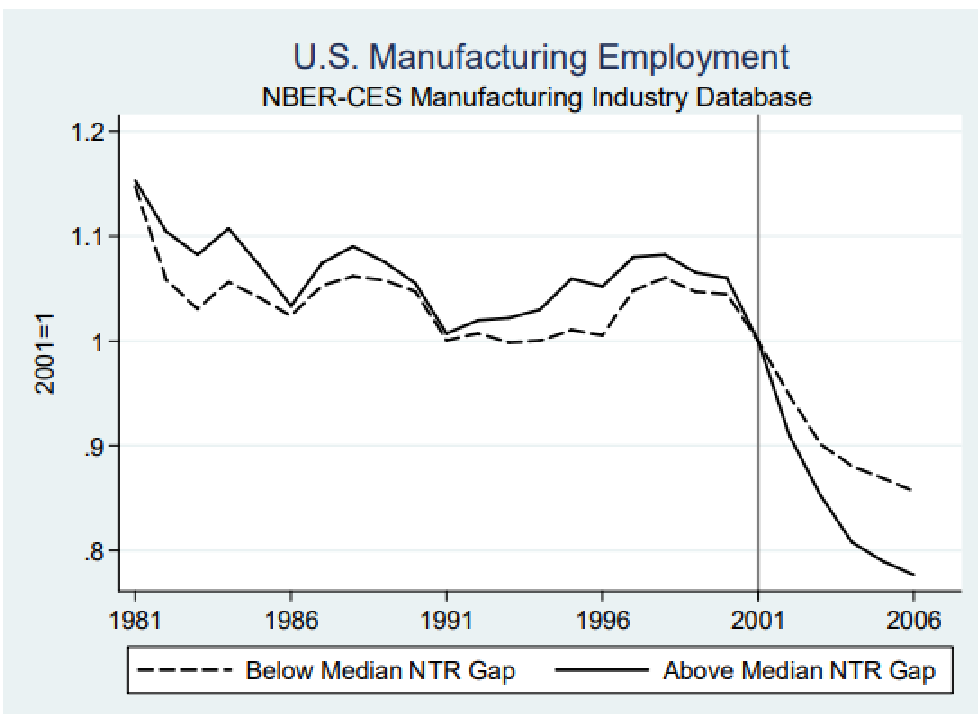

Figure 2: Effect of PNTR on Manufacturing Employment

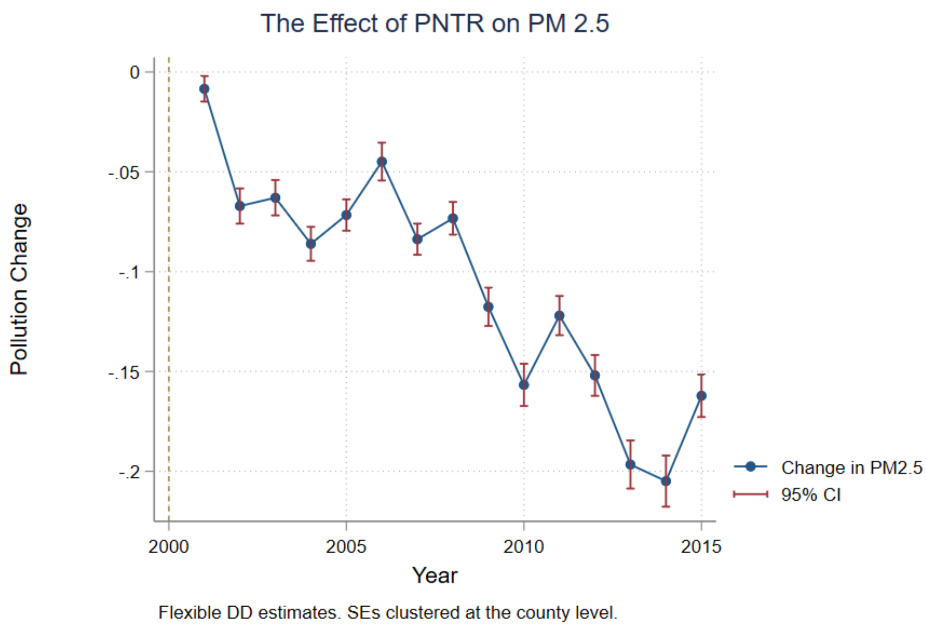

Figure 3: PM25 Flexible DD (No Controls)

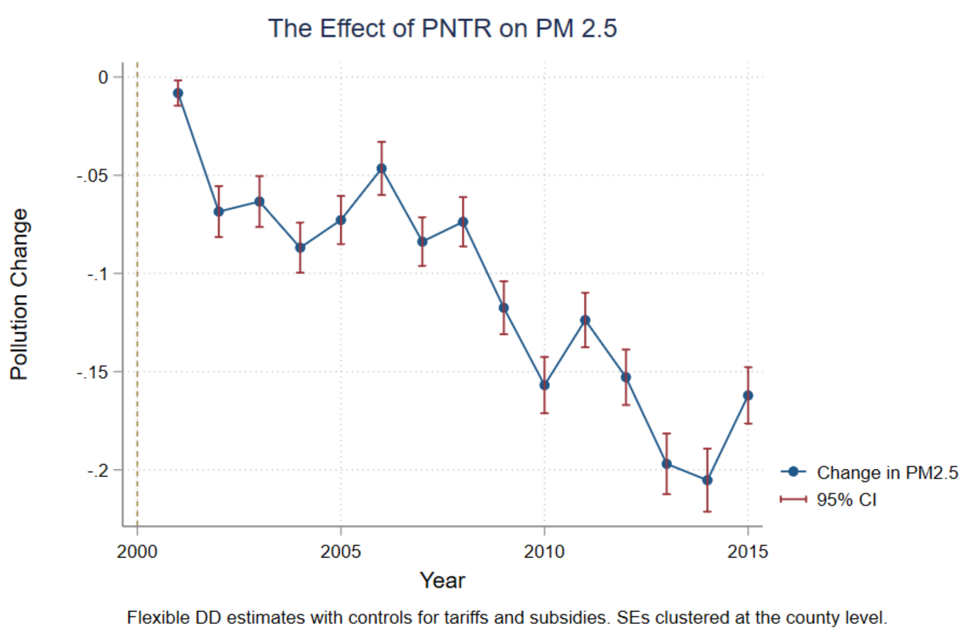

Figure 4: PM25 Flexible DD (w/ Controls)

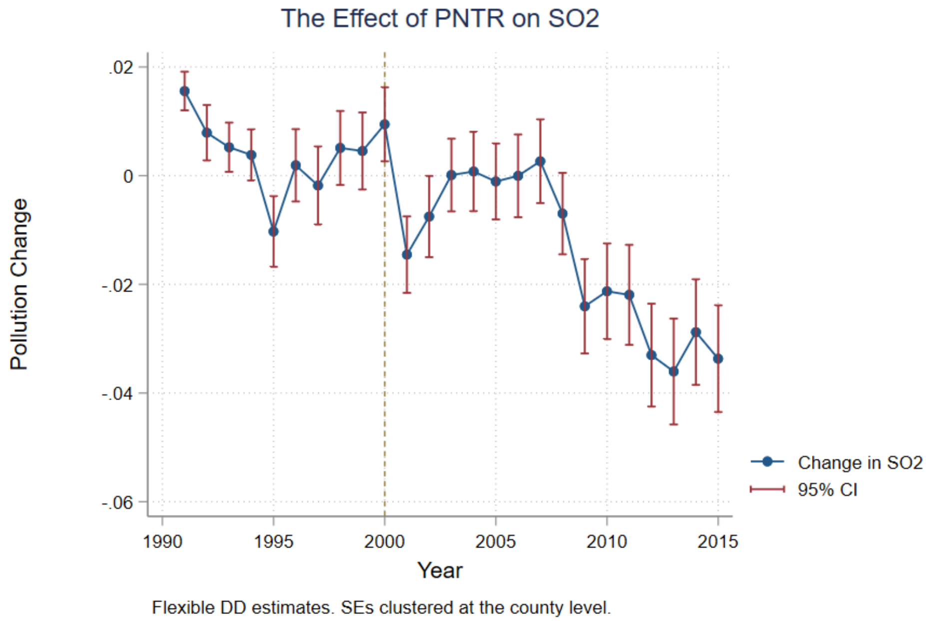

Figure 5: SO2 Flexible DD (No Controls)

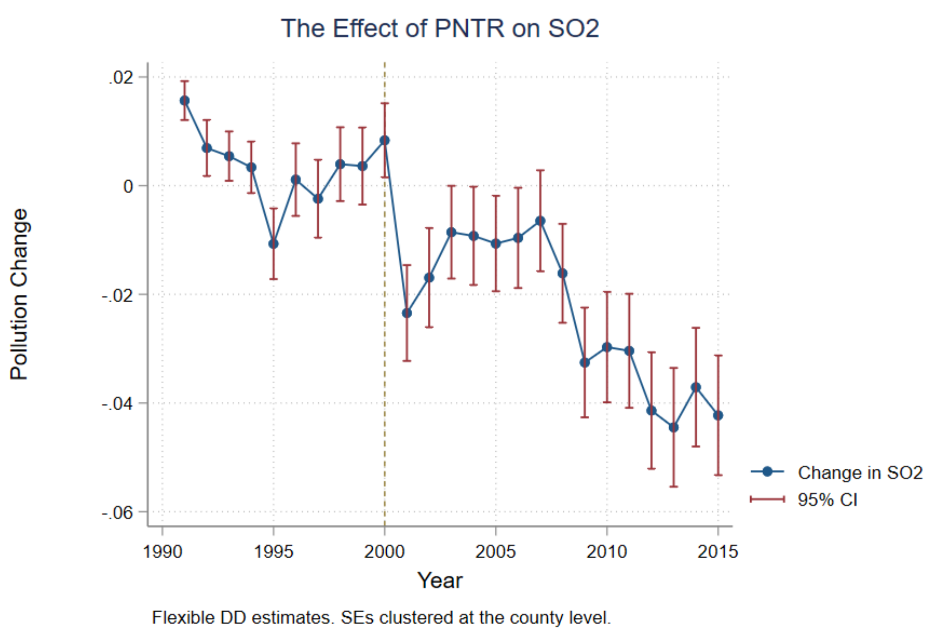

Figure 6: SO2 Flexible DD (w/ Controls)

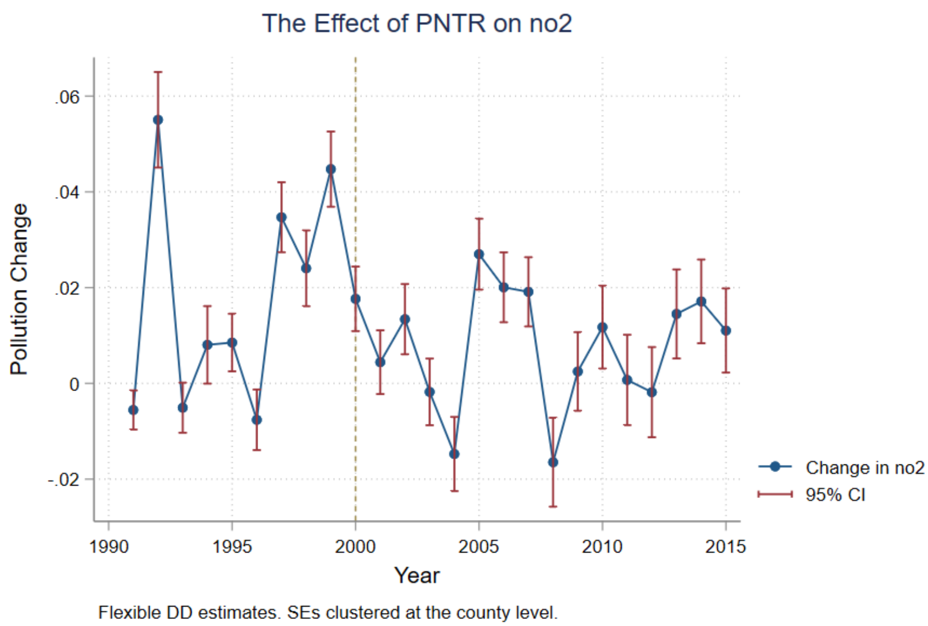

Figure 7: NO2 Flexible DD (No Controls)

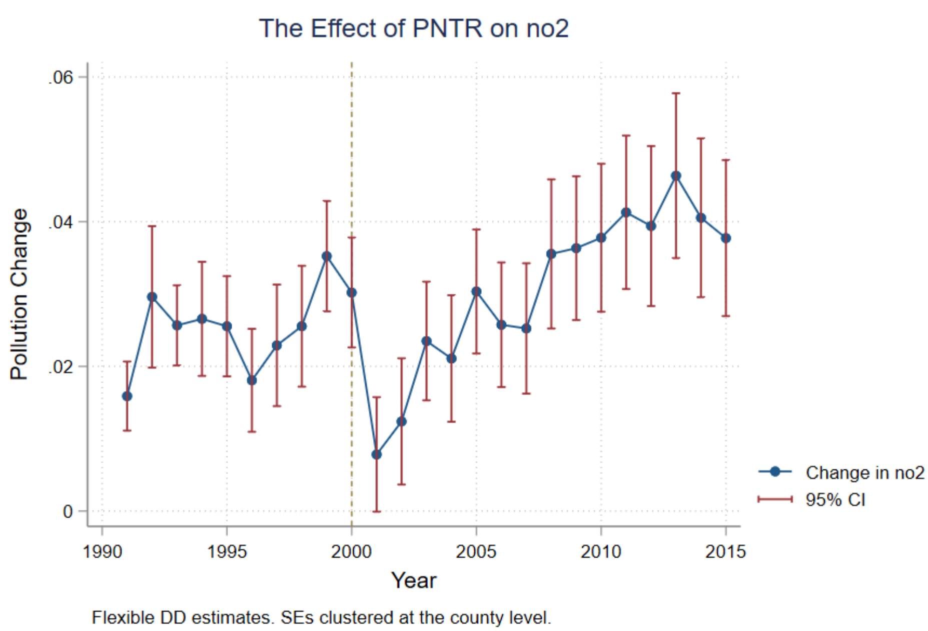

Figure 8: NO2 Flexible DD (w/ Controls)

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