Clearing the air: How fine particulate matter regulations reshape farmland values in U.S. corn and soybean regions

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Abstract

We investigate the impact of air quality regulations targeting fine particulate matter ($PM_{2.5}$) on farmland values in corn- and soybean-producing counties in the United States over the period 1997–2022. Using self-reported farmland value data from the Agricultural Census and county-level pollution classifications provided by the Environmental Protection Agency, we employ a difference-in-differences event-study design—incorporating inverse probability weighting and doubly robust estimators—to estimate the causal effect of regulatory interventions. Our primary analysis contrasts "non-attainment" counties, which failed to meet the National Ambient Air Quality Standards for $PM_{2.5}$, with those that consistently maintained compliance. We further assess heterogeneous treatment effects by extending our analysis with a triple-difference specification comparing counties with high versus low fertilizer use. Additionally, we employ the recentered influence function to conduct an unconditional quantile analysis across the entire distribution of farmland values. Our estimates indicate an 8.80–8.94% decline in farmland values in "non-attainment" counties in response to the enforcement of $PM_{2.5}$ standards, suggesting that the economic costs of the prescribed standards were capitalized into farmland values, particularly in regions with higher fertilizer use. However, this impact was not uniform, with more pronounced effects observed among counties at the lower end of the farmland value distribution.

Keywords: Air Quality Standards; Difference-in-Differences; Farmland Values; Unconditional Quantile Regression

JEL Classification: C21; Q15; Q53

1 Introduction

Agriculture in the United States (U.S.) is a significant source of air pollution, particularly through fine particulate matter ($PM_{2.5}$) emissions (Lelieveld et al., 2015; Giannadaki et al., 2018; Domingo et al., 2021), primarily formed from precursor pollutants like ammonia (NH_3). These ammonia emissions, largely stemming from nitrogen-based fertilizers used in agriculture, contribute around 30% of $PM_{2.5}$ pollution nationwide and 55% in the Corn Belt (Wyer et al., 2022).

Despite agriculture's substantial role in air quality degradation, it has historically been exempt from stringent environmental regulations due to its classification as a non-point source of pollution (Ruhl, 2000). However, increased scrutiny over agriculture's impact on air quality has led decision-makers to increasingly involve the sector in efforts to meet the National Ambient Air Quality Standards (NAAQS). In areas that fail to meet these standards, designated as "non-attainment," the Environmental Protection Agency (EPA) requires states to implement State Implementation Plans (SIPs). These plans outline specific and enforceable measures needed to comply with the NAAQS, which can be tailored to local agricultural activities. Additionally, in areas where agricultural activities contribute significantly to NAAQS violations, the EPA recommends the adoption of conservation practices approved by the U.S. Department of Agriculture (USDA) as part of these plans (Dressing, 2003).

Yet, the implementation of specific measures for agriculture continues to be debated. Many stakeholders in the sector regularly express concerns that stricter regulations may disproportionately affect farmers, especially in "non-attainment" areas (Aigner et al., 2003). Moreover, the lack of comprehensive studies examining the financial implications of air quality regulations for the agricultural sector has left this question unanswered.

Farmland represents the primary input and resource for the vast majority of U.S. farm households,[1] and its value serves as an important indicator of the agricultural sector's financial stability (Borchers et al., 2014). Changes in farmland values, in response to air quality regulations, can reflect how these regulations are capitalized into land prices, offering insights into their impact on the financial well-being of the agricultural sector. However, it remains unclear whether air quality regulations, particularly in "non-attainment" areas, lead to higher or lower land prices, as these regulations influence farmland values through several channels. Farmland may experience growth or decline in value depending on whether the benefits of cleaner air outweigh the costs of regulatory compliance. Moreover, benefits and costs may be heterogeneous across farmland types. Thus, estimating how air quality regulations are capitalized into farmland values is an important topic for informing policy decisions, which have historically been motivated by balancing environmental goals with the economic viability of the agricultural sector.

This paper examines how updates to the $PM_{2.5}$ NAAQS in the mid- and late-2000s have influenced farmland values across U.S. counties. We combine the EPA's classification of counties as either "attainment" or "non-attainment," based on their compliance with these standards, with county-level farmland value estimates from the USDA Census of Agriculture spanning from 1997 to 2022. Although these estimates are derived from opinion-based surveys, they represent the only county-level data covering such an extended period. The Census includes operations of all sizes, from small-scale urban plots to large commercial farms, ensuring a broad representation of the agricultural sector. Our analysis focuses on counties that are major producers of corn and soybeans, given the importance of these crops in U.S. agriculture. Regions heavily specialized in these crops have also been key areas of regulatory focus for the EPA. As a result, air quality policies in these areas have intensified in response to "non-attainment" status, leading to significant reductions in emissions from stationary sources (Zhang et al., 2024).

We use a difference-in-differences (DiD) event-study design to evaluate the causal effects of the NAAQS for $PM_{2.5}$ on farmland values, comparing outcomes in counties designated as "non-attainment" with those that

[1] In the U.S., the value of farm real estate assets (land and its attachments) accounted for USD 2.56 trillion or 82.7 percent of farm sector assets in 2019 (USDA, 2019).

remained in compliance throughout the study period. We further employ a triple-difference specification by incorporating fertilizer intensity as an additional layer of heterogeneity, which allows us to assess whether shifts in agricultural practices—such as fertilizer use—help explain the regulations' impact on farmland values. Finally, to determine whether these effects are distributed evenly or unevenly across the farmland value distribution, we incorporate recent advances in unconditional quantile regressions (UQR) estimated using a re-centered influence function (RIF) (Firpo et al., 2009) into our DiD analysis. This procedure offers the analytical advantage of directly estimating the marginal effects of $PM_{2.5}$ regulations at any point of the farmland value distribution.

A critical challenge in identifying the causal effect of $PM_{2.5}$ regulations on farmland values is to ensure the credibility of the Parallel Trends Assumption (PTA)—namely, that, in the absence of regulation, farmland values in "non-attainment" counties would have followed the same trajectory as those in "attainment" counties (Angrist and Pischke, 2009). However, farmland values may correlate with local conditions (e.g., economic activity, climate, or farming practices) that also affect treatment assignment, thus violating the identification assumption of conditional parallel trends. To address this issue, we first estimate a propensity score model using pre-treatment economic, demographic, and climatic characteristics identified as potential confounders. We then use estimated propensity scores to compute stabilized Inverse Probability Weights (IPW), which balance these observed covariates between "non-attainment" and "attainment" counties (Abadie, 2005). Next, we combine these stabilized IPWs with the Outcome Regression (OR) model of Heckman et al. (1997) in a doubly robust framework, following Sant'Anna and Zhao (2020). Under this approach, our estimates remain consistent as long as either the propensity score model or the outcome regression model is correctly specified, offering an additional layer of protection against confounding. We conduct extensive robustness checks—including tests of pre-trend equivalence and alternative model specifications—to reinforce the credibility of our identifying assumptions and confirm that the observed effects are not artifacts of omitted variables or functional form misspecification.

Our results show that farmland values in "non-attainment" counties have been negatively affected by $PM_{2.5}$ NAAQS compared to those in compliance with the prescribed standards, implying that the compliance costs of environmental regulations have been effectively priced into land values. As a consequence, "non-attainment" counties—initially characterized by higher average per-acre land values—have experienced a narrowing of the gap with "attainment" counties. Moreover, the relative decrease in farmland values has been more pronounced in counties with higher fertilizer use, suggesting that the regulations have effectively targeted areas with intensive agricultural practices and greater pollution potential. Finally, our results show that the "non-attainment" status has exacerbated disparities in farmland values among the affected counties over time, underscoring the distributional impact of complying with stricter regulations. Specifically, the narrowing of the land value gap between "non-attainment" and "attainment" counties has occurred to the detriment of an unequal distribution of regulatory impacts, with "non-attainment" counties that have lower initial land values experiencing a greater relative loss in farmland value compared to higher-value counties.

This article contributes to the understanding of policy impacts on farmland values by documenting effects driven by environmental regulations that have been largely overlooked in the literature. While extensive research exists on how agricultural support programs (Barnard et al., 1997; Weersink et al., 1999; Shaik et al., 2005; Latruffe and Le Mouël, 2009; Ifft et al., 2015) and sustainable agricultural practices (King and Sinden, 1988; Lynch and Geoghegan, 2007; Chen et al., 2023) influence farmland values, studies on the impact of environmental regulations remain comparatively rare. Sanders and Barreca (2022) examine the effects of the Acid Rain Program in the U.S., showing that reduced ambient sulfate levels decreased agricultural productivity by limiting soil sulfur, which in turn led to crop revenue losses for corn and soybeans, along with declines in land values. The response of farmland values to air quality standards that we identify in this paper expands this discussion by focusing on a different component of the U.S. efforts to improve air quality and protect public health and the environment.

This article also relates to the literature on the distributional impacts of environmental regulations, which examines the unequal economic and environmental consequences these policies impose across different regions, economic sectors, and social groups. A vast body of research has explored how environmental risks are distributed across populations to assess whether environmental policies help reduce disparities in risk exposure.[2] However, understanding the distribution of welfare gains from policy interventions also requires considering how changes in environmental quality influence financial compensations, since these economic shifts directly impact overall population welfare (Parry et al., 2005). Several empirical studies on the uneven distribution of the benefits of environmental improvements have focused on compensations through housing prices and wages. These studies demonstrate that such unevenly distributed benefits are reflected in capital gains for property owners (Grainger, 2012) and in wage premiums for workers in polluted areas (Roka and Palmquist, 1997; Blomquist et al., 1988). This article extends this issue to the agricultural sector, where the effects of stricter air quality regulations may similarly result in differential capitalization in farmland values. It also improves the analysis by integrating recent advances in unconditional quantile and recentered influence function (RIF) regressions in a DiD setting. This methodology addresses the limitations of Average Treatment Effects (ATE), which often overlooks valuable heterogeneity within the data (Athey and Imbens, 2016; Abadie, 2005) and the selection bias that may occur in traditional sample-stratifying approaches commonly used in this literature (Grainger, 2012; Bento et al., 2015; Jbaily et al., 2022). By examining the entire distribution of farmland values, our approach pinpoints which land value categories are most sensitive to stricter air quality standards, offering critical insights for policymakers.

The remainder of the paper proceeds as follows. Section 2 provides an overview of the evolving role of agriculture in $PM_{2.5}$ regulations and explores potential pathways for how these standards may affect farmland values. Section 3 presents the data sources, variables, and some stylized facts. Section 4 details the empirical methodology. Section 5 presents the empirical results. Section 6 concludes the paper.

2 Background

2.1 The evolving role of agriculture in $PM_{2.5}$ regulations

The regulatory framework governing fine particulate matter ($PM_{2.5}$) emissions is established under the Clean Air Act (CAA), which grants the Environmental Protection Agency (EPA) the authority to set and enforce National Ambient Air Quality Standards (NAAQS) for various pollutants, including $PM_{2.5}$.

Historically, the CAA has primarily targeted major sources of pollution, clearly identified in the industrial, transportation, and energy sectors. However, the agricultural sector has largely benefited from exemptions and selective regulation to balance environmental protection with the sector's operational and economic needs. While agricultural activities such as tilling, prescribed burning, and the use of nitrogen-based fertilizers have been recognized as contributors to $PM_{2.5}$ emissions, they have typically fallen below the thresholds defining "major sources" targeted by the EPA. As a result, the environmental impact of the agricultural sector has often remained outside the scope of stringent oversight, making it a minor focus under the CAA's framework (Ruhl, 2000).

Since the mid-2000s, however, perceptions of agriculture's role in air quality management have begun to shift, with the sector gaining increasing recognition for its contributions to $PM_{2.5}$ emissions. Insights from relevant "grey" sources, such as meeting minutes from the Agricultural Air Quality Task Force (AAQTF) — a collaborative advisory body established to address air quality issues related to agriculture— highlight how agriculture gradually became a focal point in $PM_{2.5}$ discussions.[3] Over time, the AAQTF has emphasized key sources of $PM_{2.5}$ emissions, including ammonia from fertilizers and dust generated by farming practices, while underscoring agriculture's potential role in helping states achieve State Implementation Plan (SIP)

[2] See Jbaily et al. (2022) for a recent study on $PM_{2.5}$ exposure disparities across U.S. population and income groups, and Currie et al. (2023) and Sager and Singer (2025) for recent articles exploring the effectiveness of $PM_{2.5}$ NAAQS in reducing pollution exposure risks between Black-White and Urban-Rural populations.

[3] The meeting minutes and reports are publicly available from 1998 to 2016. https://www.nrcs.usda.gov/conservation-basics/natural-resource-concerns/air/usda-agricultural-air-quality-task-force.

compliance with $PM_{2.5}$ standards. This evolving perception reflects broader efforts to align the agricultural sector with stricter air quality standards, balancing the regulatory leniency historically afforded by the CAA with the increasing responsibility of agriculture to mitigate its environmental impact.

In the mid-2000s, the AAQTF focused on identifying the scope of agricultural contributions to $PM_{2.5}$ and ensuring fairness in emerging regulatory frameworks. Discussions emphasized the need for rigorous modeling of ammonia emissions from fertilizers and livestock, reflecting a growing recognition that agriculture could be a significant source of $PM_{2.5}$ precursors. These discussions echoed the implementation of the National Air Emissions Monitoring Study (NAEMS) under the 2005 Air Compliance Agreement between the EPA and the agricultural sector, which aimed to collect data on air emissions from livestock operations, focusing on key pollutants, including ammonia, hydrogen sulfide, and particulate matter. During this period, concerns arose over whether existing $PM_{2.5}$ monitoring systems accurately captured emissions specific to agriculture, such as particulate matter from farm dust and ammonia released by fertilizers and livestock operations. As concerns about data reliability grew, the task force prioritized robust data collection and validation methods to prevent unnecessary restrictions on agricultural operations. The limitations of particulate sampling methods and inconsistencies in placement guidelines raised concerns about compliance for farms near "non-attainment" areas. Inaccurate or unfair measurements could expose these farms to stricter regulations and potential penalties. In the late 2000s, attention shifted toward promoting conservation practices and incentive-based programs to reduce $PM_{2.5}$ emissions. Discussions within the AAQTF highlighted the relevance of adopting practices such as no-till farming, cover cropping, and precision agriculture for agricultural producers, in a context where federal programs like the Environmental Quality Incentives Program (EQIP), designed to promote conservation and sustainable farming practices, were enhanced by the 2008 Farm Bill. This move toward voluntary and incentive-based strategies reflected growing awareness of the technical challenges involved in directly regulating agricultural emissions.

By the early 2010s, AAQTF discussions had expanded to address potential revisions of the NAAQS for $PM_{2.5}$, while continuing to caution against regulatory overreach. Although no immediate changes were mandated, there was growing recognition that improved data collection and emissions tracking could lead to stricter controls in the agricultural sector in the future. Discussions increasingly focused on ammonia emissions as a key driver of $PM_{2.5}$ formation, signaling that as monitoring systems and research advanced, agriculture's role would likely come under greater scrutiny, potentially subjecting livestock operations and fertilizer management to stricter oversight. By the mid-2010s, the AAQTF acknowledged that the integration of agricultural emissions data—such as findings from the NAEMS—into SIPs marked a new phase in aligning air quality improvements with on-farm practices. Strategies such as cover cropping, reduced tillage, and improved manure management were increasingly recognized as practical tools for reducing emissions. This period also saw significant policy advancements, with the adoption of the USDA's Conservation Practice Standard 376, known as Field Operations Emissions Reduction and initially piloted in California, as a national standard in October 2014. This expansion reflected a broader commitment to addressing particulate matter and other emissions from agricultural field operations.

2.2 Implications for farmland values

The growing emphasis on aligning sustainable practices in the agricultural sector with regulatory compliance has a direct impact on farmland values, particularly in regions subject to stricter air quality standards. By altering environmental conditions and farming practices, stricter air quality regulations—whether enforced through mandatory compliance or driven by economic incentives from federal farm policies—have the potential to reshape agricultural returns and land valuations in "non-attainment" areas. However, since farmland values are influenced by a complex interplay of both farm and nonfarm factors, the effects of these regulations manifest through a diverse range of mechanisms, as highlighted by various studies.

The primary impact of these regulations occurs through the financial costs agricultural producers incur to comply with more stringent environmental standards. By placing financial pressure on agricultural producers, these costs can reduce profitability, making land less attractive to potential buyers and investors, which ultimately drives down farmland values. For instance, Vukina and Wossink (2000) conducted an analysis of the Dutch nutrient quota system that illustrates how binding environmental quotas can erode farmland value. Similarly, Letort and Temesgen (2014) observed that stricter nitrogen management zones in France's Bretagne region negatively impacted land values, while more voluntary, incentive-based programs helped stabilize or even increase them.

Federal farm policies can mitigate the direct burden of compliance by offering financial incentives to farmers who adopt sustainable practices, such as precision agriculture and soil conservation. Studies by Miranowski and Hammes (1984); Ervin and Mill (1985), and more recently Piñeiro et al. (2020) and Chen et al. (2023), demonstrate that adopting advanced agricultural techniques, such as precision farming or soil conservation, can improve long-term agricultural productivity. King and Sinden (1988) highlight how investments in soil conservation enhance the land's productive potential, although these benefits may not always be immediately reflected in farmland values. Yet, as noted by Aigner et al. (2003) and Plastina et al. (2020), sustainable agricultural practices can carry risks, such as uncertain yields or lower short-term profitability, which may limit their immediate impact on farmland values.

In addition to these direct financial and operational effects, there are also broader, long-term benefits associated with cleaner air and improved environmental quality. Chang et al. (2016) and Zivin and Neidell (2012) emphasize how improving air quality benefits public health and environmental conditions, which in turn can enhance labor productivity. Cleaner air creates better conditions for crops and healthier work environments for farm laborers, potentially increasing the attractiveness and value of farmland. Lobell and Burney (2021) further demonstrate that improved environmental quality directly improves agricultural productivity, which is likely to contribute to higher farmland valuations over time.

The impact of $PM_{2.5}$ regulations on farmland values thus hinges on a delicate balance between the immediate financial burden of compliance and the more gradual, but potentially transformative, benefits of cleaner air and enhanced environmental conditions. This trade-off not only shapes farmland values but can also act as a mechanism through which air quality regulations may generate distributional effects across different areas.

By standardizing air quality requirements across counties, $PM_{2.5}$ NAAQS can disrupt the "pollution havens" phenomenon described by Herath et al. (2005) by reducing the incentive for intensive agricultural operations to relocate to areas with weaker environmental enforcement. Lee et al. (2021)'s findings from Taiwan highlight how weak enforcement can lead to increased farmland values near polluting factories, as non-agricultural economic opportunities emerge. In "non-attainment" regions where air quality fails to meet federal standards, stricter regulations could dampen such speculative gains in farmland value. As a result, farmland values may begin to converge between "non-attainment" areas—where land tends to command higher prices but is subject to stricter oversight and higher compliance costs—and "attainment" areas, making the $PM_{2.5}$ regulation relatively progressive across these two types of areas.

However, the implementation of stricter air quality standards could potentially exacerbate disparities across "non-attainment" counties. Borck and Schrauth (2021) and Carozzi and Roth (2023) provide evidence that a higher urban density intensifies local pollution exposure, which stricter $PM_{2.5}$ regulations in "non-attainment" areas could alleviate. Higher-value farmland, often located at the urban fringe, could benefit significantly from these improvements. Chicoine (1981) and Delbecq et al. (2014) emphasize that farmland near urban centers commands higher prices due to its accessibility and proximity to markets, advantages that may be further enhanced by reduced pollution exposure under stricter standards. In contrast, lower-value farmland, typically located in remote, rural areas with limited access to urban markets and infrastructure (Nickerson and Zhang, 2014; Shi et al., 1997), may face challenges in realizing similar benefits, as reduced pollution exposure in these regions could offer fewer economic advantages.

Stricter air quality regulations may also prompt agricultural producers in "non-attainment" areas to reduce the intensity of input factors such as pesticides, which could potentially affect profitability. Complying with more rigorous standards often requires capital investment in pollution control technologies and sustainable practices, which can further reduce agricultural profitability (Leng et al., 2023). High-value farmlands are generally better positioned to adapt, as they face less financial stress due to lower solvency (debt-to-asset) ratios (Burns et al., 2018). This financial advantage may enable them to invest in technologies and practices that reduce emissions or improve efficiency, thus mitigating the adverse impacts of stricter regulations on agricultural profitability and land value. In contrast, lower-value lands may lack the financial resources necessary for such adaptations, making them more vulnerable to the adverse effects of increased regulatory demands. Within "non-attainment" areas, lower-value farmland could therefore experience larger absolute losses in value compared to more valuable farmland.

In summary, stricter $PM_{2.5}$ regulations can yield complex and distributional consequences for farmland values. While some operations may benefit from improved air quality and supportive policies that increase productivity and land values, others, however, may face significant financial burdens from compliance. As a result, stricter regulations can exacerbate existing inequalities, with high-value farmlands better positioned to thrive under tighter standards, whereas lower-value lands struggling to adapt.

3 Data

3.1 Study area

Our study focuses on counties where corn and soybean acreage exceeds 5% of the total land area. This threshold, consistent with the broader delineation of the Corn Belt proposed by Green et al. (2018), provides a comprehensive representation of high-production agricultural regions, both in compliance with and in violation of the EPA's NAAQS for $PM_{2.5}$. A total of 1,929 counties surpassed this threshold during the study period, spanning the Midwest, South, Great Plains, and parts of the Northeast, as illustrated in Figure A.1 in Appendix A.

Together, corn and soybeans account for over half of the nation's cropland and generate an estimated 60–70% of total U.S. grain production sales (USDA, 2024a). As highlighted in the literature, their cultivation has historically relied on intensive agricultural methods heavily dependent on synthetic fertilizers, significantly contributing to ammonia emissions—a major precursor to $PM_{2.5}$ pollution (Wyer et al., 2022). This reliance on high-input farming has also been associated with substantial greenhouse gas emissions, further linking these regions to global environmental challenges (Lu et al., 2018; Yu et al., 2020; Behrer and Lobell, 2022).

However, in recent years, there has been a gradual shift toward the integration of advanced technologies, conservation practices, and optimized input use in corn and soybean production (Saavoss et al., 2021; Vaiknoras and Hubbs, 2023). This development has occurred within the context of broader air quality improvement efforts enacted through policies like the CAA. For example, Zhang et al. (2024) note that the evolution of SIPs, designed to address localized sources of pollution, has played a crucial role in reducing air pollution in states heavily specialized in corn and soybean production.[4]

This shift from high-input methods to more sustainable practices, in the context of evolving public health and environmental policies and localized adaptations, makes corn- and soybean-producing counties an appropriate setting to examine the impacts of environmental regulation on agricultural outcomes.

[4]Zhang et al. (2024) note that the Midwest, which initially lagged behind in air quality control, significantly reduced stationary source emissions from 45.31 million tons in 2000 to 20.71 million tons in 2020. This progress is largely attributed to the region's adoption of stringent SIPs, particularly in states like Illinois and Missouri, where policies combined robust enforcement measures with clear regulatory standards, reflecting a proactive stance on emission reduction. They further emphasize that Kentucky, Ohio, and Pennsylvania, despite facing economic and political hurdles—such as limited budgets, competing priorities, and political inertia—implemented innovative policies that streamlined administrative processes, empowered local governments, and encouraged public participation.

3.2 Designation status

The data collection process begins with the EPA Green Book, which reports county-level compliance status with the NAAQS for various criteria pollutants, including $PM_{2.5}$, annually from 2005 to 2024. Counties are designated as either "attainment" or "non-attainment" based on whether local pollution levels—calculated from local monitoring data—exceed or meet EPA thresholds for $PM_{2.5}$, ozone, sulfur dioxide, nitrogen dioxide, carbon monoxide, or lead. These designations, which are updated in response to changes in regulatory standards, form the basis for policy interventions and help determine where stricter controls or corrective measures are needed to protect public health.[5]

The stringency of these designations is directly tied to the evolution of NAAQS standards, which have become progressively stricter over time. Since their introduction in 1997, the NAAQS for $PM_{2.5}$ have undergone multiple revisions to reflect growing scientific evidence on the health risks associated with fine particulate pollution. The original 1997 standards set the annual limit for $PM_{2.5}$ at 15.0 μg/m^3 (based on a 3-year average of annual mean concentrations) and the 24-hour standard at 65 μg/m^3 (based on a 3-year average of the 98th percentile of daily concentrations). These standards were tightened in 2006, with the 24-hour standard reduced from 65 to 35 μg/m^3, while the annual standard remained unchanged. Further tightening occurred in 2012 when the annual standard was lowered to 12.0 $\mu g/m^3$, though the 24-hour standard of 35 μg/m^3 remained in place.[6] The evolution and tightening of these standards is summarized in Table 1.

Table 1: Evolution of $PM_{2.5}$ standards

Year	Average time	Level	Form
1997, 2005, 2008	24-hour	65 μg/m^3	98th percentile, averaged over 3 years
1997, 2005, 2008	annual	15 μg/m^3	Annual arithmetic mean, averaged over 3 years
2006, 2009, 2012	24-hour	35 μg/m^3	98th percentile, averaged over 3 years
2012, 2015, 2016	annual	12 μg/m^3	Annual arithmetic mean, averaged over 3 years

Note: The first year listed is the year in which the updated final rule was published in the Federal Register. The second year listed is the year in which areas were fist designated as "non-attainment" under the new standards. The final date is the year in which states had to submit their plans for achieving or maintaining compliance with the revised $PM_{2.5}$ standards. Source: EPA.

Following Currie et al. (2023), we aggregate counties into commuting zones (CZs) to better align with the spatial scale at which environmental policies and their economic effects, such as changes in farmland values, occur.[7] Just as "non-attainment" areas often coincide with Metropolitan Statistical Areas (MSAs) or CZs, these aggregations reflect the broader "air regions" defined by the EPA, which extend across multiple counties. As such, the boundaries of "non-attainment" areas typically extend beyond individual counties, as pollution can spill over into neighboring areas, impacting a larger regional economy.

Of the 1,929 counties exceeding the 5% threshold for combined corn and soybean acreage, 300 were designated as "non-attainment" under the different $PM_{2.5}$ NAAQS. The remaining counties were never designated as "non-attainment" under any revision of the $PM_{2.5}$ NAAQS. Panels A and B of Figure 1 illustrate the status of these counties and the corresponding trends in $PM_{2.5}$ concentration estimates obtained from the database of Meng et al. (2019). We use the 2001–2003 annual average concentrations as the baseline period (consistent with the EPA's three-year averaged monitor readings used for designation under the 1997

[5]For the purposes of our analysis, we treat full and partial "non-attainment" statuses as equivalent when assigning treatment status.

[6]These stricter standards, aimed at improving public health outcomes, remain in effect as of 2024. On February 7, 2024, the EPA issued a final rule to strengthen the nation's NAAQS for fine particle pollution. Under this rule, the annual primary (health-based) $PM_{2.5}$ standard has been revised from 12.0 to 9.0 μg/m^3, while the primary $PM_{2.5}$ standard remains at 35 μg/m^3.

[7]Developed by the USDA Economic Research Service using census-derived commuting patterns, these commuting zones reflect worker flows and economic interdependencies between counties. https://www.ers.usda.gov/data-products/commuting-zones-and-labor-market-areas/documentation.

PM$_{2.5}$ NAAQS), and the 2015–2019 annual averages for the final period.[8]

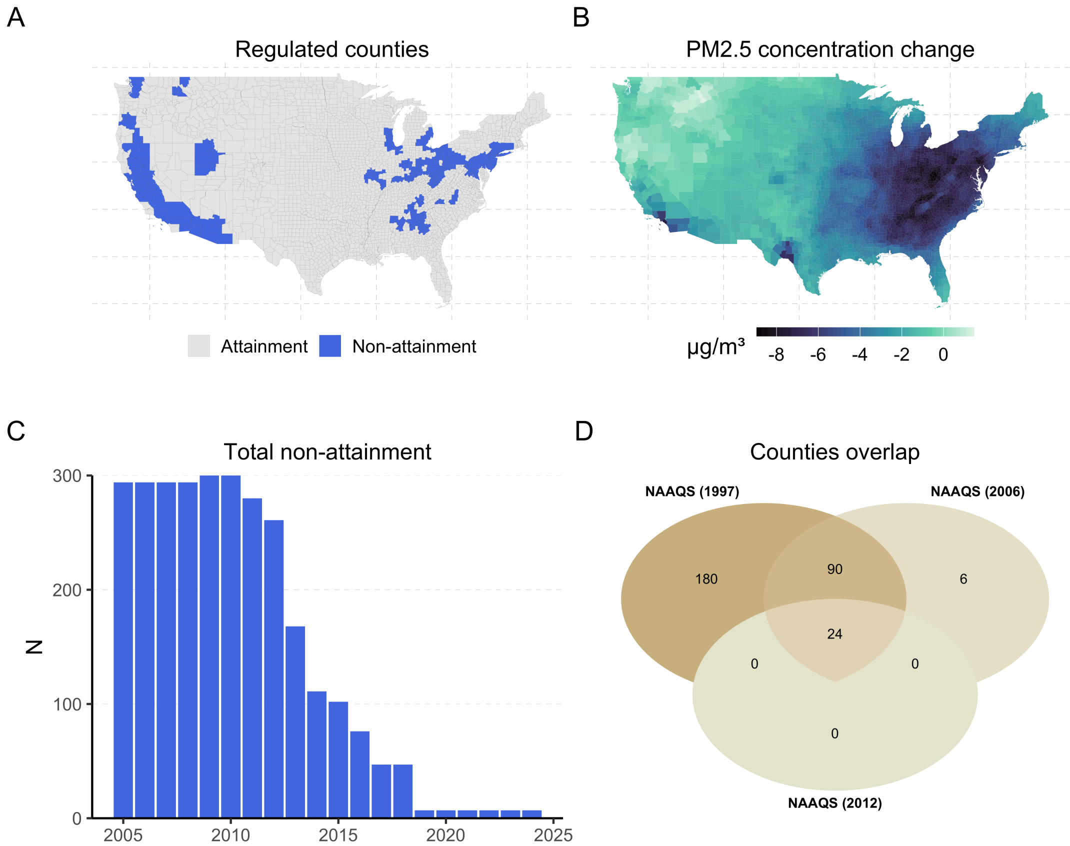


Figure 1: PM$_{2.5}$ concentrations and "non-attainment" designations

Note: Panel A shows the geographical distribution of counties designated as "non-attainment" over the study period in the U.S. Panel B presents the changes in PM$_{2.5}$ concentrations between the reference period of 2001–2003 and the period 2015–2019, using estimated PM$_{2.5}$ concentrations from Meng et al. (2019) database. Panel C depicts the total number of "non-attainment" counties in our sample from 2005 to 2024. Panel D illustrates the overlap of "non-attainment" designations under the 1997, 2006, and 2012 PM$_{2.5}$ NAAQS across our sample. Source: EPA Green Book, authors' calculation from Meng et al. (2019).

Updates to NAAQS led to changes in the EPA's designation process, which significantly impacted regulatory enforcement and air quality, particularly in the eastern U.S. Counties in this region that were initially designated as "non-attainment" saw a substantial decline in air pollution emissions from 2001–2003 to 2015–2019 (Figure 1, Panel B). In our sample, average PM$_{2.5}$ concentrations declined by 4.68 μg/m^3 over the same period. Figure 1, Panel C, illustrates the role of EPA enforcement efforts in bringing corn- and soybean-producing areas into compliance. When counties were first designated under the 1997 PM$_{2.5}$ NAAQS in April 2005, 294 were classified as "non-attainment," all exceeding the annual threshold, with some also violating the daily threshold.[9] By 2020, only seven counties remained in "non-attainment" status,

[8] We use PM$_{2.5}$ concentration estimates from a high-resolution dataset that integrates satellite remote sensing, chemical transport models, and ground-based calibrations (Meng et al., 2019). These data, resolved at a 1-km grid, enable precise estimation of county-level PM$_{2.5}$ concentrations. By intersecting these spatially disaggregated data with county boundary shapefiles from the U.S. Census Bureau, we compute annual PM$_{2.5}$ averages for each county and calculate the changes in PM$_{2.5}$ concentrations between the reference period of 2001–2003, when PM$_{2.5}$ concentrations were used to designate counties as "attainment" or "non-attainment" under the 1997 rule, and the period 2015–2019. The dataset is freely available at: `https://sites.wustl.edu/acag/datasets/historical-pm2-5-across-north-america/`.

[9] PM$_{2.5}$ measurements used to designate "non-attainment" status come from a limited network of ground monitors deployed by the EPA between 1999 and January 2001. Although these monitors were predominantly located in more densely populated counties, they covered only about 20% of all counties, potentially overlooking areas that might otherwise have been subject to regulation (Sager and Singer, 2025).

demonstrating significant progress toward compliance.[10]

Interestingly, the reclassification of counties into "non-attainment" status under the 1997 $PM_{2.5}$ NAAQS saw little change under the subsequent 2006 and 2012 revisions. From the initial designation in 2005 until the full enforcement of the regulation in 2011—which included the period for SIP submissions and the first compliance deadlines (2008–2010)—no counties were reclassified to "attainment." Furthermore, following the 2006 revision of the daily $PM_{2.5}$ standard, which took effect in December 2009, only six counties were newly designated as "non-attainment." Finally, no county has ever been reclassified from "attainment" to "non-attainment" under the 2012 revised standards. Put simply, once a county was designated as "non-attainment" under the 1997 $PM_{2.5}$ standards, it remained regulated in all subsequent revisions (Figure 1, Panel D).

3.3 Farmland values

Our outcome measures rely on data from the Census of Agriculture, conducted every five years by the National Agricultural Statistics Service (NASS). The Census covers a wide range of farm operations—ranging from small urban plots to large rural enterprises—and includes data on land use, ownership, operator characteristics, production practices, income, and expenditures at the county level. For the purposes of this study, we focus on the per-acre estimated value of farmland, which reflects the market value of both land and buildings as reported by farm operators. These data are collected through self-reported estimates, reflecting the price at which land would be sold under current market conditions, regardless of ownership. The use of self-reported values to measure farmland values is well-established in the literature (Deschênes and Greenstone, 2007), and these values offer the most comprehensive dataset available for the analysis period (1997–2022). While their validity may raise concerns, there is evidence supporting the reliability of these measures. A closer examination shows that self-reported farmland values closely track market trends, making them reliable indicators of land prices (Zakrzewicz et al., 2012). Furthermore, the USDA has implemented rigorous methodologies, including probabilistic record linkage and imputation for missing data, to address concerns about potential biases arising from respondents' perceptions or market thinness (USDA, 2024b).

The USDA Census data refines the initial dataset of counties with corn and soybean acreage exceeding 5% of the total land area, resulting in an unbalanced panel of farmland value observations.[11] In total, our final sample consists of 10,768 county-year observations corresponding to an average of 1,796 counties per Census year. This sample includes, on average, 286 counties (15.9%) designated as "non-attainment" under the NAAQS for $PM_{2.5}$.[12]

Between 1997 and 2022, farmland values in our sample increased substantially—from \$1,606 per acre in 1997 to \$5,591 per acre in 2022, a 3.5-fold increase.[13] In 1997, counties designated as "non-attainment" had an average farmland value of \$2,780 per acre, nearly double that of the \$1,378 per acre in "attainment" counties. By 2022, while "non-attainment" counties still had higher farmland values, averaging \$7,824 per acre compared to \$5,182 per acre in "attainment" counties, the relative difference between the two groups decreased, from 101.6% in 1997 to 51.0%.[14]

The slower growth in "non-attainment" counties contributed to the narrowing gap over time, with a notice-

[10] For a five-year overview of counties' "attainment"/"non-attainment" status, see Figure A.2 in Appendix A.

[11] Some counties have missing data for certain years in the USDA Census of Agriculture due to withheld values intended to prevent the disclosure of information about individual operations.

[12] The proportion of counties in "non-attainment" areas versus "attainment" areas is consistent across years and is not significantly affected by the unbalanced nature of the panel. Additionally, the sample has been winsorized at the 1% level to mitigate the influence of extreme values.

[13] Farmland values in our sample consistently exceed national averages. For context, nationwide farmland values increased from \$942 per acre in 1997 to \$3,800 per acre in 2022. See the Land Values 2022 Summary from the United States Department of Agriculture available at https://www.nass.usda.gov/Publications/Todays_Reports/reports/land0822.pdf

[14] The summary statistics of farmland values by status are presented in Table A.1 in Appendix A.

able shift observed between 2007 and 2012 in "non-attainment" counties, as shown in Figure 2, Panel A. This change was particularly evident at the lower end of the farmland value distribution (around the 20th percentile), suggesting that the narrowing gap was not uniform across the value spectrum (Figure 2, Panel B). In Panels C and D, which focus on $PM_{2.5}$ concentrations, a significant overall decline is observed in both county groups, with a sharp convergence beginning in 2007. By the end of the period, "attainment" and "non-attainment" counties exhibited comparable $PM_{2.5}$ concentration levels.

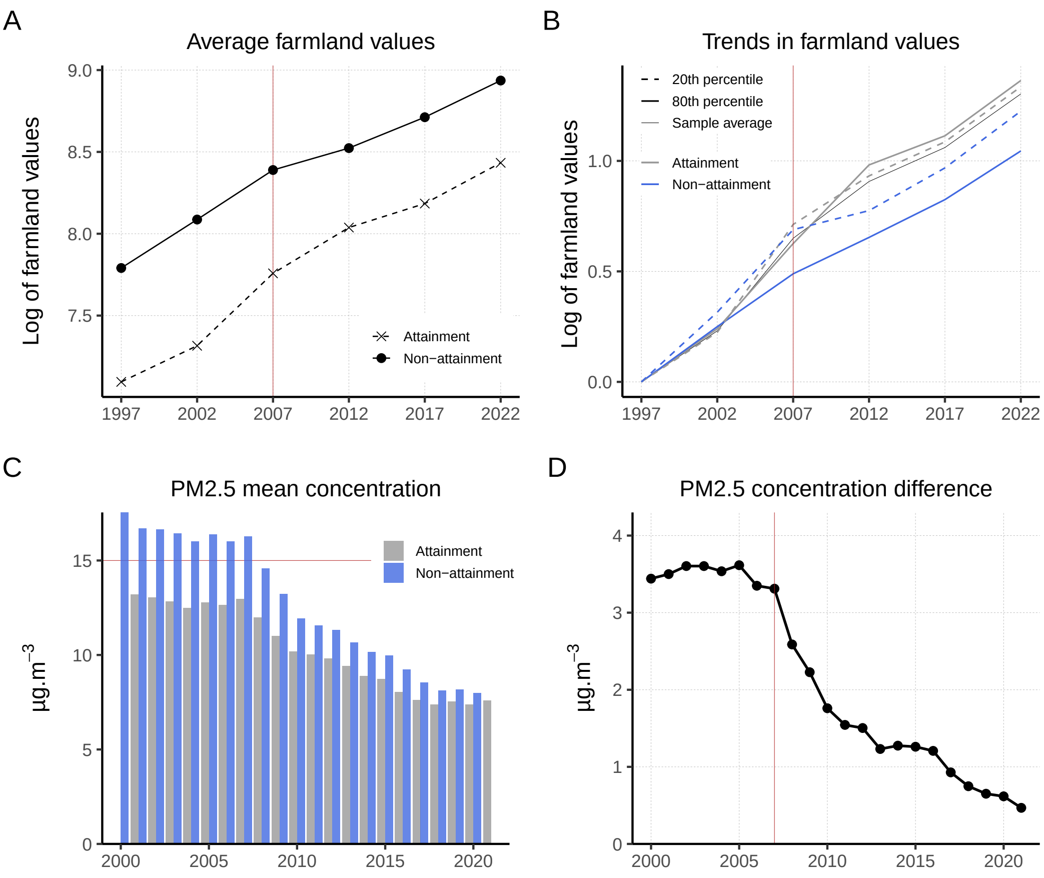


Figure 2: $PM_{2.5}$ concentrations and farmland values

Note: Panel A displays the log-transformed average farmland values over time for "attainment" and "non-attainment" counties. Panel B shows the log-transformed trends in farmland values, re-based to the 1997 Census year, for the 20^{th} and 80^{th} percentiles, as well as the sample average in both "attainment" and "non-attainment" counties. Panel C presents the mean $PM_{2.5}$ concentrations for both "attainment" and "non-attainment" counties from 2000 to 2020. Panel D shows the difference in $PM_{2.5}$ concentration between "non-attainment" and "attainment" counties over time. Source: Census of Agriculture, authors' calculations from Meng et al. (2019).

4 Identification strategy

4.1 Baseline specification

As seen in Section 3, nearly all counties designated as "non-attainment" under the 2006 and 2012 standards had already been classified as such in 1997.[15] Therefore, we define treatment as a county's initial designation as "non-attainment" under the $PM_{2.5}$ NAAQS, i.e., those that failed to meet the 1997 air quality standards.

[15] Only six counties do not overlap with previous "non-attainment" designations.

Counties that maintained "attainment" status throughout the study period and were never designated as "non-attainment" under any revision of the $PM_{2.5}$ NAAQS are included in the control group.

We designate 2012 as the start of the post-treatment period, corresponding to the first census year after the enforcement of the 1997 NAAQS for $PM_{2.5}$, which began with the 2008 State Implementation Plan (SIP) submissions and the 2010 compliance deadlines.[16] We also test an alternative specification using 2007 as the treatment start year. Since regulatory enforcement started after 2007, any treatment effect observed in that year would indicate the influence of factors other than the regulation—such as anticipatory behavior or pre-existing trends. In contrast, finding no effect for 2007 would support the validity of our main specification.[17]

A critical characteristic of the "non-attainment" designation process is its absorbing nature. Once a county is designated as "non-attainment" under the CAA, it remains under continuous regulatory requirements, even if air quality improvements later bring it into compliance with the NAAQS. Rather than reverting to a fully deregulated status, such a county is reclassified under "maintenance" status. This reclassification ensures ongoing regulatory oversight, requiring the county to implement and maintain emission control measures to prevent future violations (EPA, 2013).

Our main specification relies on the following event study:

$$Y_{it} = \lambda_i + \delta_t + \sum_{k \neq -15} \beta_k \left(\mathbb{1}\{t - t^* = k\} \times \mathbb{1}\{D_i = 1\} \right) + \epsilon_{it} \tag{1}$$

where Y_{it} denotes the log-transformed farmland values for county i in census year t; λ_i and δ_t capture county and year fixed effects, respectively, with the year fixed effects accounting for time-varying factors, including interest rates, that influence land prices across all counties. $\mathbb{1}\{D_i = 1\}$ is an indicator equal to 1 if county i is designated as non-attainment and $\mathbb{1}\{t - t^* = k\}$ is an indicator that equals 1 if the census year t is exactly k years relative to the treatment year, t^*. The coefficients β_k represent the Average Treatment Effect on the Treated (ATT), which measures the difference in log farmland values between counties regulated under the $PM_{2.5}$ standards and those that remain in compliance at each relative time period k. We exclude $k = -15$ from the estimation to serve as the baseline period.

A key identification assumption underlying the specification in Equation (1) is the parallel trends assumption (PTA), which requires that, in the absence of treatment, treated and control counties would have followed similar trends in farmland values. However, in our application, systematic differences between the two groups may arise due to the non-random assignment to treatment. In particular, factors such as proximity to metropolitan areas or historical compliance with other environmental regulations may simultaneously affect both the likelihood of being designated as "non-attainment" and the evolution of farmland values (Sager and Singer, 2025).[18] Given these observable imbalances, we cannot rule out the possibility that post-regulation differences in farmland value trends are driven by unobserved shocks unrelated to the $PM_{2.5}$ NAAQS designation.

One way to increase the credibility of the PTA is to require that it holds conditionally on covariates—the conditional parallel trends assumption (CPTA). However, simply adding time-varying covariates to the standard two-way fixed effects (TWFE) model does not guarantee that the CPTA holds. When treatment effects are heterogeneous, the TWFE estimator may produce biased estimates of the ATT (Meyer, 1995; Abadie, 2005). To mitigate this issue, we explore alternative estimation strategies that account for potential bias and improve identification validity.

[16]See Figure A.3 in Appendix A for the $PM_{2.5}$ NAAQS timeline.

[17]If farmers and state governments anticipated the regulatory changes introduced by the $PM_{2.5}$ NAAQS and began adjusting their practices or policies before the formal treatment date, it would violate the standard DiD assumption of no anticipation. In such cases, the impact attributed to the 2012 treatment date could be underestimated, as some effects may have already occurred by 2007.

[18]Of the 300 counties in our CZ sample designated as "non-attainment" for the $PM_{2.5}$ NAAQS, 146 had previously been regulated under one or more other NAAQS—specifically, those for sulfur dioxide (1971), PM_{10} (1987), and/or carbon monoxide (1971).

4.2 Estimation procedure

Recent advances in DiD econometrics have introduced semi-/non-parametric methods such as matching, Outcome Regression (OR), Inverse Probability Weights (IPW) and Doubly Robust estimators (DR), which allow for consistent estimation of the ATT in the presence of heterogeneous effects (Heckman et al., 1997; Abadie, 2005; Athey and Imbens, 2016; De Chaisemartin and D'Haultfoeuille, 2020; Callaway and Sant'Anna, 2021; Sun and Abraham, 2021). In particular, IPW has been widely used to re-balance the control group by assigning weights based on the inverse of estimated propensity scores. This procedure constructs a synthetic counterfactual distribution, thereby approximating the experimental conditions of a randomized trial. By re-weighting the control group to better resemble the treatment group, the estimator improves the balance between the two groups and strengthens the CPTA—ensuring that any observed divergence in post-treatment outcomes can be more credibly attributed to the regulatory intervention rather than to underlying pre-treatment differences.

Simulation studies show that when the propensity score model is correctly specified, IPW tends to produce less biased estimates compared to alternative methods like matching or outcome regression. Moreover, IPW estimates are unbiased as long as the treatment and control groups are well-balanced (Zhou et al., 2020; Bettega et al., 2024).[19] However, the standard practice of using unstabilized IPW (Horvitz–Thompson) approach has been shown to yield weights that can be extremely large if some units have a very low probability of being in their observed group. These excessively large weights inflate the variance of the estimator and can make the results unstable.

In this article, we use a Hájek-type stabilized IPW technique to provide consistent estimates of the ATT. Compared to the conventional Horvitz-Thompson estimator, the stabilized IPW estimator minimizes the influence of extreme weights, which can arise when some propensity scores are very close to zero (Abadie, 2005). The Hájek-type SIPW estimator is generally preferred over the unstabilized IPW estimator for ATT, due to its improved finite-sample stability and precision, resulting in gain of efficiency (lower variance) over the unstabilized version, especially in scenarios with limited overlap.

Formally, the stabilized IPW approach begins by estimating the propensity score, $\pi(x_i)$, i.e., the probability of a county receiving treatment, based on its pre-treatment characteristics $\mathbf{X}_{i,0}$, using logistic regression as:

$$\pi(x_i) = \Lambda(x_i) \equiv \frac{\exp(\mathbf{X}_{i,0}'\gamma_0)}{1 + \exp(\mathbf{X}_{i,0}'\gamma_0)} \tag{2}$$

where γ_0 is a vector of parameters to be estimated and $\Lambda(x_i)$ denotes the logistic function. The vector $\mathbf{X}_{i,0}$ includes a set of baseline confounders that influence both treatment assignment and farmland values.

These confounders encompass climatic, agricultural, demographic, and economic factors, including accumulated Growing Degree Days, drought frequency, intensity and frequency of storm-related winds, surface temperature inversion, off-farm income, fertilizer and chemical expenses, federal payments, farm concentration, irrigation, population density, and poverty estimates. A detailed discussion and full description of the selected covariates is provided in Appendix B. All confounders are measured over the pre-treatment period to minimize potential confounding and avoid including post-treatment variables, which could act as bad controls.

We first estimate the propensity score model from Equation (2), including all confounders in $\mathbf{X'}_{i,0}$ as linear terms, without incorporating interaction effects or second-order polynomial transformations. Since the con-

[19] Several techniques have been proposed to address selection bias in panel DiD models. For instance, matching and overlap weights improve covariate balance but may be limited in high-dimensional settings or exclude observations with extreme propensity scores (Crump et al., 2009; Abadie and Imbens, 2011). Machine learning methods (Random Forest, Gradient Boosting, DoubleML), while flexible, often require careful calibration (Chernozhukov et al., 2018). To our knowledge, few studies have applied these algorithms to the panel structure in DiD frameworks. In contrast, (stabilized) IPW effectively accounts for the panel structure and can limit extreme weights (Abadie, 2005), offering an optimal solution to mitigate selection bias and increase the credibility of the CPTA. For a recent literature survey on advances in DiD models, see Roth et al. (2023).

sistency of the IPW approach in estimating the ATT depends on the specification of the propensity score model, we estimate an alternative model that incorporates interactions among chemical and fertilizer expenditures, irrigation and drought frequency, and county population density and poverty.[20] We also examine additional specifications that include each set of interactions individually, all of which yield similar results.[21]

Next, we estimate the ATT using the Hájek-type stabilized IPW estimator, defined as:

$$\text{ATT}_{\text{sipw}} = \mathbb{E}\left[\left(\frac{D_i}{\mathbb{E}[D_i]} - \frac{\hat{\pi}(x_i)(1-D_i)}{(1-\hat{\pi}(x_i))\mathbb{E}\left[\frac{\hat{\pi}(x_i)(1-D_i)}{1-\hat{\pi}(x_i)}\right]}\right)\cdot \Delta y_i\right] \tag{3}$$

where $\hat{\pi}(x_i)$ is the estimated propensity score from the logit regression using maximum likelihood estimation and Δy_i is the outcome difference for county i. The term on the right side inside the brackets represents the stabilized weight applied to untreated units that can be interpreted as the inverse probability of being in the control group, scaled by the overall odds of treatment in the sample. The term on the left side represents the weights for the "non-attainment" counties.

Since the Parallel Trends Assumption (PTA) cannot be directly tested, we rely on an event-study design as an indirect check to assess its validity. By analyzing event study coefficients and the trends leading up to the regulation on $PM_{2.5}$ levels, we compare the pre-treatment trends between "non-attainment" and "attainment" counties to assess whether the two groups followed similar trends before the regulation's implementation (Currie et al., 2023; Sager and Singer, 2025). Evidence of parallel pre-treatment trends would support the validity of the PTA and reinforce the credibility of our causal estimates. Additionally, we conduct placebo tests as further robustness checks. However, as no statistical test can conclusively prove the absence of violations of the PTA, we conduct a sensitivity analysis that relaxes the strict conditional parallel trends assumption (CPTA). We follow the approach of Rambachan and Roth (2023), calculating confidence intervals within which our estimates remain valid in the presence of small, undetected departures from the CPTA.

A visual inspection of the propensity score distributions for the treatment and control groups in Figure 3 shows that the re-weighting process effectively balances their distributions. Before re-weighting, the propensity score distributions for "attainment" and "non-attainment" counties differ markedly, while after the application of IPW, the overlap between the two groups improves substantially.

To further assess covariate balance, we report the average covariate values, standardized mean differences (SMDs), t-test statistics, and p-values for mean differences for each group. The results are summarized in Table C.1 in Appendix C.[22] Overall, the weighted sample exhibits reduced differences across most covariates: after re-weighting, nearly all SMDs fall below the strict threshold of ± 0.1, indicating improved comparability between the treatment and control groups.[23]

[20] The interaction between chemical and fertilizer expenses examines their combined effect on ammonia emissions and farmland values, particularly in regions subject to strict environmental regulations. Irrigation practices are also interacted with climatic variables, such as drought frequency and growing degree days, to assess how the effectiveness of water management strategies varies with the severity of climatic stressors. Finally, the interaction between population density and counties' poverty estimates captures the compounded impact of urbanization and economic deprivation on the adoption of agricultural technologies in low-income areas.

[21] Detailed results for these alternative models are available upon request.

[22] We recommend relying on the conventional SMD threshold, as a large part of the literature advises against using hypothesis tests for balance assessment. This is because balance reflects the comparability of the sample itself, rather than serving as an inference about a larger population (Austin, 2009; Imai et al., 2008; Ho et al., 2007).

[23] The one notable exception is the cumulative Growing Degree Days (GDD) variable, with an SMD of −0.167, which still indicates an acceptable level of balance. In the binary exposure causal inference literature, a commonly suggested cutoff value for the SMD is ± 0.2 (Austin, 2009). However, we also test an alternative specification excluding potential confounders that show lower imbalance prior to adjustment and higher imbalance in post-weighting process, with no noticeable change in the results compared to our benchmark specification (see Table C.3 in Appendix C).

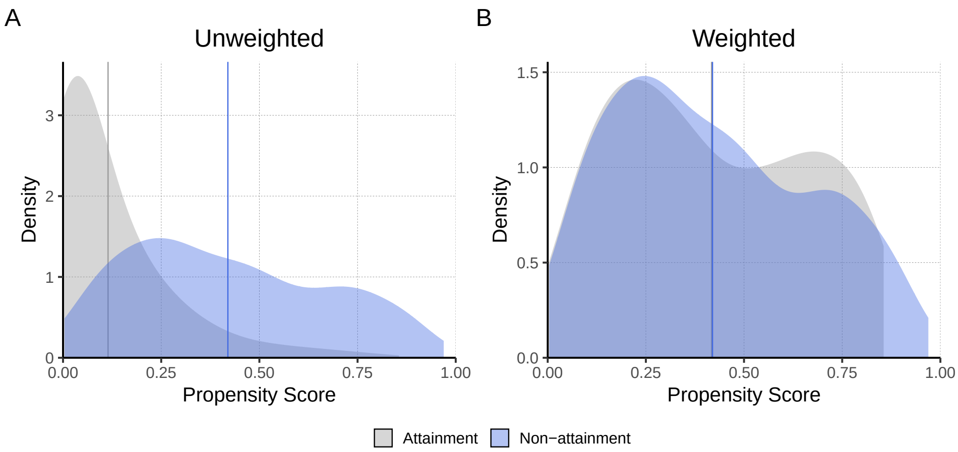


Figure 3: Distribution of propensity scores for the treatment and control groups, before and after re-weighting

Note: The figure displays kernel density estimates of the propensity score distributions for "attainment" and "non-attainment" groups before (Panel A) and after (Panel B) re-weighting. The grey and dark blue vertical lines denote the average estimated propensity scores before and after applying weights for the "attainment" and "non-attainment" counties, respectively.

To assess the robustness of our specification in Equation (1), we compare the ATT estimates from our primary IPW approach with those obtained using an improved doubly robust (DR) estimator (Sant'Anna and Zhao, 2020). The DR estimator combines Outcome Regression (OR) (Heckman et al., 1997) and IPW, offering greater efficiency in treatment effect estimation. Our implementation follows a two-step approach. First, we estimate propensity scores using Inverse Probability Tilting (IPT) (Graham et al., 2012), which improves finite-sample performance compared to traditional maximum likelihood methods. Next, we estimate the outcome regression using weighted least squares (WLS), assigning weights based on estimated propensity scores to mitigate heteroskedasticity associated with treatment assignment. The key advantage of the DR estimator lies in its double robustness—ensuring that ATT estimates remain consistent even if either the OR model or the propensity score model is misspecified. This dual safeguard reinforces the validity of our point estimates and confidence intervals under model uncertainty.

We also estimate our benchmark model using recent advancements in synthetic control methods (SCM). While SCM was initially designed for a single treated unit, recent methodological advances have extended its applicability to multiple treated units with heterogeneous effects (Abadie and L'hour, 2021; Ben-Michael et al., 2021). SCM creates a synthetic counterfactual by constructing a weighted combination of control units that closely match the treated units in pre-treatment characteristics. Unlike the IPW framework, SCM does not rely on a pre-specified functional form, adding an additional layer of robustness to our estimation strategy.

We further implement the augmented synthetic control method (ASCM) proposed by Ben-Michael et al. (2021) and Abadie and L'hour (2021) to mitigate residual bias stemming from imperfect pre-treatment matching. Unlike traditional SCM—which constructs a synthetic counterfactual solely based on weighted control units that closely follow the treated unit's pre-treatment trajectory—ASCM begins with the standard SCM estimate and then uses an outcome regression model to predict the treated unit's counterfactual outcome. This model quantifies the bias arising from any pre-treatment discrepancies between the treated unit and its synthetic control, effectively correcting the initial SCM estimate. When pre-treatment fit is strong, the estimated bias is negligible, and ASCM yields results nearly identical to SCM. Conversely, when pre-treatment fit is weak, the outcome model provides a substantial adjustment by leveraging the relationship between pre-treatment predictors and outcomes, thereby reducing bias in the final treatment effect estimate.[24]

[24]We test the specification in Equation (1) by implementing synthetic control method (SCM) estimators both with and without

Baseline estimates are calculated with bootstrapped standard errors based on 10,000 repetitions clustered at the county level. Given the potential spatial correlation in farmland values, standard errors are also clustered at the CZ level. To address concerns that spatial correlation may extend beyond these boundaries and bias the standard errors, Conley (HAC) standard errors are also employed (Conley, 1999).

4.3 Extensions

Potential mechanisms

As discussed in Section 2.3, $PM_{2.5}$ NAAQS can exert downward pressure on farmland values in "non-attainment" counties through two primary channels: (a) they can increase the costs of compliance, as farmers in "non-attainment" areas must invest in technologies or agricultural practices to reduce emissions and meet stricter air quality standards, which raises operational costs; and (b) they can make farmland less attractive in "non-attainment" counties, as the economic environment in these counties no longer benefits from the less stringent environmental regulations that were previously in place.

While we cannot definitively isolate the role of each mechanism in driving the observed slower growth in farmland values in "non-attainment" counties, we refine the difference-in-differences (DD) framework by distinguishing counties based on their intensity of fertilizer use. This decomposition allows us to investigate potential heterogeneity in the treatment effect, particularly since areas with higher agricultural intensification may face greater compliance costs due to the need for more significant changes in practices to meet stricter environmental standards. Therefore, the treatment effect may be more pronounced in these areas, where the increased operational costs from compliance are likely to be higher.

To formally account for this potential mechanism, we augment our IPW-DiD event-study framework by adopting a Difference-in-Differences-in-Differences (DDD) specification, incorporating fertilizer intensity as a moderating variable. Specifically, we define the categorical moderator, l_i, based on county-averaged fertilizer expenses per harvested acre during the pre-treatment period as follows:

$$l_i = \begin{cases} A, & \text{if } k_i > K \text{ (high-fertilizer-intensity counties)} \\ B, & \text{otherwise (low-fertilizer-intensity counties)} \end{cases} \tag{4}$$

where k_i represents fertilizer expenditures per harvested acre for county i, and K is the average from the pretreatment period, used as a threshold value to classify counties into high- and low-intensity fertilizer users.

The DDD specification assumes that only regulated counties in the high-intensity subgroup ($l_i = A$) are directly exposed to the full effects of $PM_{2.5}$ constraints. In contrast, counties with lower fertilizer intensity ($l_i = B$) in "non-attainment" areas may experience weaker or no direct regulatory effects, as stricter air quality policies are likely to bind more strongly for larger-scale, high-emission farming operations. Thus, in the post-treatment period, high-intensity counties in regulated areas receive the strongest form of treatment, while other counties serve as controls, allowing us to further isolate the regulatory effects from confounding trends. To capture this heterogeneity in regulatory impact, the DDD specification incorporates counties in a comparable subgroup as an additional counterfactual. This approach allows us to account for differences in treatment intensity, while still controlling for all the variables included in Equation (1). Thus, our augmented

bias correction. Bias correction is used in synthetic control methods to account for residual discrepancies between the treated units and their synthetic counterparts, which may persist even after matching on pre-treatment characteristics. While the standard synthetic control approach constructs a counterfactual by selecting a convex combination of control units, an imperfect pre-treatment fit can lead to biased estimates of treatment effects. Bias correction techniques—implemented via regression-based methods like ordinary least squares (OLS), Lasso, Ridge, or Elastic Net—aim to reduce this bias by adjusting the synthetic control weights so that the pre-treatment outcomes of the synthetic control more closely align with those of the treated units. For further details, see Ben-Michael et al. (2021).

model is:

$$\begin{aligned} Y_{it} = \lambda_i + \delta_t + \sum_{k \neq -15} \beta_k \left(\mathbb{1}\{t - t^* = k\} \times \mathbb{1}\{D_i = 1\} \right) \\ + \sum_{k \neq -15} \varphi_k \left(\mathbb{1}\{t - t^* = k\} \times \mathbb{1}\{D_i = 1\} \times \mathbb{1}\{l_i = A\} \right) + \Psi_{it} + \epsilon_{it} \end{aligned} \tag{5}$$

Where the vector Ψ_{it} includes the full set of two-way interaction terms. The φ_k coefficients capture whether—and by how much—regulatory impacts differ in counties with high fertilizer intensity.

By disentangling the effects across fertilizer intensity levels, this approach offers insights into the potential transmission channels through which $PM_{2.5}$ regulations may influence farmland values. If farmland values in high-intensity fertilizer counties exhibit a more pronounced response, it would imply that the policy is, at least in part, transmitted through direct changes in agricultural practices—potentially by altering fertilizer application decisions or related agronomic inputs. Conversely, if the differences in farmland value responses between high- and low-intensity counties are negligible or statistically insignificant, it would suggest that the regulation's effect is primarily channeled through broader environmental or economic shifts.

To better isolate the direct regulatory effects on agriculture, we use the USDA Economic Research Service's 2003 Urban Influence Code (UIC) classification to test our DDD specification in Equation (5) on a subsample that excludes counties in large metropolitan areas.[25] The UIC classification assigns each U.S. county a code from 1 to 12 based on its status, size, and proximity to metro and rural areas. We exclude counties located in large metropolitan areas (i.e., those with 1 million or more residents) to focus on a subsample with reduced influence from major urban centers. This stratification aims to isolate the regulatory effects on farmland values in regions where agriculture is more prominent and local land markets are less affected by urban development pressures. In large metropolitan counties, farmland values may be influenced by non-agricultural factors such as urban sprawl and industrial activities, which could mask the direct impact of the regulation on agricultural land values. By contrast, focusing on rural and smaller urban areas allows us to more clearly observe the policy's effects in predominantly agricultural settings with minimal interference from major urban dynamics.

Distributional impacts

The economic impact of stricter air quality regulations can differ substantially across the distribution of farmland values. As discussed in Section 2, lower-valued farmland may bear disproportionate regulatory costs or suffer from productivity declines, whereas higher-valued farmland may absorb these costs more easily or even benefit from improved environmental conditions. Traditional mean-based DiD methods fail to capture such distributional effects, potentially masking important heterogeneities in the regulation's impacts.[26]

Advances in quantile regression offer a way to assess how $PM_{2.5}$ NAAQS affect farmland values across the entire distribution (Firpo et al., 2009). In particular, Unconditional Quantile Regression (UQR)—as proposed by Firpo et al. (2009)—uses the Recentered Influence Function (RIF) to approximate the marginal effect that small shifts in the distribution of explanatory variables have on each unconditional quantile of the outcome variable. Unlike Conditional Quantile Regression (CQR), which focuses on the relationship between independent variables and the dependent variable across conditional quantiles (i.e., after conditioning on specific covariates), UQR estimates treatment effects across the entire unconditional distribution of the outcome variable without conditioning on covariates (Rios-Avila and Maroto, 2024). Recent studies (Currie et al., 2023; Sager and Singer, 2025) have extended the application of UQR within a DiD framework, often referred to as RIF-OLS DiD, to explore the policy impacts at various quantiles.

The RIF transforms the original outcome variable, Y_{it}, in a way that enables the estimation of treatment

[25] We use the 2003 Urban Influence Code (UIC) for the pre-policy intervention period. The dataset is available at: `https://www.ers.usda.gov/data-products/urban-influence-codes`.

[26] For instance, if regulatory costs or productivity losses are concentrated among lower-value farmland, traditional DiD estimates may understate the economic burden on more vulnerable agricultural properties. Conversely, if higher-value farmland benefits from improved environmental quality, standard analyses may fail to capture these positive externalities.

effects at different points in the outcome distribution. For a given quantile τ, the RIF is given by:

$$\text{RIF}(Y_{it}; Q_\tau) = Q_\tau + \frac{\tau - \mathbb{1}\{Y_{it} \leq Q_\tau\}}{f_Y Q_\tau} \tag{6}$$

where Q_τ is the τ-th quantile of Y_{it}, $\mathbb{1}\{\}$ is an indicator function that equals 1 if $Y_i t$ is less than or equal to Q_τ and 0 otherwise, and $f_Y Q_\tau$ denotes the density of $Y_i t$ at Q_τ.

Incorporating the RIF into our DiD framework enables us to analyze how the distribution of farmland values changes over time because counties are subject to air quality regulations. The extended event-study DiD specification is:

$$\text{RIF}(Y_{it}; Q_\tau) = \lambda_i^\tau + \delta_t^\tau + \sum_{k \neq -15} \beta_k^\tau \left[\mathbb{1}\{t - t^* = k\} \times \mathbb{1}\{D_i = 1\}\right] + \epsilon_{it}^\tau \tag{7}$$

where RIF$(Y_{it}; Q_\tau)$ is the Recentered Influence Function at the τ-th quantile of the outcome variable Y_{it}. β_k^τ represents the quantile treatment effect for the treated (QTT) at the τ-th quantile of the outcome distribution (farmland values). λ_i^τ, δ_t^τ, and ϵ_{it}^τ correspond to county fixed effects, year fixed effects, and the error term at the τ-th quantile, respectively.

Although UQR-based DiD provides a powerful way to examine distributional impacts, it can introduce bias if the treatment variable is purely categorical (e.g., "non-attainment" vs. "attainment"). UQR offers a linear approximation of how unconditional quantiles shift in response to small changes in covariate distributions, but these approximations may be invalid for large shifts. To address this limitation, recent studies have recommended using IPW or other reweighting strategies (Rothe, 2010; Donald and Hsu, 2014; Firpo and Pinto, 2016; Firpo et al., 2018; Avila Uribe, 2023). By re-weighting observations according to the probability of treatment, IPW ensures that the RIF-OLS estimator accurately captures the causal effect of the policy across the full outcome distribution. After re-weighting, distributional treatment effects are computed by comparing the relevant outcome statistics between treated and untreated groups.

Formally, if we assume that treatment assignment is unconfounded (i.e., independent of potential outcomes, conditional on observed confounders $\mathbf{X}_{i,0}$) and that there is sufficient overlap in the covariate distributions between treated and control groups, the unconditional treatment effect on a given distributional statistic $v(\cdot)$ can be expressed as:

$$\Delta v = v_1 - v_0 = v(F_{Y_1}) - v(F_{Y_0}) \tag{8}$$

Where F_{Y_1} and F_{Y_0} represent the outcome distributions under "non-attainment" and "attainment" of the $PM_{2.5}$ NAAQS, respectively.

A key advantage of this approach is that, if $v(\cdot)$ is chosen to be the mean (i.e., average farmland values), the resulting estimates coincide with the standard ATT from the baseline DiD model. This equivalence allows direct comparisons between mean-based estimates and QTT at any quantile τ and for any period k.

In practice, we implement this approach within the event-study specification using Hajek-stabilized IPW and an improved doubly robust estimator. We further assess the robustness of the QTT estimates by testing alternative propensity score models, as we did when estimating the ATT.

5 Results

5.1 Baseline Results

Table 2 displays the event-study coefficients (β_k) from our benchmark specification in Equation (1), where the dependent variable is the estimated farmland value for counties with at least 5% soybean or corn acreage. In the Census years before the enforcement of the regulation, trends in farmland values between regulated and unregulated counties are indistinguishable from zero—the χ^2 test fails to reject the null hypothesis that all pre-treatment ATT are equal to zero under both the Stabilized IPW and Doubly Robust specifications. In contrast, following enforcement, our estimates indicate that $PM_{2.5}$ NAAQS produced a negative capitalization effect, with farmland values in non-attainment counties declining on average by 8.80%–8.94% attributable to the regulation (calculated as $100 \times (e^{\text{ATT (post)}} - 1)$). This decline persisted throughout the post-regulation period, suggesting a sustained regulatory impact. Notably, our estimates are comparable to those of Sanders and Barreca (2022), who report a 7% decrease in agricultural land values for corn and soybeans following the 1995 Acid Rain Program.

Table 2: The impact of stricter $PM_{2.5}$ regulations on farmland values

	Stabilized IPW				Doubly Robust			
	Coeff	se	95% CI		Coeff	se	95% CI	
			lower	upper			lower	upper
ATT (pre)	0.0039	0.0100	-0.0158	0.0235	0.0009	0.0095	-0.0176	0.0195
ATT (post)	-0.0921***	0.0173	-0.1261	-0.0582	-0.0937***	0.0147	-0.1226	-0.0648
$t-10$	0.0049	0.0187	-0.0319	0.0416	0.0026	0.0185	-0.0336	0.0388
$t-5$	0.0029	0.0180	-0.0324	0.0381	-0.0007	0.0170	-0.0340	0.0325
t^*	-0.0805***	0.0150	-0.1099	-0.0511	-0.0825***	0.0141	-0.1102	-0.0549
$t+5$	-0.1025***	0.0217	-0.1451	-0.0599	-0.1083***	0.0189	-0.1453	-0.0713
$t+10$	-0.0935***	0.0241	-0.1406	-0.0463	-0.0904***	0.0207	-0.1310	-0.0498
Pre-trend (χ^2)	0.1501				0.0199			
p-value	0.9277				0.9901			
N	9078				9078			

Note: This table presents the regression coefficients from estimates of Equation (1). t^* stands for the treatment date, 2012. The specification is estimated using Stabilized Inverse Probability Weighting (IPW) and Doubly Robust estimators. Bootstrapped standard errors (se) with 10,000 repetitions are clustered by counties. *** indicates a significance level of 1%. The pretrend test reports the χ^2 statistic and the p-value for a joint test of the null hypothesis that all pretreatment average treatment effects (ATT) are equal to zero.

The capitalized cost in "non-attainment" counties can be approximated by multiplying the average percent decline in farmland values by the average land value per acre over the post-treatment period ($7,528.20 in 2022 prices). In the absence of regulation, the counterfactual farmland values per acre in "non-attainment" counties would have been approximately $662.48 ($673.02) higher in 2022 prices, according to the IPW (DR) estimates. As a result, our estimates show that the $PM_{2.5}$ NAAQS have narrowed the land price gap between "non-attainment" and "attainment" counties. The difference in farmland values over the post-treatment period decreases from a predicted difference of approximately $3,225.75 ($3,236.28) per acre in 2022 prices, according to the IPW (DR) estimates, to an observed difference of around $2,737.90 per acre. These figures represent a reduction of about 17.8% (18.2%) in the gap, which can be attributed to the impact of stricter air quality regulations.

We first explore the robustness of our findings by testing some variations in our estimates: (i) dropping the six newly regulated, non-overlapping counties designated in 2009 under the 2006 $PM_{2.5}$ NAAQS; and (ii) using an alternative specification of the propensity score model, which excludes covariates with lower imbalance adjustment (the Wind Speed and the GDD variables).[27] Results, presented in Tables C.2 and C.3 in Appendix C, are qualitatively unchanged.

We conduct a placebo test by randomly assigning treatment status to U.S. counties—ensuring that no genuine treatment is applied. This test serves two key purposes. First, it acts as a falsification exercise: if our DiD

[27] Diagnostic statistics for the balance check are provided in Table C.1 in Appendix C. Any confounder with a standardized mean difference (SMD) outside the range of ±0.1, either before or after applying IPW, is excluded from the propensity score model. Equation (1) is then re-estimated using this refined set of confounders.

estimator is valid and the observed treatment effects are truly driven by the intervention, then randomly assigning treatment should yield no significant effects. Second, it reinforces the parallel trends assumption by demonstrating that significant effects are unlikely to be an artifact of random variation. To implement this test, we generate a uniformly distributed random variable for each county and classify counties as "treated" if their random draw falls below a predetermined threshold (λ). We conduct the analysis using two thresholds: $\lambda = 0.5$ (assigning treatment to 50% of counties and $\lambda = 0.15$ (mimicking the actual proportion of non-attainment counties). Additionally, we perform a permutation test with 5,000 random assignments to ensure that our findings are not driven by a specific randomization. As shown in Table C.4 and Figure C.1 in Appendix C, when treatment is randomly assigned, the estimated effects vanish, lending further credibility to our identification strategy.

We also perform a pre-treatment falsification test by assigning the treatment status as if the intervention had occurred in 2007, just before the effective enforcement of the 1997 $PM_{2.5}$ NAAQS, i.e., the 2008 State Implementation Plan (SIP). Under the DiD framework, if the parallel trends assumption holds and there are no anticipation effects, we should observe no significant treatment effect in a pre-intervention period. The results shown in Figure C.2 in appendix C indicate that both IPW and DR specifications display similar pre-treatment trends and significant post-treatment negative effects on farmland values, irrespective of the chosen treatment year. This consistency across both treatment dates reinforces the robustness of our findings, indicating that the observed decreases in farmland values are indeed attributable to the implementation of more stringent regulations and not to anticipatory actions by farmers or state governments.

5.2 Addressing additional concerns

Violations of conditional parallel trends

Although event-study pre-treatment coefficients and the corresponding statistical test indicate no significant pre-trend, it is important to acknowledge that tests for pre-trends often suffer from low power. Consequently, small yet undetected departures from the conditional parallel trends assumption (CPTA) may persist, potentially biasing our causal inference.

To reinforce the plausibility of the CPTA, we assess the sensitivity of our findings to potential violations of this assumption by following the approach outlined by Rambachan and Roth (2023). Instead of assuming that trends must be exactly parallel, we allow for a bounded amount of trend divergence, introducing flexibility in how trends can differ before and after the treatment. The bounds for this divergence are calibrated using farmland values from the pre-treatment period and then applied to the post-treatment period. The flexibility is represented by a sensitivity parameter denoted as $\bar{M}$, which defines the maximum allowable deviation between the pre-treatment trends of the treated and control groups.[28] This method provides a range (or interval) of possible values for the ATT. Estimates for the treatment effect remain valid as long as the divergence in trends stays within the bounds set by $\bar{M}$.[29]

Figure 4 displays the results from our implementation of this sensitivity test. When $\bar{M} = 0$, the estimates

[28]For example, setting $\bar{M}$ to 1 assumes that post-treatment violations are, at most, the same size as pre-treatment violations, while $\bar{M} = 2$ allows the post-treatment violations to be up to twice the size of the pre-treatment violations.

[29]Recall that in the estimated model from Equation (1), we assume that the untreated outcome trends of both treated and control units would have followed the same path in the absence of treatment. The observed coefficient vector,

$$\beta = (\beta_{t-10}, \ldots, \beta_{t+10})$$

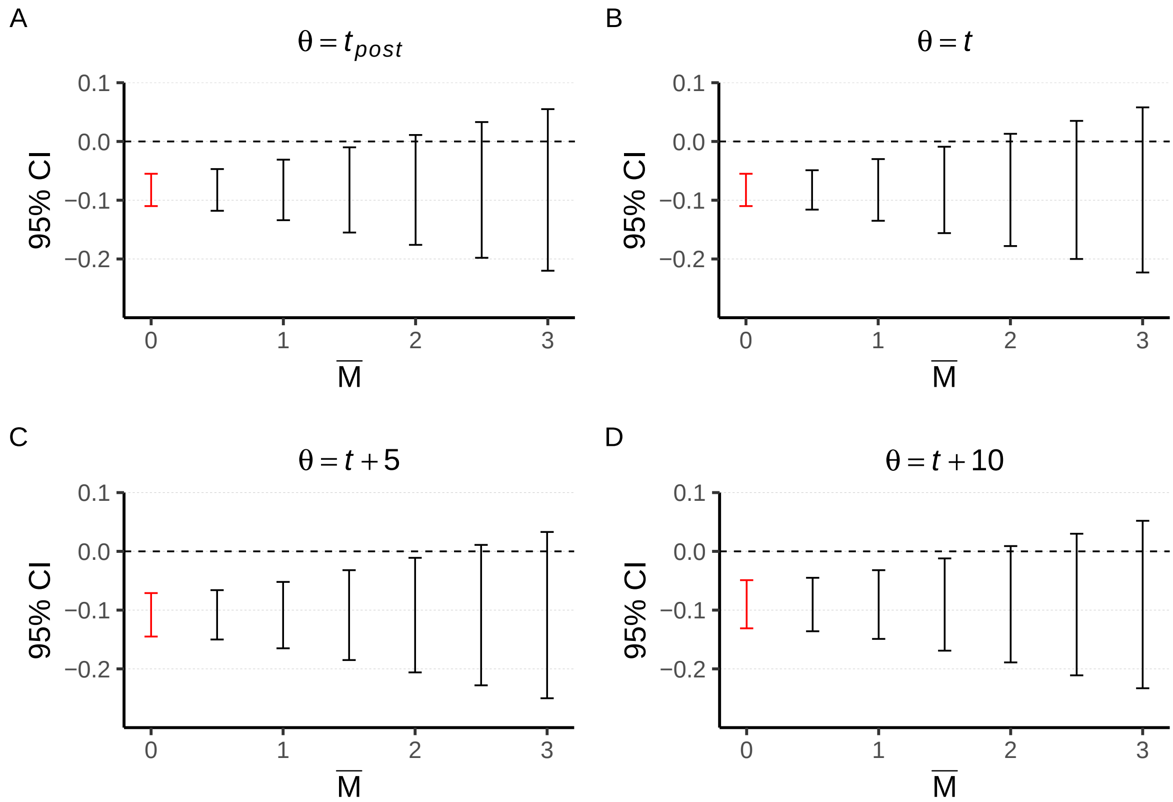
can be decomposed as:

$$\beta = \underbrace{\begin{bmatrix} \zeta_{pre} \\ \zeta_{post} \end{bmatrix}}_{\zeta} + \underbrace{\begin{bmatrix} \eta_{pre} \\ \eta_{post} \end{bmatrix}}_{\eta}$$

where ζ represents the dynamic causal effects (with the assumption $\zeta_{pre} = 0$), and η captures the latent differences in trends that would have occurred in the absence of treatment. Under strict CPTA, ($\eta = 0$), which implies that ($\beta = \zeta$); thus, the treatment effect—or any linear combination (θ) of the post-treatment effects—is point-identified.

align with those of the baseline under the strict CPTA, yielding the original (point-identified) confidence interval for any linear combination θ of the post-treatment effects. As $\bar{M}$ increases, the corresponding robust confidence intervals widen, reflecting the increased uncertainty regarding the counterfactual trend. The "breakdown value", i.e., the smallest value of $\bar{M}$ for which the robust confidence interval first includes zero, indicates that our findings would only be overturned if post-treatment deviations were at least twice (or, for $t+5$, 2.5 times) the maximum deviation observed in the pre-treatment period. Hence, even moderate departures from perfect trend parallelism would not be sufficient to nullify our estimated treatment effects.

Figure 4: Parallel trends sensitivity analysis



Note: Each panel shows the 95% confidence intervals (CIs) for different post-treatment effect estimates (θ) as a function of the sensitivity parameter $\bar{M}$. The red error bars correspond to the baseline estimates under strict CPTA ($\bar{M} = 0$).

Spatial dependence

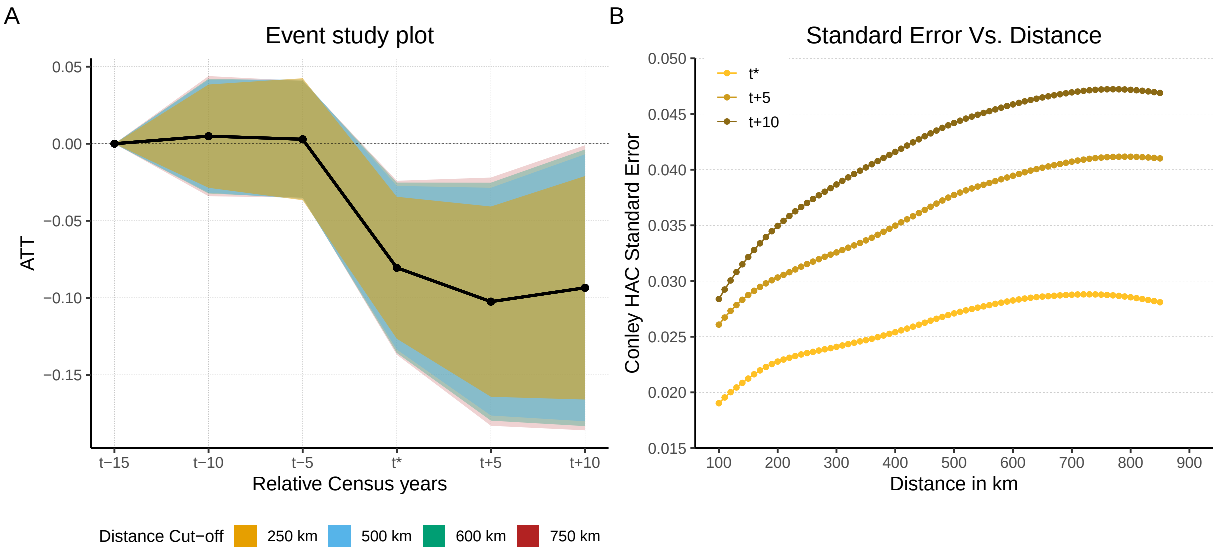
Another concern is the presence of spatial dependence, since the impact of the regulation on farmland values in one county can be influenced by the values in neighboring counties. If such correlations are ignored, standard errors may be underestimated potentially inflating the statistical significance of our estimates. To obtain correct inference in the presence of spatial dependence at a broader administrative level, we re-estimate our model with standard errors clustered at the Commuting Zone (CZ) level. As shown in Table C.5, the post-treatment coefficients remain highly significant under this alternative clustering scheme, reinforcing the robustness of our findings.

Nevertheless, spatial correlation can extend beyond the boundaries of these administrative zones, potentially leading to downward-biased standard errors if such extended dependence is not fully captured after clustering at the CZ level. To address this, we compute Conley (HAC) spatial standard errors (Conley, 1999), which explicitly model spatial dependence by allowing error terms to correlate over geographic distance. This approach assumes that the influence of one county's farmland values on neighboring counties diminishes with distance, using a specified cutoff beyond which correlations are considered negligible.

We first calculate each county's geographic and population centroids (in decimal degrees, using the WGS 1984 coordinate system). We then compute Haversine distances between counties, which measure the straight-line distance between two points on the Earth's surface. These distances are then used in the application of a Bartlett kernel, which assigns more weight to errors between counties that are geographically closer. We consider several distance thresholds (e.g., 250 km, 500 km, 600 km, 750 km), each representing a maximum distance beyond which error correlations are assumed negligible. We incrementally increase these distance cutoffs until the standard errors stabilize, indicating that the spatial correlation structure is adequately captured.

Figure 5 shows that the 95% confidence intervals calculated from Conley standard errors indicate significant post-regulation ATT estimates.

Figure 5: Stabilized IPW-DiD results with Conley (HAC) standard errors



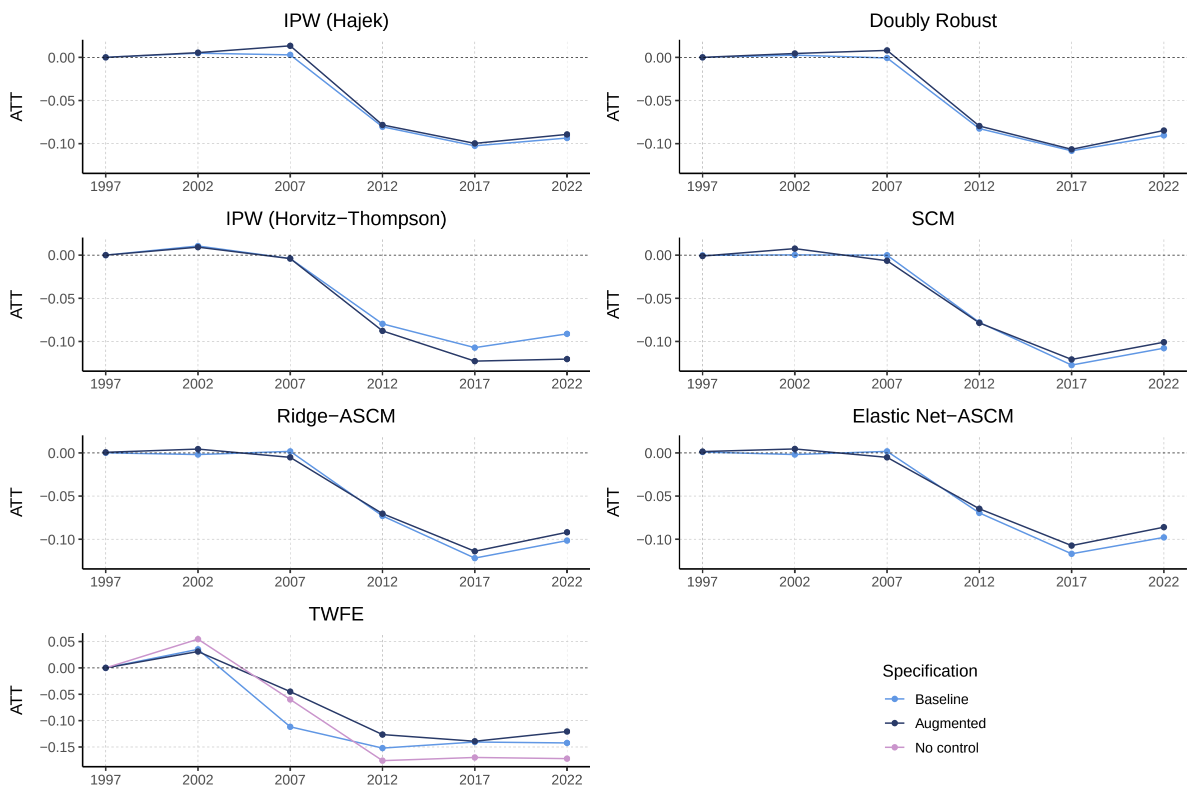
Note: Panel A illustrates the 95% confidence intervals (shaded regions) for the ATT estimates based on the Stabilized IPW-DiD method with Conley HAC standard errors. Panel B shows the relationship between standard error and distance for three different post-treatment periods (t, $t+5$, and $t+10$). The varying distance cut-offs (250 km, 500 km, 600 km, and 750 km) reflect the maximum distance up to which spatial correlations are assumed to influence the standard errors.

A higher distance cut-off leads to wider intervals because more distant county-pairs are potentially correlated. The farther we extend the spatial correlation allowance, the more we guard against understated standard errors due to overlooked long-distance correlations, thus creating larger error bars. As shown in Panel B, the standard errors stabilize once the distance cutoff becomes sufficiently large (around 750 km and beyond), suggesting that by this point, most of the relevant spatial correlation has been captured. This plateau indicates that at 750 km, the estimator has captured virtually all meaningful spatial correlation in the data. Increasing the distance threshold further (up to 900 km or more) does not significantly increase the uncertainty, providing confidence that the regulation effect is not driven by underestimating standard errors.

Sensitivity analyses

We conduct sensitivity analyses by running multiple, complementary estimation strategies and alternative specifications. The graphs (event-study plots) in Figure 6 show nearly identical average treatment effects on the treated (ATT) estimates from both stabilized (Hájek) IPW and the Horvitz–Thompson IPW estimator, suggesting that the weighting procedure is stable and not overly sensitive to extreme weights. The doubly robust (DR) estimator proposed by Sant'Anna and Zhao (2020), which combines propensity score weighting with an outcome regression, likewise yields an ATT estimate that closely aligns with the IPW results. This consistency indicates robustness, as the DR estimator remains valid if either the propensity score or the outcome model is correctly specified, thereby minimizing the risk of systematic bias arising from model misspecification. Statistically, if the propensity score model were severely misspecified, the IPW estimator would be biased, and the DR estimator would likely diverge.

Figure 6: Robustness and sensitivity to alternative estimators and baseline specifications



Note: For the Inverse Probability Weighting (IPW), Doubly Robust, Synthetic Control Method (SCM), and Augmented Synthetic Control Method (ASCM) estimations, the baseline specification includes all confounders without interaction effects in the propensity score model. The augmented specification incorporates interactions among chemical and fertilizer expenditures, irrigation and drought frequency, and county population density and poverty in the propensity score model. For the Two-Way Fixed Effects (TWFE) estimates, both the baseline and augmented specifications include confounders as linear controls in the Difference-in-Differences (DiD) regression. The "No control" specification refers to the canonical TWFE model, which includes county and census year fixed effects as controls. This specification corresponds to our baseline specification in Equation (1), but without applying a weighting scheme.

The ATT estimates derived from the propensity score methods (IPW and DR) closely match those obtained from synthetic control (SC) and augmented SC methods (ASCM), increasing confidence that neither approach is idiosyncratically biased. Indeed, the IPW/DR estimate closely mirrors that produced by a conventional multiple-treated-unit SC analysis. Both Ridge and Elastic Net ASCM, which incorporate covariates and trends in a penalized regression framework, yield ATT estimates consistent with the IPW/DR estimates. This coherence provides strong evidence that both methodologies capture the same underlying causal effect. It also implies that two fundamentally different approaches—one based on modeling treatment assignment via propensity scores and the other on matching outcome trajectories—converge on the same effect size and trajectory pattern. The alignment of propensity score methods with SCM/ASCM results further reinforces confidence in the covariates selected and the functional form of the propensity score model.

As noted by Meyer (1995) and Abadie (2005), the TWFE estimator can yield biased ATT estimates when treatment effects are heterogeneous. Our results show that TWFE estimates differ substantially not only from those obtained via the IPW/DR approaches but also from SCM and ASCM estimates (using both ridge-based and elastic net-based regression methods). This divergence suggests that the underlying assumption of parallel trends is violated. For example, while our baseline IPW/DR estimates indicate an

8.80%/8.94% decline in farmland values post-regulation, a TWFE specification incorporating only unit and time fixed effects yields an average ATT of -15.85%. Incorporating additional covariates—particularly through models that include interactions among key variables—reduces the downward bias; however, violations of the parallel trends assumption persist, with the adjusted TWFE ATT averaging approximately -12.10%

Finally, even when we augment the propensity score model to account for potential non-linearities or interactions among key variables, the estimated ATT remains largely unchanged. This result suggests that the baseline specification effectively captures the primary determinants of treatment assignment and outcomes, and that additional model complexity contributes little to explanatory power or inference.

5.3 Treatment Heterogeneous Effect

Evidence from the triple-difference approach

Table 3 presents the stabilized IPW-triple difference (DDD) results based on estimating Equation (5).

Table 3: Stabilized IPW-Triple Differences (DDD) estimates

	Incl. counties in large metro areas		Excl. counties in large metro areas	
	ATT (β_k)	φ_k	ATT (β_k)	φ_k
$t-10$	0.0012	0.0047	0.0046	-0.0130
$t-5$	0.0211	0.0353	0.0121	-0.0251
t^*	-0.0807***	-0.0010	-0.0745**	-0.0301
$t+5$	-0.0682**	-0.0537*	-0.0234	-0.0992***
$t+10$	-0.0369	-0.0761***	0.0104	-0.1268***
Pre-trend (χ^2)	0.4921	0.6123	1.0701	0.3448
p-value	0.1121	0.8943	0.3803	0.6849
N	8,960		7,748	

Note: This table reports the estimated Average Treatment Effects on the Treated (ATT) from the triple difference (DDD) specification:

$Y_{it} = \lambda_i + \delta_t + \sum_{k\neq -15} \beta_k \left(1\{t - t^* = k\} \times 1\{D_i = 1\}\right) + \sum_{k\neq -15} \varphi_k \left(1\{t - t^* = k\} \times 1\{D_i = 1\} \times 1\{l_i = 1\}\right) + \Psi_{it} + \epsilon_{it}$.

Estimates are presented separately for counties including and excluding large metropolitan areas. The specification is estimated using Stabilized Inverse Probability Weighting (IPW). Bootstrapped standard errors with 10,000 repetitions (se) are clustered by counties. ***, **, and * indicate significance levels of 1%, 5%, and 10%, respectively. The pretrend test reports the χ^2 statistic and the p-value for a joint test of the null hypothesis that all pretreatment average treatment effects (ATT) are equal to zero.

At the time of the regulatory intervention (t^*), farmland values in "non-attainment" counties with relatively low fertilizer use show a decline comparable to our baseline DiD estimates from Equation (1), as indicated by the coefficient β in the triple-difference (DDD) specification. Because β captures the baseline treatment effect for counties below the high-intensity fertilizer threshold, its significance at t^* indicates that simply being subject to stricter $\mathrm{PM}_{2.5}$ standards is sufficient to generate a short-term adjustment in farmland values—even when fertilizer intensity is relatively modest.

As one moves to subsequent census years $(t + 5)$ and $(t + 10)$, however, this effect diminishes or becomes statistically insignificant for these lower-intensity counties. This pattern suggests that the initial adjustment in farmland values is not strongly persistent in areas where fertilizer reliance is relatively low. In such places, producers may only need modest technological adaptations or changes in agricultural practices to comply with $\mathrm{PM}_{2.5}$ regulations, and farmland values appear to converge toward near-baseline levels over time. From a policy standpoint, this indicates that stricter air quality standards may be effectively internalized without imposing a lasting economic burden in lower-intensity regions.

In contrast, the negative and significant estimates for the coefficient φ at $(t+5)$ and $(t+10)$ suggest that high-fertilizer-use counties undergo a more pronounced and enduring adjustment in farmland values. Conceptually, the coefficient φ reflects the incremental impact for counties that are both subject to "non-attainment" regulations and above the threshold for high fertilizer use—indicating additional compliance costs or necessary practice changes. In these more fertilizer-intensive areas, the compliance costs of $\mathrm{PM}_{2.5}$ regulations appear to be effectively priced into land values over a longer horizon. This effect is especially evident in counties outside large metropolitan areas, where the relatively lower presence of pollution sources, compared to metropolitan areas, can amplify the direct effects that air pollution regulations have on agricultural practices.

Finally, pre-trend tests (χ^2) show no significant differences in pre-treatment trends, supporting the validity of the identification strategy.

We perform several robustness checks to confirm that the results are not affected by the estimator or by the specification of the propensity score model. In both cases, the results are unchanged.[30]

Evidence from quantile treatment effects

Our previous findings indicate an average decline in farmland values between 8.80% and 8.94% from the IPW and DR estimates, respectively, in "non-attainment" counties attributable to the enforcement of the $\mathrm{PM}_{2.5}$ NAAQS. However, this average effect provides only limited insight into the heterogeneous impacts across the farmland value distribution. In particular, one may question whether the average treatment effect accurately reflects the overall impact or whether it conceals significant heterogeneity driven by more pronounced effects in specific segments of the farmland value distribution.

To investigate this potential distributional impact, we employ unconditional quantile regression estimates. Our results suggest that environmental regulation has exerted a markedly stronger negative effect in counties with lower-valued farmland compared to counties with higher-valued farmland.

Figure 7 displays the Quantile Treatment Effect on the Treated (QTT) estimates obtained by stabilized IPW RIF-OLS regression in several percentiles (τ) ranging from the 10th to the 90th percentile. The analysis reveals that the regulatory effect on farmland values intensifies as the Census years move further from the treatment date and is predominantly concentrated among counties with lower average farmland values.

Panel A of Figure 7 presents the QTT estimates, which measure the impact of the $\mathrm{PM}_{2.5}$ NAAQS regulations at specific quantiles (τ) of the farmland value distribution in "non-attainment" counties. The estimated coefficients should be interpreted as the impact that the enforcement of the regulation has on the entire distribution of farmland values, holding constant the distribution of other county characteristics. In other words, these QTT estimates illustrate how the farmland value distribution would differ if all counties were subject to the $\mathrm{PM}_{2.5}$ NAAQS regulations, compared to a scenario where no counties were subject to these regulations.

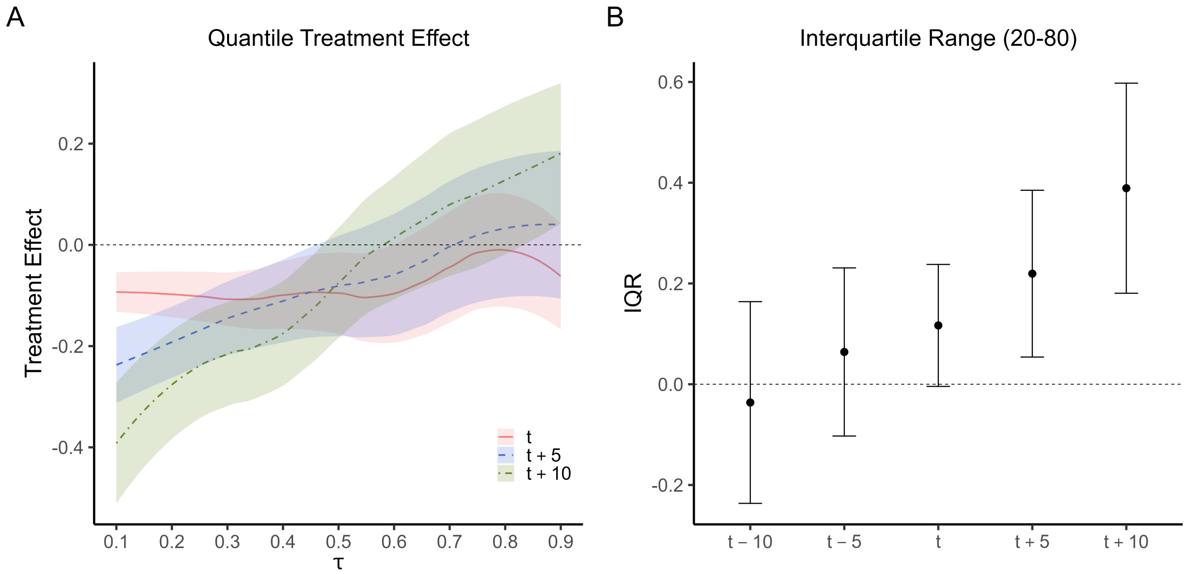
In the treatment year t, the Quantile Treatment Effect (QTT), indicated by the red solid line, shows that the overall negative mean effect is largely driven by counties with farmland values below the 60th percentile. While the average QTT across all quantiles is –7.35%, the estimates for the lower part of the distribution are consistently lower, around –11.5%, and are statistically significant. Counties at the lower end of the farmland value distribution would therefore experience an 11.5% reduction in farmland values under the regulated scenario, compared to a scenario where no counties are regulated. The adverse impact of the regulation is particularly pronounced in counties with lower farmland values, illustrating the heterogeneous effects of the $\mathrm{PM}_{2.5}$ NAAQS across the farmland value distribution. Beyond the 60th percentile, the negative effects become statistically insignificant and gradually converge to zero.

At $t+5$, the impact remains largely concentrated in counties with lower farmland values, as shown by the blue solid line. The average QTT across the entire distribution is –7.80%, while the effect in the 25th percentile is –16.5%. Effects become statistically insignificant for values of τ exceeding the 48th percentile.

[30] The detailed results are available upon request to avoid an excessively lengthy appendix.

This pattern strengthens at $t + 10$, where the average QTT between the 10th and 90th percentiles is –7.46%, showing larger effects in the lower part of the distribution. At the 25th percentile, the estimated effect increases to –21.5%, while statistically insignificant impacts remain above the 48th percentile.

Figure 7: The distributional impact of stricter $PM_{2.5}$ regulations on farmland values



Note: Panel A displays the quantile treatment effect on the treated (QTT) across different quantiles (τ) ranging from 0.1 to 0.9. The QTT is visually represented for three time periods: the treatment date (t, red solid line), five years after the treatment ($t + 5$, blue dashed line), and ten years after the treatment ($t + 10$, green dotted line). The shaded areas around the lines represent 95% confidence intervals. Panel B illustrates the interquartile range (IQR) for the treatment effect across the different pre- and post-treatment dates.

Panel B of Figure 7 further illustrates this widening disparity by depicting the evolution of the 20th–80th interquartile range (IQR) for the treatment effect across different pre- and post-treatment dates. In the pre-treatment years, the IQR is close to zero, indicating no substantial difference in the variability or distribution of farmland values before the regulation's enforcement. However, from the treatment year onward, the IQR begins to increase, reflecting growing disparities in the impact of the regulation. By ten years post-treatment, the IQR is notably larger, indicating that the gap between "non-attainment" counties in the lower and upper quantiles has widened. This widening gap corresponds with the findings from Panel A, where counties in lower quantiles continue to experience negative effects following stricter air quality regulations while counties in upper quantiles appear not to be affected.

We also assess the robustness of our results across various estimators and model specifications. The results for the treatment date, as well as five and ten years post-treatment, reported in Figure C.3 in Appendix C, clearly demonstrate that the findings are consistent across these robustness checks.

Together, these results highlight the distributional consequences of complying with stricter $PM_{2.5}$ regulations. They suggest that while "non-attainment" counties with lower-value farmlands experience sustained negative impacts, counties with higher-value farmlands are not affected and can eventually benefit from the regulations, as evidenced by the positive QTT in the upper quantiles ten years post-treatment.[31] These patterns are consistent with the expectation that counties with lower-value farmland may face greater challenges in covering compliance costs, while counties with higher-value farmland are likely to absorb these costs more easily. These findings also underscore that examining only the ATT can be misleading, as it overlooks the important distributional effects linked to the implementation of stricter air quality regulations.

[31] Firpo et al. (2009) caution that estimates from extreme quantiles should be interpreted carefully, as estimation uncertainty increases in these regions.

6 Conclusion

This article investigates the extent to which the 1997 National Ambient Air Quality Standards (NAAQS) for $PM_{2.5}$ were capitalized into farmland values in corn and soybean-producing counties in the United States from 1997 to 2022. We estimate, using a difference-in-differences event-study design, the treatment effect of being designated as "non-attainment" on farmland values during both the periods prior to and following the enforcement of the regulation.

We estimate an 8.80%–8.94% decline in farmland values in "non-attainment" counties in response to the enforcement of $PM_{2.5}$ standards, indicating that the economic costs associated with air quality regulations have been capitalized into farmland values in counties that did not initially meet the required standards. While farmland values in "non-attainment" counties were initially significantly higher, we show that the imposition of stricter air quality standards has contributed to reducing the gap in farmland values between the two groups over time.

When comparing counties with above-average fertilizer usage to those with less intensive practices, we find that the impact of air quality regulations on farmland values was not uniform and was possibly mediated by agricultural practices. Our estimates show that the enforcement of the $PM_{2.5}$ NAAQS results in significant declines in farmland values for counties with higher fertilizer usage five and ten years after the enforcement of air quality standards. In contrast, for counties with less intensive practices, the average treatment effect converges to zero over time. These findings suggest that the policy measures have effectively targeted regions with higher agricultural intensity, where fertilizer use serves as a key precursor to elevated $PM_{2.5}$ concentrations.

Our analysis also indicates that the enforcement of air quality regulations under the "non-attainment" designation has produced regressive distributional outcomes. Specifically, counties with lower baseline farmland values (i.e., those in the lower quantiles) experienced sustained reductions following the enforcement of the $PM_{2.5}$ NAAQS, whereas counties with farmland values above the median exhibit no statistically significant effect. These findings underscore that the economic consequences of the regulation are not uniformly distributed across counties and are likely influenced by underlying determinants—such as limited access to capital, scale economies, and sensitivity to compliance costs. Relying exclusively on average estimates such as the ATT can obscure these heterogeneous effects. While the ATT provides a useful summary of the overall impact, it masks significant variations across different segments of the distribution. Thus, incorporating distributional analyses is essential to capture the full spectrum of policy outcomes and to guide more targeted and effective interventions.

Our results highlight the difficulty of finding simple solutions to the problems of nonpoint pollution as agriculture and its compliance with air quality regulations. It is important for policymakers to be aware of the potential negative impact air quality standards may have on agriculture, particularly in environmentally sensitive regions such as corn and soybean-producing areas. Beyond the immediate environmental benefits, the observed decline in farmland values carries broader economic implications. Farmland is a key asset that influences credit access, investment decisions, and the overall financial stability of agricultural communities. When farmland values depreciate–particularly in regions where they are already low–the agricultural sector may face reduced collateral for loans, a diminished capacity to invest in technological upgrades or sustainable practices, and heightened financial vulnerability. These economic pressures can further exacerbate rural disparities and slow the modernization of agricultural operations.

Therefore, our results emphasize the need for efficient mechanisms to assist financially constrained counties in minimizing potential losses caused by increased regulatory demands, while also addressing the technical challenges involved in directly regulating agricultural emissions. Such measures could be essential for timely policy adjustments, ensuring that regulations remain equitable and do not inadvertently exacerbate disparities in farmland values.

Finally, further research is needed to better understand the results presented in this paper. In particular, it is important to note that these results are based on self-reported farmland values, which reflect farmers' perceptions of market conditions. However, we must acknowledge that self-reported farmland values serve only as a proxy for actual transaction prices. Future research could explore how closely these perceptions align with observed market trends, providing additional insights into the broader implications of regulatory interventions. Additionally, other factors driving the estimated decrease in farmland values in ”non-attainment” counties warrant further investigation. Addressing this question will require the collection of disaggregated data, which remains a task for future research.

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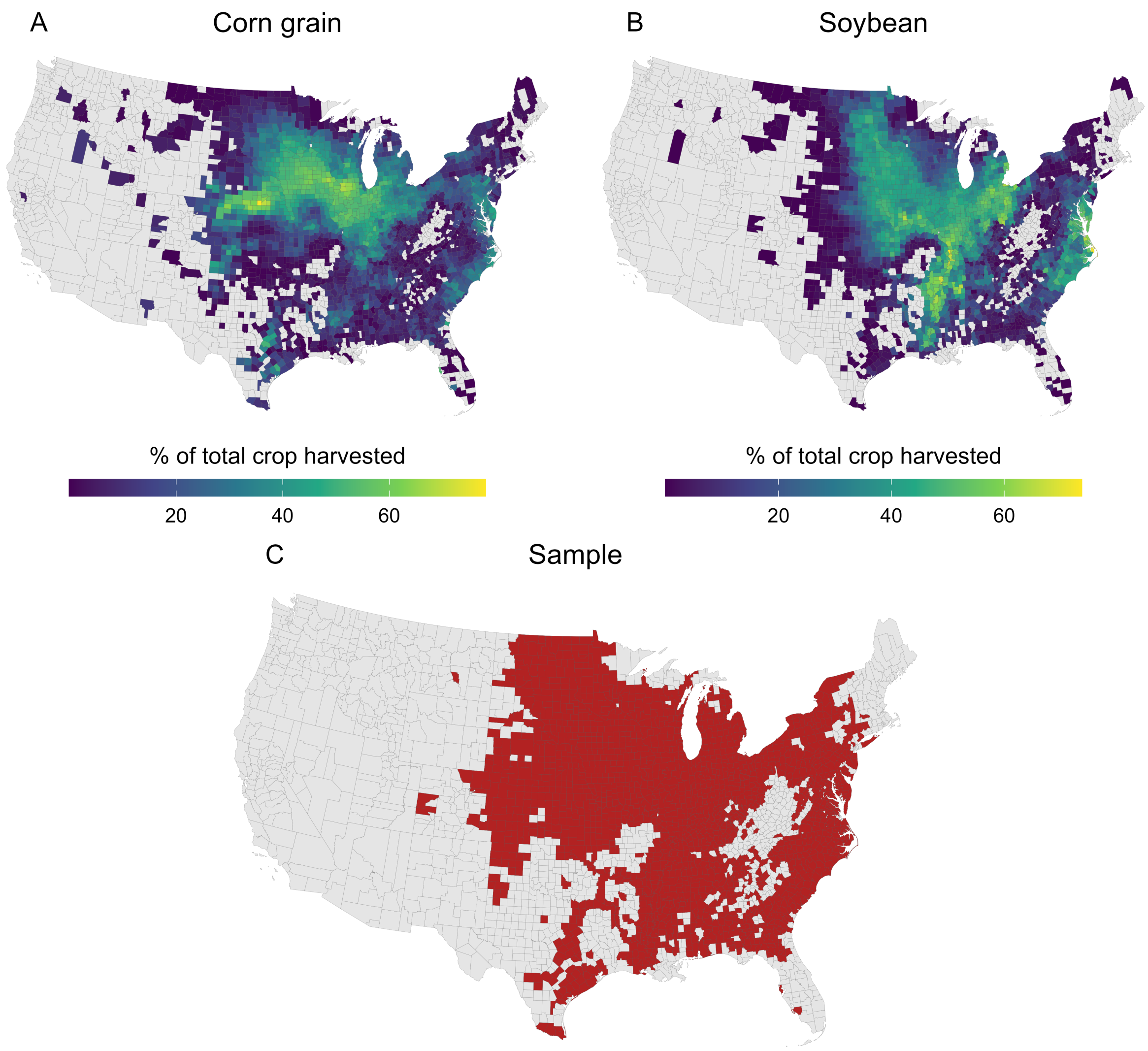
Appendices

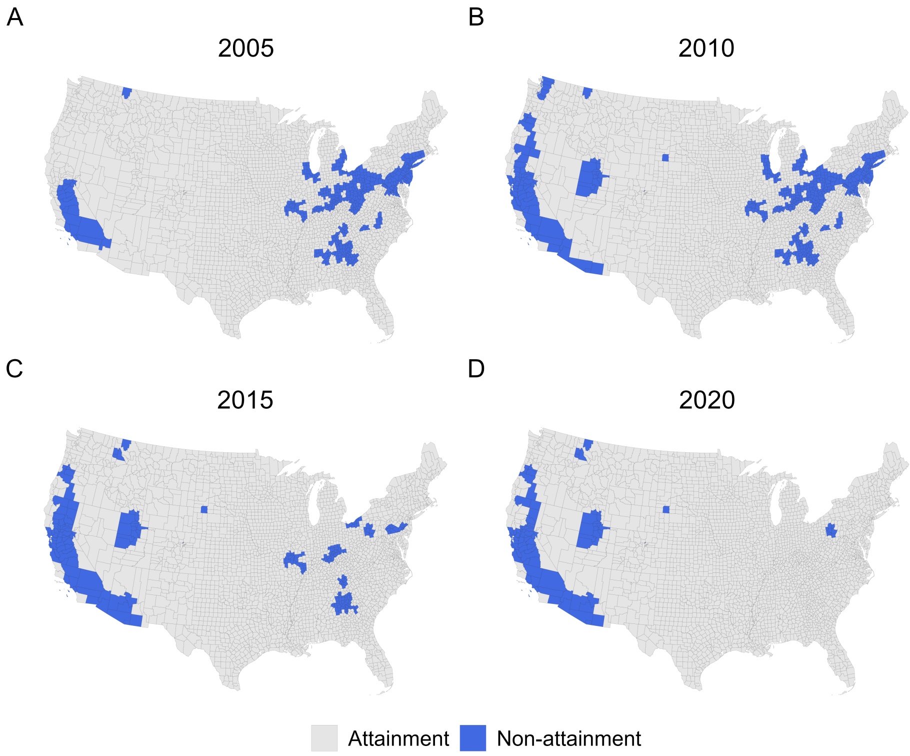
Appendix A - Data

Figure A.1: Geographical study area



Note: Maps A and B illustrate the average harvested acres of corn and soybeans, respectively, as a proportion of the total cropland harvested acres by county, averaged over the period from 1997 to 2022. Map C shows the delineation of counties that are major producers of corn and soybeans, including all counties where the proportion of land dedicated to corn and soybean cultivation exceeds 5%. Source: USDA-NASS Crop Data Layers, USDA Census of Agriculture.

Figure A.2: Counties' status for PM2.5 NAAQS



Note: The figure shows the evolution of "non-attainment" areas under the $PM_{2.5}$ NAAQS in the U.S. between 2005 and 2020. Counties classified as "non-attainment" (blue) represent regions where air quality does not meet federal standards, while "attainment" counties (grey) meet regulatory thresholds. Source: EPA Green Book.

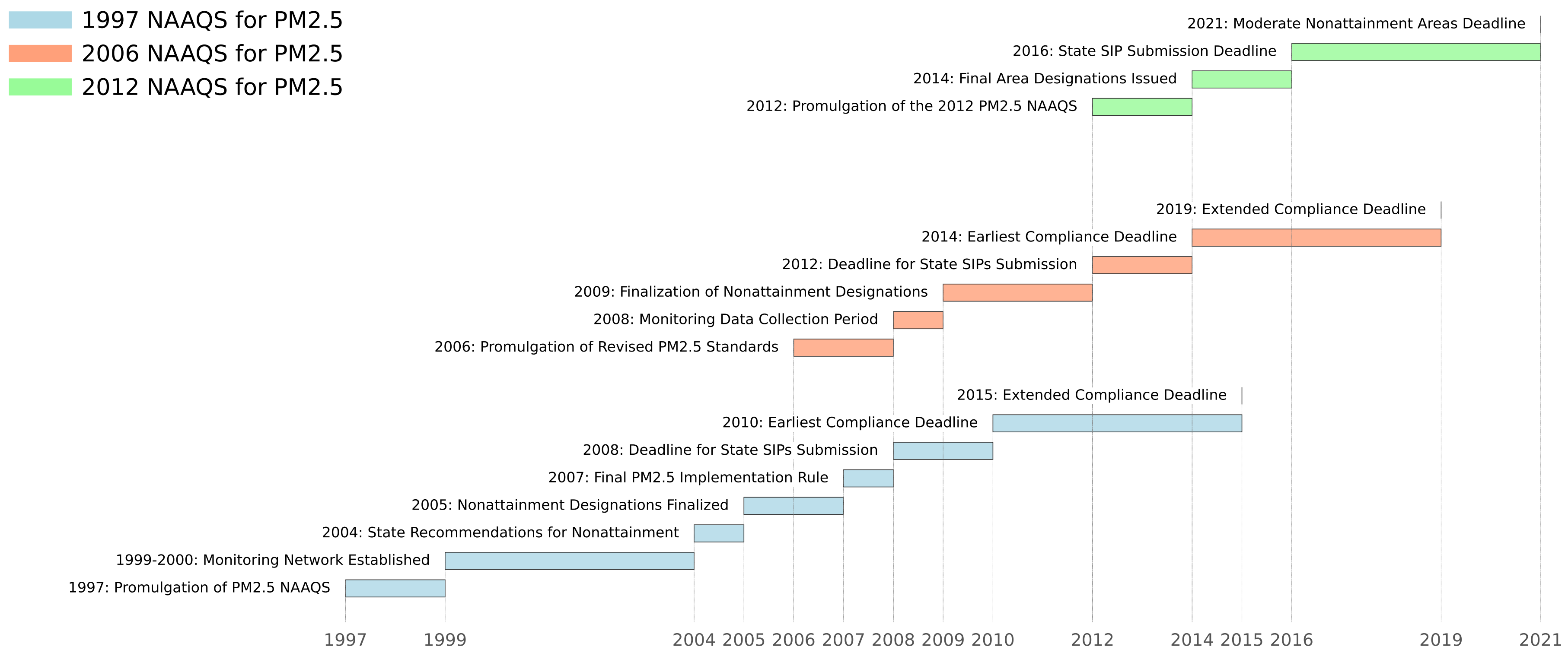


Figure A.3: Timeline of National Ambient Air Quality Standards (NAAQS) for Particulate Matter of less than 2.5 microns in diameters.

Table A.1: Summary statistics

	Status	
	Attainment	Non-attainment
Treatment status		
Number of counties	1509	286
Proportion (%)	84.1	15.9
Std Dev	55.21	7.76
Farm land value in $/acre		
Average	3105.29	5195.90
Std Dev	1572.86	2490.91
Minimum	525.16	1450
25^{th} perc.	1988.67	3420.12
50^{th} perc.	2759.17	4653.75
75^{th} perc.	4048.91	6200.71
Maximum	11518.48	12633.78
Farm land value growth in % (1997-2022)		
Average 5-Year Census	+30.8	+23.2

Appendix B - Covariates

B1 Potential confounders

The Inverse Probability Weighting (IPW) framework relies on the assumption of conditional unconfoundedness, which states that once we control for a carefully chosen set of covariates, the treatment assignment is independent of the potential outcomes. The choice of covariates for the propensity score estimation should be guided by economic theory and prior empirical findings, while ensuring that the model remains parsimonious (Imbens and Rubin, 2015). Including extraneous variables can unnecessarily restrict common support and cause inefficiencies in the weighting process, while omitting key confounders can introduce significant bias into estimated treatment effects (Brookhart et al., 2006).

We adjust for three types of covariates based on their theorized association with both treatment and outcome. The first set includes economic and operational agricultural characteristics at the county level that can plausibly influence both the treatment status and farmland values. These variables include off-farm income, fertilizer and chemical expenses, federal payments, farm concentration, and irrigation. Off-farm income provides financial stability, affecting how farms adapt to economic and regulatory conditions (Kirwan, 2009). Chemical and fertilizer expenses play a significant role in ammonia emissions, a precursor to $PM_{2.5}$, thus contributing to the likelihood of non-attainment with $PM_{2.5}$ National Ambient Air Quality Standards (NAAQS), especially in the eastern United States (Paulot and Jacob, 2014). Irrigation practices, which are critical under climate variability, reflect water management strategies that influence farm operations. Federal payments reflect the level of government support that not only impacts farm behavior but also drives farmland values (Kirwan, 2009). Farm concentration, which captures the structural differences in land use, can drive distinct responses to environmental regulations and land values (Plogmann et al., 2022; Lewis et al., 2023).

Our second set of confounders captures county-level poverty rates and population densities as socioeconomic confounders to address potential variations in local market structures and demographic contexts (Bekkerman et al., 2019). The final set of confounders consists of weather and atmospheric variables. Variations in climate—such as changes in temperature, precipitation, and extreme weather events—directly influence crop yields, farming costs, and land use decisions, making these factors key determinants of farmland values (Schlenker et al., 2007; Massetti and Mendelsohn, 2020). In addition, climate variability can drive changes in agricultural practices, as unpredictable weather and prolonged droughts can lead to increased use of fertilizers and pesticides or expanded irrigation to stabilize yields. Although these responses can help manage short-term risks, they often result in long-term environmental impacts such as nutrient runoff, soil degradation, and higher greenhouse gas emissions (Xu et al., 2018).

We incorporate climatic variables that are key in accounting for weather-related stressors that shape farm productivity and economic resilience. These variables include the frequency of droughts to reflect their impact on water availability, plant stress, and overall farm productivity. Additionally, growing degree days (GDD) with a threshold of 29°C capture cumulative heat exposure, which influences plant development and the likelihood of yield losses. We also include the intensity and frequency of storm-related winds, which cause crop damage, soil erosion, and other disruptions to agricultural productivity, as well as surface temperature inversions to account for stable atmospheric conditions that trap pollutants and elevate $PM_{2.5}$ concentrations, potentially affecting the 24-hour air quality standard independently of economic activity. All atmospheric and weather covariates were accumulated from 1992 to the present to account for heterogeneous exposure to long-term climate change trends and specific extreme events that may occur between census dates.

The definitions and sources of data for confounding variables are presented in the following table. The methodologies used to calculate weather and atmospheric variables are detailed in Appendix B3.

B2 Covariates description and data sources

Variables	Description	Source
Drought	Log of yearly cumulative counts of moderate to extreme dry conditions based on the Palmer Drought Severity Index	Author's calculation based on the gridMET dataset
Inversion	Log of yearly sum of days with surface thermal inversion	Author's calculation based on ERA5 - ECMWF
GDD	Log of cumulative sum of Growing Degree Days (GDD)	Author's calculation based on Gridmet dataset
Wind speed	Log of cumulative sum of maximum sustained wind speed from asymmetrical wind fields around storm tracks	Author's calculation based on International Best Track Archive for Climate Stewardship (IBTrACS)
Poverty	All ages in Poverty, Rate Estimate	Census Bureau, Small Area Income and Poverty Estimates (SAIPE)
Density	Log of average counties' population density	US Census Bureau
Off farm	Log of off-farm income per operation	USDA - Census of Agriculture
Chemical	Log of expenses for insecticides, herbicides, fungicides, and other pesticides excluding commercial fertilizer purchased (per operation)	USDA - Census of Agriculture
Fertilizer	Log of expenses for fertilizer, lime, rock phosphate, and gypsum and the costs of custom application (per operation)	USDA - Census of Agriculture
Irrigation	Log of irrigated cultivated lands (per operation)	USDA - Census of Agriculture
Payments	Log of total federal farm payments (per operation)	USDA - Census of Agriculture
Concentration	Log of number of operation per crop acres harvested	USDA - Census of Agriculture

B3 Weather and atmospheric covariates

Growing degree days

The calculation of degree days, often referred to as "growing degree days" (GDD), is tailored to the specific thermal requirements of a crop. The method involves using a base temperature (the minimum temperature required for crop growth) and an upper threshold (the temperature beyond which additional warmth does not accelerate growth).

We follow Butler et al. (2018) to calculate GDDs using daily maximum and daily minimum 2 m temperature data from ERA5-Land. ERA5-Land is a reanalysis dataset that offers gridded data at a resolution of $0.1^\circ \times 0.1^\circ$ over the global land surface (Muñoz-Sabater et al., 2021). For any grid point g at any given day d, we compute daily heat unit, $GDD_{g,d}$, as:

$$GDD_{g,d} = \frac{T^*_{min,g,d} + T^*_{max,g,d}}{2} - T_{low} \tag{1.B}$$

where,

$$T^*_{max,g,d} = \begin{cases} T_{max,g,d} & \text{if } T_{low} < T_{max,g,d} < T_{high}, \\ T_{low} & \text{if } T_{max,g,d} \leq T_{low}, \\ T_{high} & \text{if } T_{max,g,d} \geq T_{high} \end{cases}$$

$T^*_{min,g,d}$ is defined by using the same low and high bounds of $T_{low} = 8°\text{C}$ and $T_{high} = 29°\text{C}$.

We calculate the average values from the EDD grid points to obtain county-level EDD values for each county in our sample. We then sum the daily GDD values over the entire growing season (from April 1st to September 30th) to get the annual total GDD for each county from 1997 to 2022.

Drought conditions

We use the Palmer Drought Severity Index (PDSI) developed by Palmer (1965), which is the most prominent index of meteorological drought used in the US (Heim Jr, 2002). The PDSI offers several advantages over temperature and precipitation anomalies alone in assessing drought conditions. These advantages stem from its ability to integrate multiple factors influencing moisture availability, such as soil moisture, evapotranspiration, and runoff, which are critical for agriculture and ecosystem health. Additionally, the PDSI accounts for the cumulative effects of weather conditions over time, enabling it to identify extended periods of dryness or wetness. This makes it a more effective indicator of long-term droughts, which have greater impacts on agriculture, water resources, and ecosystems.

We retrieved gridded data on the Palmer Drought Severity Index (PDSI) from the gridMET dataset, which provides high-resolution, daily surface meteorological data across the contiguous United States. The PDSI in the gridMET dataset is calculated using a water balance approach that incorporates temperature, precipitation, and *reference evapotranspiration* (ET_0), calculated using the following Penman-Monteith equation:

$$ET_0 = \frac{0.408\Delta(R_n - G) + \gamma\frac{900}{T+273}u_2(e_s - e_a)}{\Delta + \gamma(1 + 0.34u_2)} \tag{2.B}$$

Where:

- R_n is the net radiation at the crop surface (MJ m^{-2} day^{-1}),
- G is the soil heat flux density (MJ m^{-2} day^{-1}),

- T is the mean daily air temperature (°C),
- u_2 is the wind speed at 2 meters height (m s^{-1}),
- e_s is the saturation vapor pressure (kPa),
- e_a is the actual vapor pressure (kPa),
- Δ is the slope of the vapor pressure curve (kPa °C^{-1}),
- γ is the psychrometric constant (kPa °C^{-1}).

The PDSI calculation uses actual evapotranspiration (AET), which reflects how much water is actually evaporated or transpired, depending on the available soil moisture. AET is derived from Reference Evapotranspiration (ET_0) in the context of limited water availability, as AET reflects the actual water loss constrained by soil moisture, while ET_0 represents the atmospheric demand for water under ideal conditions (with sufficient moisture). The PDSI also incorporates other key components of the water balance, including precipitation (P), soil Moisture Recharge (R), runoff (RO) and potential moisture loss (PL) from the soil through evapotranspiration.

The PDSI is calculated iteratively over time using a recursive formula that incorporates both the current month's moisture anomaly Z_t and the PDSI value from the previous month $PDSI_{t-1}$. The formula is:

$$PDSI_t = \alpha \times Z_t + \beta \times PDSI_{t-1} \tag{3.B}$$

Where Z_t is the moisture anomaly, which represents the deviation of the current moisture conditions from the long-term normal. $PDSI_{t-1}$ is the previous month's PDSI value, allowing the index to carry forward moisture conditions from the previous month. α and β are weighting factors that account for the persistence of drought or wet conditions.

The resulting PDSI value is a standardized index that ranges from negative values, indicating drought, to positive values, indicating wet conditions. Specifically, values between 0 and -1 indicate mild drought, between -1 and -2 indicate moderate drought, between -2 and -3 indicate severe drought, and values below -3 indicate extreme drought. Conversely, positive values represent wetter-than-normal conditions, with higher values indicating more significant deviations from normal.

We then spatially aggregate monthly PDSI grid cell values at the county level for each county in our sample. To obtain county-specific measures of relevant climate conditions, we use a weighted spatial mean that considers the fraction of each grid cell covered by the county's borders. Finally, for each year, we sum the number of instances in which each county experienced moderate to extreme drought conditions.

Thermal inversions

Several studies have shown that the use of high-altitude weather conditions, especially thermal inversions, allows for the isolation of the causal effect of pollutants on economic activity (Avila Uribe, 2023; Dechezleprêtre et al., 2019). During a thermal inversion, the atmosphere is stable and air circulation is very limited, the inversion layer then acts as a "lid" blocking pollutants along the layer and thus promoting the occurrence of pollution peaks.

Data on thermal inversions comes from the ERA5 reanalysis. We obtain mean air temperature measures over the North American domain at a spatial resolution of 0.1° from January 1, 1995 00:00:00 UTM to December 31, 2020 18:00:00 UTM (every 6 hours). Temperature (tmp) are retrieved at multiple pressure levels from 1000 hPa (approximately 111 m above the surface) to 1 hPa (top of atmosphere) divided into $j = 1, ..., 37$ categories where $j = 1$ denotes the lowest atmospheric level above surface (higher pressure level). Since surface pressure may be lower than atmospheric pressure at higher pressure levels due to land surface elevation or low-pressure systems, j is defined dynamically in each grid cell and for each time unit. The index $j = 1$ always corresponds to the lowest pressure level above the surface.

Following Dechezleprêtre et al. (2019) and Chen et al. (2018), we define the presence of thermal inversions τ if temperature inversions occur between the lowest level and the second-lowest level above the local surface for any gridcell and any time level unit:

$$Inv_s = \frac{temp_{j=2} - temp_{j=1}}{z_{j=2} - z_{j=1}} \times \mathbb{1}\{I_J > \gamma\} \tag{4.B}$$

where $\mathbb{1}\{I_J > \gamma\} = 1$ if $I_J > \gamma$ and 0 otherwise. $temp$ is the air temperature and z is the altitude. $temp_{j=2} - temp_{j=1}$ is the strength of the temperature inversion in Kelvin, and $z_{j=2} - z_{j=1}$ is the depth of the inversion measured in hPa. The parameter γ ($0 \leq \gamma \leq 0.5$) measures the critical adiabatic thermal gradient in $K \cdot hPa^{-1}$, characterizing the magnitude of the inversion.

Our final index is calculated as the sum of days with at least one thermal inversion during a year. Our benchmark index is calculated using $\gamma = 0$. We conducted tests on various threshold levels for the parameter γ to identify days with the most significant thermal inversions, which yielded comparable results in our econometric analyses.

Storm and hurricane exposure index

Data comes from the International Best Track Archive for Climate Stewardship (IBTrACS) version 4 dataset, which provides global tropical cyclone data. The dataset includes information on the geographical positions (latitude and longitude) of the storm's center at 3-hour intervals, along with storm characteristics such as maximum sustained wind speed, minimum pressure, and radius of maximum winds. The dataset covers the period from 1848 to the present day and is updated annually. The storm tracks are extracted from 1994 to 2022 by filtering the dataset based on the storm's name and geographical location of interest (LOI) with a buffer size of 300 km around the LOI. The LOI is defined as the Contiguous United States of America, which includes the continental United (the lower 48).

Once the storm tracks data are retrieved, we use the Willoughby et al. (2006) model for the radial wind profile and the Chen (1994) model for the asymmetry of the wind field to model the wind behavior around the storm tracks. The Willoughby model provides a detailed parametric representation of the hurricane's wind field. The model considers the wind speed profile both inside and outside the radius of maximum winds (R_m). The Willoughby et al. (2006) model provides a symmetrical wind field around the cyclone centre. However, cyclonic winds are not symmetric, and an order zero asymmetry is caused by the storm translation (forward motion). In a symmetrical wind field model, the wind speed and direction are the same at any point equidistant from the cyclone center, regardless of the direction from the center. While this simplification is useful for theoretical and initial estimations, cyclonic winds are not perfectly symmetrical in real world scenario. One significant cause of this asymmetry is the storm's translation or forward motion. As the cyclone moves, the winds on one side (usually the right side in the Northern Hemisphere) tend to be stronger than on the other side (the left side). This is due to the additive effect of the cyclone's rotational wind and its forward movement. Since symmetrical models do not account for this forward-motion-induced asymmetry, potentially leading to inaccuracies in predicting wind speeds and impacts on different sides of the cyclone, the Chen (1994) model is used to account for this asymmetry.

The Chen (1994) model improves upon symmetrical wind field models by addressing the asymmetry in cyclonic wind fields caused by the storm's forward motion. It includes the effects of this forward motion, accounting for enhanced wind speeds on the right side (in the Northern Hemisphere) and reduced speeds on the left side. This adjustment results in a more accurate representation of wind distribution compared to symmetrical models. The model uses parameterized adjustments to dynamically reflect the specific characteristics of each storm, such as translation speed, size, and intensity. By reducing the biases inherent in symmetrical models, the Chen model ensures more accurate wind speed predictions and better represents real-world conditions. This leads to improved weather forecasting, more precise risk assessments for wind damage.

Willoughby et al. (2006) model

The Willoughby et al. (2006) model is used to compute the radial wind profile of a storm. The model considers two regions: inside the eye and at external radii, for which the wind formulations use different exponents to better match observations. In this model, the wind speed increases as a power function of the radius inside the eye and decays exponentially outside the eye after a smooth polynomial transition across the eyewall (see also Willoughby (1995), Willoughby and Rahn (2004)).

- Inside Rm ($r < Rm$):

$$V_r = V_m \left(\frac{r}{R_m} \right)^n \tag{5.B}$$

where V_r is the radial wind speed at distance r from the storm center, V_m is the maximum wind speed, R_m is the radius of maximum winds, and n is the exponent of the wind profile. Denoting ϕ as the latitude in degrees, the exponent $n = 2.1340 + 0.0077.V_m - 0.4522.\ln(R_m) - 0.0038.\phi$ as in Willoughby et al. (2006).

- Outside Rm ($r > Rm$):

$$V_r = V_m \left((1 - A) \times e^{-\frac{|r - R_m|}{X_1}} + A \times e^{-\frac{|r - R_m|}{X_2}} \right) \tag{6.B}$$

where,

- $X_1 = 287.6 - 1.942V_m + 7.799 \ln(R_m) + 1.819|\phi|$
- X_2 is typically set to 25
- $A = 0.5913 + 0.0029.V_m - 0.1361 \ln(R_m) - 0.0042 \times |\phi|$ with $A \geq 0$

The Willoughby model provides a detailed profile of wind speeds at various radial distances from the storm center, both inside and outside the radius of maximum winds. As the storm progresses over time, V_r is computed at each time step t for different radial distances from the center. We use a time step of 15 minutes to capture the evolution of wind speeds as the storm moves by interpolating the storm track data provided in IBTrACS dataset. This generates a time series ($v(t)$) of wind speeds at various locations around the storm track.

Chen (1994) model for wind field asymmetry

The asymmetry caused by the translation of the storm can be added as follows,

$$\vec{V} = \vec{V_c} + C \times \vec{V_t} \tag{7.B}$$

where $\vec{V}$ is the corrected wind speed vector. $\vec{V_c}$ is the symmetric wind speed vector computed using the Willoughby model. C is the asymmetry correction factor. $\vec{V_t}$ is the translation speed vector of the storm obtained from the IBTrACS storm's track data, which includes the geographical positions (latitude and longitude) of the storm's center every 3 hours minutes that is interpolated to 15 minutes to compute the radial wind speed.

The asymmetry correction factor C is computed as:

$$C = \frac{3 \cdot R_m^{3/2} \cdot r^{3/2}}{R_m^3 + r^3 + R_m^{3/2} \cdot r^{3/2}} \tag{8.B}$$

where R_m is the radius of maximum winds. r is the radial distance from the storm center.

Storm exposure indices

The maximum sustained wind speed (MSW) over the lifespan of a storm is mathematically defined as:

$$MSW = max(v(t)|t \in [0, T]) \tag{9.B}$$

where $v(t)$ is the wind speed at time t and T is the lifespan of the storm. The MSW then corresponds to the highest wind speed recorded during the entire lifespan of the storm for any location in the radii around storm tracks.

Appendix C - Balance checks and sensitivity tests

Table C.1: Balance check

	Unweighted Sample					Weighted Sample			
	Average			t-stat	p-value	Average		t-stat	p-value
	Control	Treated	SMD			Control	SMD		
Drought	3.059	2.840	-0.141	-2.028	0.043	2.879	-0.025	-0.283	0.778
Inversion	4.824	4.865	0.498	5.510	0.000	4.860	0.060	0.464	0.643
GDD	4.576	4.529	-0.297	-3.670	0.000	4.556	-0.167	-1.320	0.187
Wind speed	1.490	1.577	0.084	1.011	0.312	1.598	-0.020	-0.155	0.877
Pop. density	2.809	4.380	1.360	19.365	0.000	4.362	0.016	0.173	0.862
Poverty	2.636	2.362	-0.618	-9.084	0.000	2.388	-0.059	-0.675	0.500
Fertilizer	9.022	8.390	-0.710	-10.098	0.000	8.356	0.038	0.453	0.651
Chemical	8.191	7.471	-0.662	-9.294	0.000	7.394	0.071	0.801	0.423
Irrigation	1.228	-0.352	-0.944	-12.454	0.000	-0.315	-0.022	-0.247	0.805
Off-farm	9.463	9.317	-0.237	-3.467	0.001	9.278	0.062	0.739	0.460
Conservation	8.937	8.406	-0.951	-12.929	0.000	8.383	0.041	0.446	0.655
Concentration	4.960	4.241	-0.898	-12.434	0.000	4.211	0.037	0.426	0.671

Note: The table provides a balance check between “attainment” and “non-attainment” counties for both unweighted and weighted samples. The ”Average” column reports the mean values for both groups along with the Standardized Mean Difference (SMD). In the weighted sample, mean values are omitted because we target the ATT by reweighting only the control group. Finally, the t-statistic tests whether the observed difference in the SMD is statistically significant.

Table C.2: Benchmark results excluding non-overlapping counties designated under the 2006 NAAQS for PM2.5

	Stabilized IPW				Doubly Robust			
	Coeff	se	95% CI		Coeff	se	95% CI	
			lower	upper			lower	upper
ATT (pre)	0.0052	0.0100	-0.0144	0.0249	0.0020	0.0096	-0.0168	0.0209
ATT (post)	-0.0926***	0.0175	-0.1270	-0.0583	-0.0935***	0.0149	-0.1227	-0.0642
$t-10$	0.0063	0.0182	-0.0295	0.0420	0.0038	0.0181	-0.0317	0.0392
$t-5$	0.0042	0.0186	-0.0323	0.0407	0.0003	0.0175	-0.0339	0.0345
t^*	-0.0819***	0.0151	-0.1115	-0.0523	-0.0832***	0.0142	-0.1110	-0.0553
$t+5$	-0.1020***	0.0220	-0.1450	-0.0589	-0.1072***	0.0191	-0.1446	-0.0697
$t+10$	-0.0940***	0.0244	-0.1418	-0.0462	-0.0901***	0.0210	-0.1313	-0.0489
Pre-trend (χ^2)	0.2802				0.0563			
p-value	0.8693				0.9722			
N	9044				9044			

Note: The table presents regression coefficients from our benchmark model after excluding six newly regulated, non-overlapping counties designated in 2009 under the 2006 NAAQS for $PM_{2.5}$. The omitted counties are Dodge County, WI (FIPS: 55027), Jefferson County, WI (FIPS: 55055), Milwaukee County, WI (FIPS: 55079), Ozaukee County, WI (FIPS: 55089), Washington County, WI (FIPS: 55131), and Waukesha County, WI (FIPS: 55133). Bootstrapped standard errors (se) with 10,000 repetitions are clustered by counties. *** denotes significance at the 1% level. The pretrend test reports the χ^2 statistic and the p-value for a joint test of the null hypothesis that all pretreatment average treatment effects (ATT) are equal to zero.

Table C.3: Benchmark results excluding covariates with lower imbalance adjustment from the propensity score model

	Stabilized IPW				Doubly Robust			
	Coeff	se	95% CI		Coeff	se	95% CI	
			lower	upper			lower	upper
ATT (pre)	-0.0003	0.0175	-0.0345	0.0339	-0.0001	0.0168	-0.0331	0.0328
ATT (post)	-0.0883***	0.0187	-0.1250	-0.0517	-0.0933***	0.0163	-0.1253	-0.0613
$t-10$	-0.0043	0.0227	-0.0487	0.0402	0.0007	0.0227	-0.0437	0.0451
$t-5$	0.0037	0.0188	-0.0332	0.0406	-0.0010	0.0176	-0.0355	0.0336
t^*	-0.0758***	0.0166	-0.1084	-0.0432	-0.0807***	0.0166	-0.1132	-0.0481
$t+5$	-0.0968***	0.0224	-0.1407	-0.0530	-0.1061***	0.0194	-0.1441	-0.0680
$t+10$	-0.0924***	0.0252	-0.1418	-0.0429	-0.0932***	0.0220	-0.1364	-0.0500
Pre-trend (χ^2)	0.1249				0.0062			
p-value	0.9394				0.9969			
N	9078				9078			

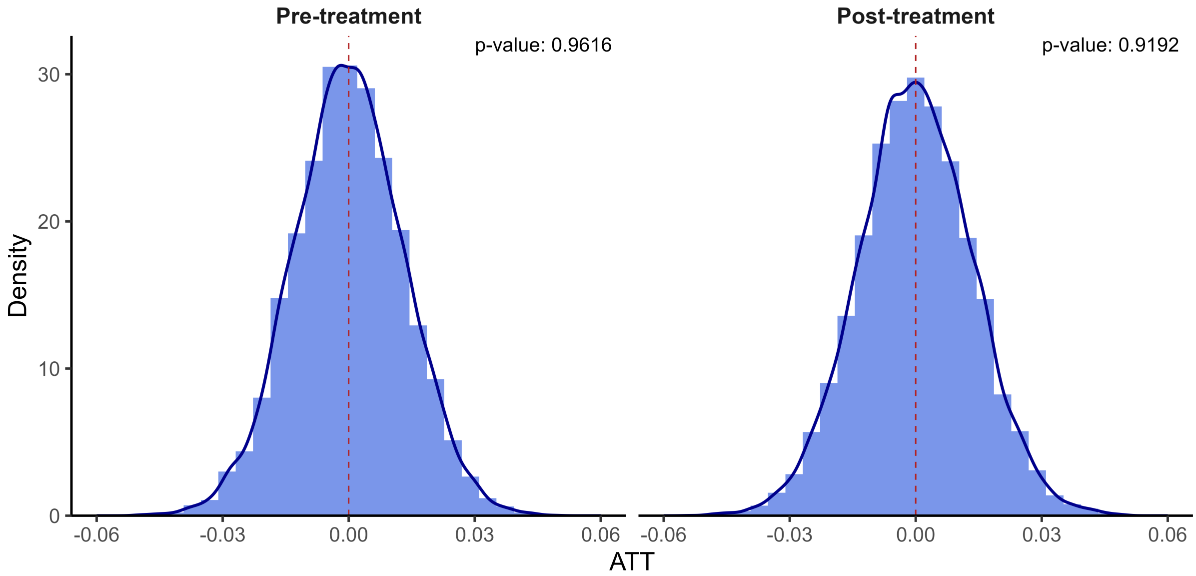
Note: This table presents the regression coefficients from our benchmark model, excluding covariates with lower imbalance adjustment from the propensity score model. Bootstrapped standard errors (se) with 10,000 repetitions are clustered by counties. *** indicates a significance level of 1%. The pretrend test reports the χ^2 statistic and the p-value for a joint test of the null hypothesis that all pretreatment average treatment effects (ATT) are equal to zero.

Table C.4: Placebo test results

	$\lambda = 0.50$				$\lambda = 0.15$			
	Coeff	se	95% CI		Coeff	se	95% CI	
			lower	upper			lower	upper
$t-10$	-0.0063	0.0099	-0.0253	0.0127	-0.0060	0.0116	-0.0289	0.0168
$t-5$	0.0035	0.0087	-0.0138	0.0208	-0.0044	0.0109	-0.0255	0.0166
t^*	0.0007	0.0090	-0.0169	0.0183	-0.0128	0.0115	-0.0352	0.0097
$t+5$	0.0051	0.0097	-0.0139	0.0241	0.0061	0.0127	-0.0187	0.0310
$t+10$	0.0136	0.0099	-0.0060	0.0334	0.0101	0.0129	-0.0149	0.0351
Pre-trend (χ^2)	1.1094				0.3031			
p-value	0.5742				0.8594			
N	9078				9078			

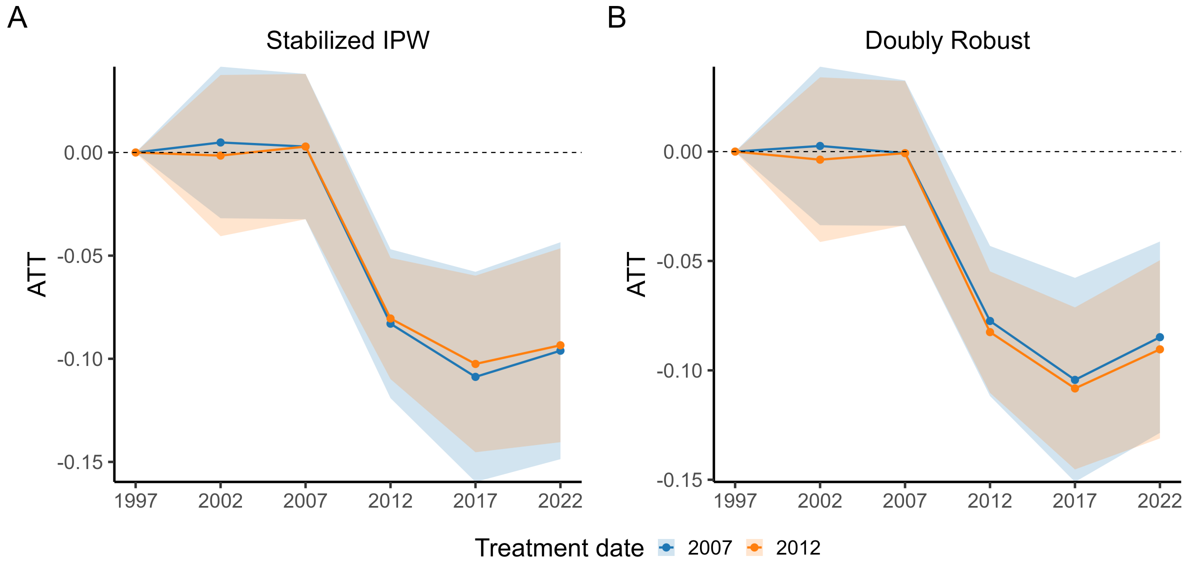
Note: This table presents the regression coefficients from the placebo test estimates of equation (1), where random treatment statuses are assigned to counties. λ refers to the threshold used to determine the proportion of counties randomly assigned as "non-attainment" in the placebo test. t stands for the treatment date, 2012. The specification is estimated using Stabilized Inverse Probability Weighting (IPW). Bootstrapped standard errors (se) with 10,000 repetitions are clustered by counties. *** indicates a significance level of 1%. The pretrend test reports the χ^2 statistic and the p-value for a joint test of the null hypothesis that all pretreatment average treatment effects (ATT) are equal to zero.

Figure C.1: Estimated average treatment effects from the permutation placebo test



Note: We randomly assign the proportion of "non-attainment" counties under the 2006 NAAQS for $PM_{2.5}$ using λ = 0.15 from 5,000 random assignments. The histogram displays the distribution of the placebo estimates. The dashed line represents the estimated average treatment effect from the obtained distribution. The p-value of the permutation placebo test is the proportion of placebo estimates that are significantly different from zero in the pre-treatment and post-treatment periods.

Figure C.2: Comparative analysis of treatment dates



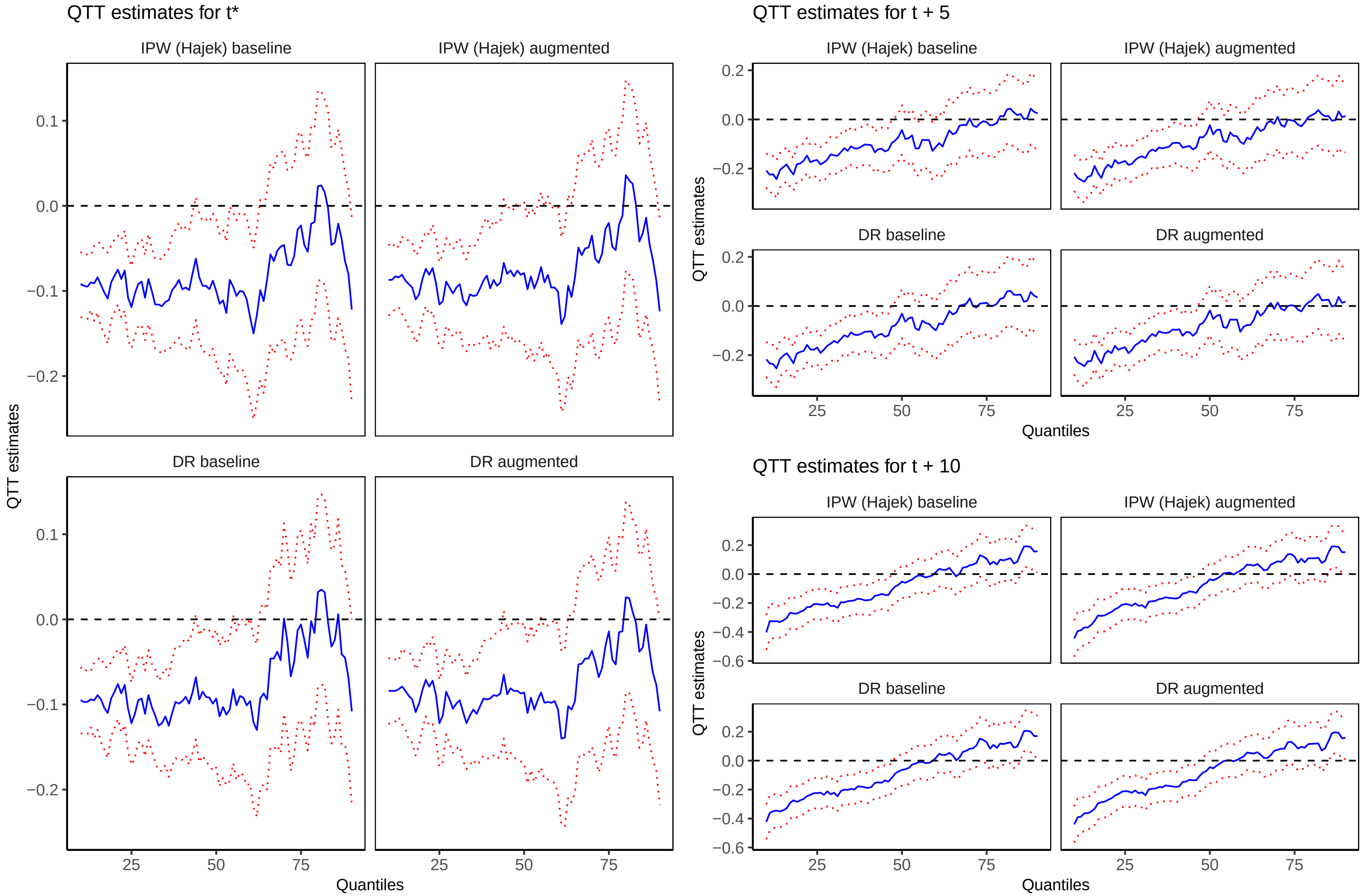
Note: These figures plot the event-time coefficient estimates from Equation (1) for the whole sample, using 2007 (2012) as the treatment date in blue (orange). Panel A refers to estimates using Stabilized IPW, and Panel B refers to estimates using Doubly Robust estimators. The shaded areas around the lines represent the 95% bootstrapped confidence intervals with 10,000 repetitions clustered at the county level.

Table C.5: Benchmark results with standard errors clustered ar the Commuting Zone (CZ) level

	Stabilized IPW				Doubly Robust			
	Coeff	se	95% CI		Coeff	se	95% CI	
			lower	upper			lower	upper
ATT (pre)	0.0039	0.0120	-0.0195	0.0273	0.0009	0.0113	-0.0213	0.0232
ATT (post)	-0.0921***	0.0213	-0.134	-0.0505	-0.0937***	0.0186	-0.1303	-0.0571
$t-10$	0.0049	0.0195	-0.0319	0.0416	0.0026	0.0186	-0.0330	0.0381
$t-5$	0.0029	0.0233	-0.0422	0.0479	-0.0007	0.0224	-0.0437	0.0423
t^*	-0.0805***	0.0194	-0.1184	-0.0425	-0.0825***	0.0187	-0.1192	-0.0458
$t+5$	-0.1025***	0.0242	-0.1508	-0.0541	-0.1083***	0.0215	-0.1512	-0.0652
$t+10$	-0.0935***	0.0281	-0.1487	-0.0381	-0.0904***	0.0237	-0.1370	-0.0438
Pre-trend (χ^2)	0.1178				0.0207			
p-value	0.9428				0.9897			
N	9078				9078			

Note: This table presents the regression coefficients from estimates of Equation (1). t^* stands for the treatment date, 2012. The specification is estimated using Stabilized Inverse Probability Weighting (IPW) and Doubly Robust estimators. Bootstrapped standard errors (se) with 10,000 repetitions are clustered by Commuting Zones (CZ). *** indicates a significance level of 1%. The pretrend test reports the χ^2 statistic and the p-value for a joint test of the null hypothesis that all pretreatment average treatment effects (ATT) are equal to zero.

Figure C.3: RIF-OLS estimates of the Quantile Treatment Effect



Note: The baseline specification includes all confounders without interaction effects in the propensity score model used for the Hajek Inverse Probability Weights (IPW) and Doubly Robust (DR) Weights estimates. The augmented specification incorporates interactions among chemical and fertilizer expenditures, irrigation and drought frequency, and county population density and poverty within the propensity score model.