The Employment and Concentration Effects of the Nonattainment Standards: Evidence from the 2004 Expansion

E. Mark Curtis
Wake Forest University
curtisem@wfu.edu

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Abstract

In 2004 the EPA implemented the largest regulatory expansion of the National Ambient Air Quality Standards since the program's inception in the 1970's. As a result, polluting plants in hundreds of counties faced significant new regulatory costs. This paper discusses the selection process by which counties were designated as nonattainment and provides empirical evidence on the impacts of these regulations on employment and market concentration in exposed industries. Results from a nonparametric differences-in-differences matching estimator show that, relative to the constructed counterfactual, employment in affected industries temporarily increased (fell less), the number of establishments permanently decreased and establishment size increased. These findings are consistent with regulation increasing firms' investment and entry costs. Furthermore, they suggest that the labor market adjustment costs of the regulation are less salient than product market surplus losses that will occur due to incumbent firms' increased market power.

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1 Introduction

The Environmental Protection Agency's National Ambient Air Quality Standards (NAAQS) have been called the most costly environmental regulation implemented in the history of the United States.[1] While proponents have cited the health benefits that come from improving air quality, industry groups decry them for the extra production costs they impose on pollution-emitting establishments. Economists have long recognized the importance of these regulations and have devoted considerable research to examining their economic impact. Indeed, a number of prominent papers have studied policy-induced changes in outcomes ranging from health benefits (Isen *et al.* 2014) to labor market transition costs (Walker 2013) to impacts on productivity (Greenstone *et al.* 2012). Employment has been a primary outcome of interest (Greenstone 2002; Kahn & Mansur 2013) and more recently economists have explored the effects on market structure (Ryan 2012). All of these papers have examined the original implementation of the NAAQS that began in the 1970's and a revision of the standards that occurred in 1990.

The more recent 2004 expansion of the NAAQS has received far less attention by researchers.[2] This despite the fact that nearly *three times* as many counties entered into nonattainment status in 2004 than entered during the 1990 expansion. Given the size of the recent expansion and the fundamental changes that have taken place to the manufacturing sector and labor markets in the United States economy over the past quarter-century, the 2004 NAAQS expansion represents a chance to reassess our understanding of this key U.S. environmental policy. Evaluating the costs and benefits of changing the ozone non-attainment standard is of considerable interest not only to policy makers who are required by law to reevaluate the non-attainment standards every five years, but also to economists seeking to understand the broader economic implications of regulating a key sector of the economy.[3]

Indeed, the manufacturing sector that was regulated in 2004 was fundamentally different than the manufacturing sector regulated under previous expansions of the Clean Air Act. Just between 1990 and 2004, capital intensity, measured as capital-output ratio, nearly

[1]See the National Association of Manufacturing sponsored report "Economic Impacts of a 65 ppb National Ambient Air Quality Standard for Ozone" which claims that proposed ozone standards would cost $140 billion per year as well as the loss of 1.4 million jobs.

[2]A partial exception is (Kahn & Mansur 2013). This paper studies the role of electricity prices, right-to-work laws and the NAAQS on county employment levels. Their cross sectional border-discontinuity method includes, but is not limited to, those counties that entered non-attainment in 2004.

[3]EPA announced on October 1, 2015 that the new standard would be 70 PBB, a level which will force roughly 100 additional counties into nonattainment based off of 2012-2014 design values for ozone found at `http://www3.epa.gov/airtrends/values.html`. Despite lawsuits from environmental groups and democratic states, the new EPA administration has postponed and delayed designating new areas as nonattainment based on the 70 PBB standard.

doubled and the large majority of manufacturing industries had become significantly more concentrated (Autor *et al.* 2017; Grullon *et al.* 2016). Despite steady increases in overall manufacturing output, the number of workers in the sector had declined by over 20% and both worker turnover and job reallocation saw noticeable downturns (Haltiwanger *et al.* 2012). The regulatory landscape was also quite different in 2004 than in 1977 or 1990, such that newly regulated plants were more likely to have already been outfitted to comply with environmental regulations and thus not as heavily impacted. The considerable changes in the manufacturing sector should give us pause when evaluating the external validity of past research with respect to present-day policy changes.

While the state of the manufacturing sector had changed considerably by 2004, the basic framework of the nonattainment standards and the types of costs they imposed on manufacturers had not. When a county enters nonattainment, polluting plants located in that county are forced to comply with a variety of new regulations. Existing plants are required to install "reasonably available control technology" (RACT) as defined by the EPA and new emission sources are required to achieve "lowest available emission rate" on top of the RACT requirement. Any new emissions source, whether it be a newly constructed plant or an expanding plant, also must undergo a lengthy "New Source Review" process and is required to obtain offsets for every new ton of emissions they produce. The upshot of these regulations is that they substantially increase entry and investment costs for firms in polluting industries (Becker & Henderson 2000; Becker 2005).[4]

This paper attempts to gain insights into how these increased costs affect labor demand and market structure by estimating the impact of the non-attainment standards on employment levels, number of establishments and establishment size using county-industry data from the County Business Patterns and the National Emissions Inventory. The outcomes examined speak both to the labor market costs faced by workers and the market structure in affected industries.[5]

While it is clear that these regulations increased costs to regulated facilities, the extent to

[4]Becker & Henderson (2000) find that chemical plants in counties designated as nonattainment for ozone had 17% higher total operating costs than similar plants in attainment counties. Using data from the Pollution Abatement Costs and Expenditures (PACE) survey, Becker (2005) finds that plants in ozone nonattainment counties had significantly higher pollution abatement costs. Publicly available PACE data shows that manufacturers spend $8.6 billion every year on pollution abatement activities with 48% of those expenditures going towards worker wages. Not surprisingly, estimates from industry reports find even higher costs of complying with the ozone nonattainment standards.

[5]Importantly, neither of these outcomes represents a direct cost that can be inserted into a cost-benefit analysis. Obtaining a dollar figure requires the use of a structural model that estimates the industry's cost structure or more detailed data on the job transition costs and wages. Nonetheless, the outcomes studied represent important and relevant variables that improve upon our current understanding of how environmental regulations impact economies.

which these direct costs manifest themselves in changes in labor demand and market structure is not immediately evident. Consider labor demand: to comply with environmental regulations plants may hire new workers to install and maintain their new pollution abating capital and to monitor their now altered production process. Assuming no change in production levels, this would result in firms demanding additional labor (Morgenstern *et al.* 2002; Greenstone *et al.* 2012). However, as costs go up, plants may also choose to downsize or relocate production to less regulated regions, thus reducing the number of workers in the regulated region. With respect to market structure, increases in fixed costs can result in fewer new firms entering the market and increased market power for incumbent firms in industries that are already highly concentrated.[6] As a result, regulation can favor large, incumbent firms and reduce market competition due to the declining number of new firms (List *et al.* 2003). Because of the theoretical ambiguity of the employment effect and the limited research on market concentration, empirical work is particularly informative.(see Heyes (2009) for a literature review on regulation and competition). The employment and market concentration effects of the policy are of first order importance to understanding the net benefits of the policy. For example, Walker (2013) finds that workers faced $5.4 billion of transition costs and Ryan (2012) finds that product market surplus declined by as much as $3.2 billion in the cement industry alone as a result of the 1990 Clean Air Act Amendments.

However, the empirical task of estimating the impact of nonattainment standards is complicated by a variety of endogeneity concerns which, even in the economics literature, are not always explicitly addressed. [7] As discussed in detail in Section 2, states negotiate with the EPA to determine which counties will be designated as in nonattainment. Exceeding the pollution threshold is the starting point for determining which counties will be subject to nonattainment regulations but it is neither a necessary or sufficient condition for entering nonattainment status. In addition to the county's pollution readings, the final determination is, among other things, based on whether their emissions contribute to nearby counties' ability to meet air quality thresholds and, crucially for identification purposes, emissions

[6] Plants in polluting industries, even in the absence of regulation tend to be capital intensive, have higher fixed costs and as a result, higher levels of market concentration.

[7] Past research on the 1977 CAAA and the 1990 CAAA has used either a differences-in-differences (DD) strategy with plant level data (Greenstone (2002), Greenstone *et al.* (2012), Walker (2013)) or a border discontinuity method with county-industry level data (Kahn & Mansur 2013). Endogeneity concerns are very context-specific. These papers, while they do not explicitly discuss the selection concerns inherit in the designation process, attempt to account for differing trends and levels using a variety of methods. A number of these papers are quite successful in doing so. DD papers need to focus on trends while cross-sectional discontinuity papers must worry about levels. The purpose of this paper is not to litigate any particular paper but rather to bring to the forefront an issue that has yet to be fully discussed and to provide a strategy to overcome this concern. In fact, the selection concerns discussed here suggest some past papers may be understating the effect of entering nonattainment status.

levels of the county's stationary sources and *trends* in the county's stationary source emissions. Because of this non-random selection process, nonattainment counties are more likely to have a larger manufacturing presence than counties that are in attainment, they are likely to have differing trends in manufacturing activity and they are more likely to be located in a metropolitan area.

Given the process by which counties are designated as nonattainment, it is not surprising that summary statistics display substantial differences between industries in nonattainment counties and the industries in the attainment counties. In short, counties switching to nonattainment not only have more employment, establishments and NO_x emissions, but they are also less likely to be experiencing reductions in industrial activity prior to the implementation of the regulations. When selection into the treatment is based on observations' trends and levels, then assuming common county or industry trends will not fully control for the selection process, nor will comparing border counties when treated counties are specifically chosen based on their characteristics. As such, the empirical strategy used to identify the effect of nonattainment status should directly control for the selection process that ensures that polluting industries in counties entering into nonattainment will be different both from national industry trends and different than the overall manufacturing trend in the county.

To account for concerns over selection into nonattainment status, this paper uses a nearest-neighbor propensity score matching technique developed by Heckman *et al.* (1997) and Heckman *et al.* (1998) and used more recently in the environmental literature by Fowlie *et al.* (2012), Banzhaf & Walsh (2008), Gray *et al.* (2014) and Petrick & Wagner (2014) among others. I gather data from the County Business Patterns and NO_x emissions data from the National Emissions Inventory dataset at the county-industry-year level. For every "dirty" county-industry that enters into non-attainment status in 2004 I construct a counterfactual of m "nearest-neighbor" county-industries based on a rich set of pre-treatment characteristics. A strict overlap condition is imposed whereby, for each county-industry entering non-attainment, only observations in the same industry and Census division are included in its pool of potential controls. From this pool, the counterfactual(s) is selected based on pre-treatment manufacturing activity levels (employment size), pre-treatment manufacturing activity trends (employment trends) and pre-treatment NO_x emissions levels.

Matching on these pre-treatment characteristics overcomes concerns that selection into the treatment is endogenous. Results from this nearest-neighbor matching specification suggest that immediately following the regulation, employment in county-industries that entered into non-attainment actually shrank less relative to employment in the constructed counterfactual. This short-term employment effect is shown to dissipate and ten years following the regulation the effect is close to zero and statistically insignificant. Previous work has shown that regulated plants face considerable capital and labor costs to comply with environmental regulations (Becker & Henderson 2000; Becker 2005) and has demonstrated theoretically that employment may rise after regulation (Greenstone *et al.* 2012; Berman & Bui 2001; Morgenstern *et al.* 2002). Nonetheless, to this author's knowledge, this is the first empirical evidence yet that such a relationship exists with any environmental regulation.

While the employment effect is temporary, the effect on market structure, as measured by number of establishments and establishment size, is persistent over the study period. The number of establishments in polluting industries shrinks by 6% ten years after the regulation and average establishment size grows by 8%. The decline in the number of establishments and the increase in plant size suggests that regulation leads to higher market concentration. Increased market concentration has clear implications for consumer surplus (Ryan 2012) and may also affect long-run outcomes such as innovation and productivity (Aghion *et al.* 2014; Bloom & Van Reenen 2007). The results, consistent with the structural model of Ryan (2012), point to the costs of the ozone nonattainment regulations being borne less by workers or capital owners and more by consumers.

While these are important findings, some caution should be taken in interpreting the results. First, neither the employment or market structure results represent a direct pecuniary measure of the cost of regulation. They are important economic outcomes with political economy and welfare implications but transforming the outcomes into a specific dollar cost requires assumptions that are beyond the scope of this paper. This is particularly relevant given that the paper does not attempt to explicitly model changes in market power for firms in the regulated industries. Doing so would require, among other things, detailed firm-level information on market share and customer location. Second, these results do not speak to the impacts of the regulations on firms' profits or productivity. Indeed, as suggested by Greenstone *et al.* (2012), hiring additional workers to comply with regulation will in fact lower plant productivity. Finally, the number of establishments and establishment size are only proxies for market structure. Products vary in the extent to which they can be transported and in the extent to which they can be substituted. While the effects on these outcomes are symptomatic of changes in market structure, more information and modeling is needed to understand changes in market power.

Despite these caveats the results are quite informative. The finding of a temporary employment increase in nonattainment counties relative to similar attainment counties suggests that, at least in the short-run, concerns over regulation-induced job loss may be overstated. Of greater concern is the apparent increase in market power that firms experience following the regulations. The nonattainment standards, by increasing the market power of firms, will create a less dynamic market and will lead to losses in overall market surplus. Although this paper does not estimate a full model estimating market surplus, past research has found that even small increases in market power for narrowly defined industries can lead to billions of dollars in lost surplus (Ryan 2012). While there are many important questions that remain, this study adds important new evidence of how the ozone nonattainment standards specifically, and environmental regulations more generally, are likely to shape today's manufacturing sector. The results shed light on policy's effect on labor demand and market structure and are particularly important for those wishing to understand the impact of future changes to the NAAQS ozone threshold levels.

The remainder of the paper is organized as follows. Section 2 presents a brief history of the CAA and the NAAQS. Section 3 describes conditions required for identification and Section 4 details important aspects of the data used in the analysis. Section 5 provides the econometric model, results and specification checks. Section 6 discusses the results. Section 7 concludes.

2 Background of NAAQS Ozone Standards

The National Ambient Air Quality Standards were first implemented following the passage of the 1970 Clean Air Act Amendments. However, due to limited funding and uncertainty surrounding the rules, they were not fully enforced until the passage of the 1977 Clean Air Act Amendments seven years later. The NAAQS set standards for six criterion pollutants, Particulate Matter, Carbon Monoxide, Nitrogen Oxides, Sulfur Dioxide, Lead and Ozone. An air quality standard is set for each of the six pollutants and every county in the United States is designated as either in attainment or nonattainment for each of the standards.

Polluting plants that are located in counties designated as nonattainment for a particular pollutant are subject to a variety of regulations. Existing plants are required to install and maintain reasonably available control technology, the precise definition of which is industry specific and negotiated between plants and regulators. New and expanding facilities are required to meet a variety of far stricter regulations. First, they must meet a Lowest Available Emissions Rate" standard. These standards require specific pollution abating capital to be installed regardless of the costs to the plant. Additionally, any new source of emissions in a nonattainment county, whether it be a new plant or an expansion of an existing plant, must be offset from an existing source within the same county.[8]

[8]As discussed in detail in (Ferris *et al.* 2013), required abatement activities vary based on the specific clas-

Of the six criterion pollutants, the standards for ozone have been the most difficult for counties to meet. The EPA has steadily lowered the specific threshold which a county must meet to be in compliance. The 1997 standard set the ozone standard at 84 parts per billion. The finalized 1997 standard gave three years for State governors to recommend and EPA to designate particular areas within states as being in nonattainment. After the initial proposed designation, another three years were given to states to comment on the EPA's list of proposed counties and to develop State Implementation Plans (SIPs).[9] As a result of the 1997 standard 446 counties were designated as nonattainment for ozone and SIPs were implemented effective July 15, 2004. Of these, 239 were in attainment for the previous ozone standard.[10]

Economists have exploited the features of the NAAQS to identify the impact of environmental regulation. There is temporal, geographic and industry variation written into the policy itself. This variation allows for the comparison of outcome variables across these dimensions accounting for preexisting trends that are common to an industry or geographic region. As discussed in (Greenstone *et al.* 2012) and (Ferris *et al.* 2013), the variation in regulation is not always as clean as it might appear. Plants in the United States are subject to a variety of other environmental regulations besides the NAAQS. For example, the NO_x Budget Trading, the Regional Greenhouse Gas Initiative, Prevention of Significant Deterioration standards are a few of the regulations which manufacturing establishments in *attainment* counties may have to comply with. Each of these has been shown to have an impact on polluting plants' activities. Additionally, the number of regulations has increased over time as has the implicit pollution tax that emitting plants face (Shapiro & Walker 2014). As a result of these additional regulations it may be possible that changes to a county's ozone attainment status is less impactful than in previous years.

An aspect of the policy which has received less attention is the process by which counties are designated as nonattainment. Generally, counties enter into nonattainment based on air quality readings picked up by monitors located in the county. Nonetheless, the actual designation of nonattainment is based on a number of additional factors. In theory, counties are designated as nonattainment when their air quality meets the standard while other counties are designated as attainment when their air quality does *not* meet the attainment standard. However, actual nonattainment status is only determined following a lengthy back-and-forth

sification of nonattainment. Counties in nonattainment may be designated as marginal, moderate, severe and extreme. Specific pollution abating capital requirements and offset ratios vary based on the specific designation.

[9]See `https://www3.epa.gov/ttn/naaqs/standards/ozone/data/19970718_presidential_memo.pdf`

[10]See `https://www3.epa.gov/airquality/greenbook/gnsum2.html` for further information regarding designation date and nonattainment areas.

between states, which generally wish to limit costs to their industries, and the EPA, the regulatory agency charged with implementing the law.

States may appeal the status of counties whose ozone levels are above the attainment standard in two ways. They can request a "bump down" whereby, if granted from the EPA, particular counties may be moved to a less stringent designation of nonattainment.[11] Second, they can request deferments of nonattainment designation to provide extra time in which to demonstrate that they are on a path to meet the new standard.[12] Frequently, states will request both "bump-downs" and deferments. For example, the Winston-Salem / Greensboro metro area was slated for designation as nonattainment for ozone based on their ozone levels. However, the state of North Carolina petitioned the EPA for a deferment, arguing that emissions were already sharply falling in the metro area and that as a result of the naturally occurring declines they were on pace to meet the standards in coming years without having to comply with the costly regulations that come with nonattainment status. They successfully petitioned EPA for a deferment of nonattainment designation and were granted a "Bump Down." In arguing for redesignation they stated "The emissions data shows an expected decrease in NOx emissions of about 382 tons per day between 2000 and 2007. Further NOx emissions reductions are expected beyond 2007 due to implementation of Federal, State and local control measures. The VOC emissions will decrease by 20 tons per day between 2000 and 2007. Again, further reductions are expected beyond 2007." In this case, the counties were designated as attainment in large part because industrial activity in the region was declining.[13]

On the flip side, the EPA has chosen to designate certain counties as nonattainment even though their ozone levels were in compliance with the NAAQS standards. Consider the recent case of the metro Atlanta area. Only four counties in the center of Metro Atlanta had ozone readings that qualified them for nonattainment status. As a result, the state of Georgia requested that only these counties be designated as nonattainment. However, the EPA came back and designated a total of *eighteen* counties in the Metro Atlanta area as nonattainment. In making their decision, the primary criterion given by the EPA for designating these specific counties as nonattainment was that their industrial polluting activity was expected to contribute to the ozone levels in the Metro Atlanta area. For each county in the broad metro

[11] There are multiple designations that counties may receive ranging from marginal to extreme. See (Ferris *et al.* 2013) for more information.

[12] Emissions trends is listed on page 3 of `https://archive.epa.gov/ozonedesignations/web/pdf/epatsd-2.pdf` as one of the criteria for counties to be considered for reclassification.

[13] See the full request for reclassification at `https://archive.epa.gov/ozonedesignations/web/pdf/denrrequest.pdf`. See `https://www3.epa.gov/airquality/greenbook/efrnrpt2.html` for a full list of counties that deferred designation.

area the EPA documented their current level of emissions and their expected future emissions of NO_x and VOC's. Counties with low emission levels were not chosen for designation into attainment (EPA 2008).

The above examples demonstrate the importance of accounting for selection into treatment status. If, for example, nonattainment counties are chosen because they have high levels of industrial activity that are not in decline, then the appropriate counterfactual should also have high levels of industrial activity that are not in decline prior to the regulation. This selection process by which counties are designated as nonattainment has not been explicitly discussed in the economics literature. The empirical analysis that follows is largely motivated by this selection process.

3 Research Design

The empirical strategy of this paper is based on the potential outcome framework. It is assumed that there are two potential regulatory states to which an observation can be assigned. In the first, the observation receives the treatment of entering nonattainment status and in the second that observation does not receive the treatment. Let $D_i = 1$ if county-industry i is subject to nonattainment regulations and let $D_i = 0$ if county-industry i remains unregulated. The potential outcomes $Y_{it}(1)$ and $Y_{it}(0)$ refer to the outcome for observation i in period t conditional on being regulated and not being regulated, respectively. The average treatment effect on the treated can therefore be written as:

$$\alpha_{TT} = E[Y_{it'}(1) - Y_{it'}(0) | D_i = 1]. \tag{1}$$

Here, t' indicates a year after the observation has entered nonattainment. Because we never observe $Y_{it'}(0)|D_i = 1$, it is necessary to construct estimates of the counterfactual outcomes using observations that did not enter into nonattainment.

Extending this framework, Heckman *et al.* (1997) and Heckman *et al.* (1998) suggest a differences-in-differences semi-parametric matching estimator to evaluate the treatment effect of public policies. The estimator they propose is the following:

$$\widehat{\alpha_{DID}} = \frac{1}{N_1} \sum_{j \in I_1} \left\{ (Y_{jt'}(1) - Y_{jt^0}(0)) - \sum_{k \in I_0} w_{jk} (Y_{kt'}(0) - Y_{kt^0}(0)) \right\}. \tag{2}$$

In the above equation α_{DID} represents the differences-in-differences matching estimator. N_1 is the number of observations in the treatment group with the treatment participants indexed by j and nonparticipants indexed by k. $Y_{jt'}(1) - Y_{jt^0}(0)$ is the change in the outcome variable for treatment observation j between period t' and t^0, where t' is a period after the treatment has been implemented and t^0 is a period just before the treatment has been implemented. Observation k, which belongs to the set of potential controls, is weighted by w_{jk}.

The nearest neighbor estimator used in the baseline specification of this paper weights control observations based on similarity to treated observations. Specifically, the analysis uses a propensity score nearest neighbor matching estimator that estimates the propensity score in a probit regression of an observation's treatment status on a list of observable characteristics. For each treated observation j, the m observations with the closest propensity score to j are chosen as j's counterfactual. The observable characteristics used to match control to treated observations are pre-treatment trends, pre-treatment employment levels, pre-treatment NO_x emissions levels and MSA status. Furthermore, the pool of potential matches for a given county-industry entering nonattainment is limited to other observations that belong to the same industry and are located in the same Census Division.[14]

This framework is extended by estimating the model for multiple t' post periods. Doing so allows for an inspection of the dynamics of the treatment effect whereby the change in the treated group is compared to the change in the constructed control group one year after the treatment, two years after the treatment, through τ years after the treatment. To test for pre-existing trends, the model is also run setting t' to years before the regulations began. Finally, all models are augmented with the regression-biased adjustment estimator suggested by Abadie & Imbens (2006). This addresses additional potential concern over bias introduced by poor match quality. Although we directly test for balance along the key covariates, this adjustment will correct for the fact that some treated observations may not have a nearest neighbor with similar enough characteristics along the continuous variables that are being used in the matching process.

[14]There are nine Census Divisions in the United States, which is to say that a match must come from the same general geographic region as the treated county-industry. The shaded regions in the background of Figure 2 depict the Census Divisions.The primary specifications set m equal to ten but results do not substantively differ when lower values of m are selected.

4 Data

The data used in the paper comes primarily from three sources: The County Business Patterns, the National Emissions Inventory and the EPA's phistory file which contains historical data for every county on their nonattainment status for each of the six criteria pollutants.[15] Previous papers on the NAAQS have used either county level data (List *et al.* 2003; Kahn & Mansur 2013; Stanley 2016) or plant level data (Greenstone 2002; Walker 2013). This paper uses county-industry level data from the CBP. Plant level data is often able to identify the exact plants that were regulated by the regulation. Understanding the plant level impact is important, but given that the policy change occurs at the county level, it is important to know the extent to which important county level outcomes are affected. For example, employment in existing plants may be unaffected but there may be fewer plant births in a regulated county. County level data will fully capture both the intensive and extensive margin on which employment changes occur.

The CBP is a yearly data product released by the Census Bureau that provides sub-national economic data by industry. The source of the CBP is the Business Register, Census' Company Organization Survey and other economic censuses and surveys such as the Census of Manufactures and the Annual Survey of Manufactures. County level data from the CBP is used here to create a panel dataset of business activity by industry between 1998 and 2013. The outcome variables of interest are employment levels, number of establishments and average establishment size in a county-industry pairing. Following previous literature, this paper uses three-digit NAICS codes as the industry level of observation (Greenstone 2002; Kahn & Mansur 2013).

While the CBP has the advantage of being publicly available, it also has the disadvantage of having to undergo a thorough review process to prevent the release of any data that would disclose the exact records of any single establishment. Therefore, if very few establishments are located in a particular industry in a county, then employment data will be suppressed for that county-industry observation. The data used in the analysis takes advantage of the thirteen establishment-size cell count variables to impute employment when it is suppressed. Employment is imputed by multiplying the number of establishments in each establishment-size cell by the midpoint establishment size of that category.[16] The baseline

[15] `http://www.epa.gov/airquality/greenbook/data_download.html` Papers studying the original 1970's designation of nonattainment did not directly observe county-level attainment status. Rather they used pollution readings to infer attainment status.

[16] All county-industry observations contain the number of establishments in narrowly defined employee size categories (1-4, 5-9, 10-19, . . . , 5,000+). See Kahn & Mansur (2013) for a full explanation of the imputation method. CBP also offers a range for the overall level of employment in the county-industry when it is sup-

analysis of this paper is performed on county-industry observations with a reasonable pre-treatment employment size. That is, observations with fewer than 50 employees in 2000 are dropped from the analysis. This is done for two reasons. First, small plants are unlikely to be impacted by the regulations as they will not be major sources of pollution. Second, small county-industries are far more likely to have employment be imputed and the method used for imputation greatly reduces the variance in the data. Because 85% of all manufacturing employment in NO_x emitting industries is located in county-industries with over 50 employees, this is unlikely to be a major concern.

The preferred specification of the paper will use only the six three-digit NAICS industries that past researchers have defined as NO_x emitting industries (Greenstone *et al.* 2012).[17] Furthermore, all county-industries in the twelve states which make up the Ozone Transport Region (OTR) are dropped from the analysis. As pointed out by Ferris *et al.* (2013), all counties in the OTR were already regulated as if they were in moderate nonattainment status for ozone.

To understand the size of the NAAQS expansion that occurred in 2004, Figure 1 displays the number of new counties that entered into nonattainment for ozone in every year between 1985 and 2011. As can be seen, the expansion of 2004 was far larger than any other year including the 1990 expansion which has been the subject of much research. The map in Figure 2 shows the counties that were in nonattainment before 2004 and the counties that entered nonattainment in 2004. The gray shaded areas in the background represent different Census divisions, which will be used to construct an appropriate counterfactual for treated observations. It should also be noted that the number of new counties that entered in 2004 is approximately the number that would enter nonattainment were the EPA to move forward with lowering the ozone standard from 75 PBB to 70 PBB. Roughly 200 additional counties would enter if the standard were lowered to 65 PBB (McCarthy 2015).

Table 1 lists summary statistics for all "dirty" county-industries that switched into nonattainment status for ozone in 2004 and county-industries which were not subject to nonattainment status between 1998 and 2013.[18] Not surprisingly, county-industries that switch into nonattainment are larger, have higher levels of NO_x emissions and are more likely to be lo-

pressed. Mian & Sufi (2012) choose to take the mean of this range when employment is missing in a county-industry cell.

[17]These are the six three-digit NAICS industries with the highest NO_x intensity where NO_x intensity is defined as total NO_x emissions in the industry divided by total output of the industry. They are Primary Metal Manufacturing (NAICS 331), Paper Manufacturing (NAICS 322), Nonmetallic Mineral Product Manufacturing (NAICS 327), Chemical Manufacturing (NAICS 325), Wood Product Manufacturing (NAICS 321) and Petroleum and Coal Products Manufacturing (NAICS 324).

[18]Note that a sub-sample of this group is defined as the "treated group" in the analysis to follow.

cated in an MSA. Importantly, employment is falling faster in attainment counties than it is in switching counties prior in the pre-period (1998-2003). Table 1 shows that across the United States there is a decline in workers, the number of establishments and workers per establishment.[19] Figure A1 plots the employment change for "dirty" and "non-dirty" industries in both attainment counties and counties that switched to nonattainment relative to their employment in 2003. Again, consistent with the selection process, "Dirty" industries in counties switching to nonattainment experience the least employment loss prior to the regulations. Table 2 quantifies these differences by running a naive difference-in-difference estimator on "dirty" industries. Specifically, it interacts an indicator variable for whether a county-industry switches to nonattainment with an indicator variable for whether the observation is after the 2004 start date. The *PostxSwitch* interaction term implies that employment was between 12 and 14 percent *higher* in switching counties relative to nonattainment. Figure A1 demonstrates that the common trends assumption required for differences-in-differences estimates to be causal is clearly violated. This motivates the matching differences-in-differences estimator described below.[20]

4.1 Identifying Assumptions

The key identifying assumption is that matching on observable covariates is able to remove biases that may be present in standard difference-in-differences estimates due to selection into the treatment. Specifically, it is assumed that the outcome of the control group, conditional on observable characteristics (historic employment trends, MSA status, NO_x emissions, NAICS 3-digit industry and Census Division) is the same as the outcome of the treated observations were they not to have entered nonattainment.

As previously mentioned, there are two distinct advantages of this method. While most past research on nonattainment standards has been forced to make parametric assumptions about the relationship between the outcome variable and the covariates (generally a set of fixed effects), the above estimator needs no such assumptions. Most importantly, the counterfactual is intentionally constructed to mirror the treated observations based on observable pre-treatment characteristics.

[19] It should be noted that the decline in workers per establishment is driven by changes in the capital-labor ratio rather than a decline in average establishment output.

[20] Table A4 shows results from a DDD which the relative change in dirty industries in switching and non-switching counties relative to the change in non-dirty industries in switching and non-switching counties. Figure A1 shows that non-dirty industries are also unlikely to be a relevant counterfactual as they are trending down faster than dirty industries. Indeed, the non-dirty attainment observations are falling faster than any other category. The large decline in the non-dirty attainment category pushes the DDD results to be far smaller in magnitude thatn the DD results.

Before moving to the results it is also important to note that the estimates are obtained for a specific set of observations. The results use data from county-industries with over 50 workers that are entering nonattainment. These estimates do not speak to changes in employment that may be occurring in counties whose initial employment level is low. By excluding these observations, the analysis focuses on the impact of nonattainment on regions with established workforces. This is a population that is of particular interest. However, there are also potential impacts of the policy along other dimensions that will not be observed. For example, if a firm is deciding where to locate a new plant, the NAAQS may result in them opening up that facility in an attainment county that currently has fewer than 50 workers in the industry. This new plant creation will not be picked up in the estimate reported in the model below. New plant births are an important and policy relevant dimension worthy of study, but given the capital intensive nature of polluting plants, new plant births are rare and it is likely that the largest impact will be on locations with an existing workforce.

5 Results

5.1 Balancing Tests

The first step in the analysis is to explore the degree to which the nearest neighbor matching process successfully constructs an appropriate counterfactual. Table 1 provides summary statistics for all county-industries that switch from attainment to nonattainment and all county-industries that stay in attainment. As discussed earlier, these observations look quite different based on key observables. Table 3 provides summary statistics for all observations that are part of the treatment group and for those observations which have been selected as matches based on their propensity scores. The treatment group differs slightly from the switchers because all observations in the Ozone Transport Region have been dropped and only observations with greater than 50 employees in 2000 are kept. As a whole, the characteristics of the constructed counterfactual now closely resemble the characteristics of the treatment group. The remaining difference between the two groups is not statistically different from zero. Pre-employment trends, MSA status and NO_x emissions per worker are all quite similar. The difference in pre-treatment employment levels is not statistically different from zero either but given that the magnitude of the difference appears larger than expected, it is worth exploring this difference in more detail.

To better understand what might be driving the remaining differences in employment levels and to visualize how the nearest neighbor matching process adjusts the counterfactual, consider Figures 3a and 3b. Figure 3a displays the kernel density of employment for universe of switchers and non-switchers and Figure 3b displays the same for treated observations and the constructed counterfactual observations. Note that the switchers have far fewer low employment observations than the non-switchers (the universe of potential controls). This is not surprising, given that the regulation was far more likely to hit counties in metro areas with a large NO_x emitting plants. After the nearest neighbor match has been performed, the set of constructed control observations looks much more similar to the switchers in the treatment group.

One remaining difference is the right tail of the distribution. There are a few county-industries in the treatment group that have very high employment. The difference in the right tails of the distribution explains much of the remaining difference between average employment in the control and treatment groups in Table 3. The model matches well on the remaining variables. Similar pre-treatment employment trends are also crucial to the validity of the employment effect and will be examined more in the coming sections.

5.2 Nearest Neighbor Matching Results

Results for the baseline nearest neighbor matching estimator are found in Tables 4, 5 and 6 and are visualized in Figure 4. Table 4 presents the estimated effect on employment, Table 5 on number of establishments and Table 6 on establishment size. The rows track the percentage change in the outcome variable since 2003 relative to the constructed counterfactual. Providing results for each post year for ten years provides an understanding of the dynamics of the effect. Each column of the table corresponds to a different set of matching variables. The tables all have a similar layout. Each cell in the table represents the estimated treatment effect of entering nonattainment status from a particular nearest-neighbor matching estimator. The four columns of each table match on different sets of potential control variables, with each column adding additional matching variables. Column 1 matches on pre-treatment trend in the key outcome variable and an MSA indicator. Column 2 additionally matches on 2000 employment level. Column 3 matches on all variables in Column 2 as well as total NO_x-employment ratio of the observation. Column 4 is identical to Column 3 but replaces the NO_x-employment ratio with the percentage change in the county's overall employment level. Important trade-offs are made when choosing which and how many variables to match on. We treat change in the outcome variable and employment level as the key matching variables. As additional matching variables are included, the group of nearest-neighbors will be less likely to resemble the treated observation on these key matching variables. However, they will be closer to the treated observation along these new dimensions which are also reasonable indicators of match quality. As discussed earlier, the primary advantage of the nearest-neighbor matching estimator is that it creates a counterfactual with similar pre-trends and similar size to the treated observations. Finally, the specifications impose a blocking condition whereby a treated observation can only be matched to observations in the same industry and same Census Division as mapped in Figure 2.[21]

Consider column 1 of Table 4. The coefficient in the first row of column 4 can be interpreted to mean that employment in 2004 was 0.84% higher in the treated observations than it was in the counterfactual observations that were generated from the the nearest-neighbor matching estimator. The second row shows that employment was 0.99% higher by 2005. This number begins to rise and by 2008, employment is 6.19% higher in county-industries switching into nonattainment than it was in their constructed counterfactual. Following 2008 this number falls, such that the employment change in treated and control counties is nearly identical. Columns 2-4 match on other variables but the overall results are consistent with Column 1. Starting in 2006, counties switching to nonattainment experience an increase in employment relative to the counterfactual. This relative employment increase in treated counties begins to dissipate in 2009 and the effect is close to zero and statistically insignificant by 2013. Many of the regulations associated with nonattainment designation are not fully enforced at the date of designation. As discussed in Ferris *et al.* (2013), it often takes three years for states to develop a State Implementation Plan and have it approved by the EPA. While speculative, the uncertainty literature developed Bloom (2009) provides another potential mechanism. Regulatory uncertainty could explain the temporary relative increase in employment as firms may have delayed making major adjustments until after they knew what the effects of the policy would be and whether there would be the need to employ workers in abatement activities. Given the cost of hiring new workers, having this option value available may have been worth the costs of temporarily holding on to workers. Once the uncertainty of the regulation was resolved, they resumed employment adjustments accordingly.

Table 5 presents the effect of the nonattainment standards on the number of establishments in industries affected by the ozone nonattainment standards. The table demonstrates that the number of establishments begins to decline in 2008 and stays lower in treated counties relative to their counterfactuals. Given that the effect begins in 2008, we test for whether this effect is merely driven by the recession in the falsification tests. While the recession is likely to have interacted with the regulations to have created the response, results from the falsification test demonstrate that the effect is *not* driven by the recession. Table 6 reports

[21] Other specifications which loosened the geographic restrictions to the broader Census Region level provided similar results, as did other specifications that matched on changes in the county's "clean" manufacturing employment changes and *overall* NO_x emissions.

the treatment effect estimates on establishment size. By 2007, average establishment size in counties entering nonattainment is between 4 and 6% larger than counterfactual observations. In some specifications this figure rises to as much as 11% higher and by 2013 all specifications show that establishment size has increased by 8-9% relative to counterfactual observations.

To better visualize the baseline results, Figure 4 plots out the treatment effect estimates for each of the outcome variables by year. The results demonstrate the patterns discussed above and provide the 90% confidence intervals for each of the estimates. The figures are directly created using the estimates in column 4 of each of the result tables. One key difference between the tables and Figure 4 is the inclusion of "lead" estimates for the years 1998-2002. These estimates simply use the percentage change in the outcome variable for each of the years 1998-2002 relative to the variable in 2003. These estimates should not be interpreted as anticipatory effects of the program. By design of the matching estimator, which selects counterfactual observations in part based on them having similar pre-treatment trends to the treatment, these estimates are expected to be near zero. They are useful for visualizing the extent to which the other matching variables (employment levels, NO_x-employment ratios, MSA designation and the forcing variables) pull the trend in the counterfactual observations away from zero. It is reassuring to see that all of the estimates, dating back to 1998 are small and that zero is always within their confidence intervals.

Figure 5 plots a similar figure to those in Figure 4 but examines the effect of nonattainment status on the number of establishments in particular establishment size bins. In many ways, this figure helps explain the effect of the nonattainment standards on each of the three outcome variables examined in Figure 4. The plot shows declines in the number of small establishments. The number of establishments with 1-49 workers and the number of establishments with 50-99 workers falls relative to the counterfactual. The number of establishments in the 100-499 employment category remains constant and the number of establishments with over 500 workers increases. This is consistent with the results in Figure 4 which show temporary increases in employment, persistent declines in the number of establishments and a persistent increase in average establishment size. There are fewer smaller establishments following the regulation, no effect on large establishments and an increase in the number of very large establishments. Despite there being heterogeneity in the sign of the treatment effect across establishment sizes, the overall number of establishments falls, because there are more small and medium establishments than there are very large establishments. The plots demonstrates that the nonattainment standards changed the establishment size distribution in affected regions and suggests that large establishments gained relative to small establishments.[22]

5.3 Falsification Tests

Although the results above provide strong evidence of a causal effect of entering nonattainment status, there still may be concern that some other unobserved difference between the treated observations and the controls is driving the difference in post-treatment outcomes. One potential confounder is the recession, which has been shown to have had heterogeneous effects across the United States. If there is an unobserved variable that is correlated both with attainment status and the extent to which counties' economies fluctuate with business cycles then the results may not be picking up the causal effect of entering nonattainment status. As a falsification test, I run Specification A again. But this time, the outcome variables are employment, number of establishments and establishment size of the six manufacturing industries with the *lowest* NO_x intensity rather than the six industries with the highest NO_x intensity.[23] The idea here is to perform an indirect test of unconfoundedness. If there is some unobserved characteristic that is driving manufacturing employment trends in treatment counties to be different than employment trends in the constructed counterfactual then this falsification test will find similar results. However, if the results of the falsification test show the treatment to have no effect then this bolsters the argument that the baseline result is capturing the treatment effect of the nonattainment standards rather than the effect of an unobserved confounding variable on manufacturing employment.

Table 7 presents the results of the falsification test. It runs the same specifications found in column 4 of tables 4, 5 and 6 but the outcome variables are for the six NAICS industries least likely to be affected by the nonattainment standards. As seen in the table, clean industries in nonattainment counties experience relatively little change in the outcomes of interest relative to their constructed counterfactual. The full set of falsification results for all specifications is shown in the appendix tables A1, A2 and A3. This falsification test should not be interpreted

[22] A few caveats should be made here. First, these estimates are not tracking establishments over time. Because of this, there is endogeneity in the establishment-size categories whereby changes in the number of establishments in one size category affects the number in other categories. For example, the number of establishments in the 50-99 category could shrink either because those establishments in that size category close or because they shrink and enter the 1-49 category. Second, as with all of the estimates, the number of establishments is falling in both the treatment and counterfactual, so the gains in the number of establishments is relative to the constructed counterfactual. Finally, standard errors are relatively large on the 500+ category. Only one of the estimates is significant at even the 10% level. Nonetheless, this plot provides important insights into the asymmetric response by establishment size.

[23] Note that for employment and establishment size, the effect begins before the recession took place. The establishment results begin simultaneously with the recession. As a result, the falsification test is particularly useful for validating the establishment estimates.

to mean that the recession had no interaction effect with the regulation's effect on the outcome variables. Rather, it serves as an assurance that the estimated treatment effects are not being driven by the regulation alone. It is still possible, and perhaps likely, that the effect of the regulations would depend on the business cycle. Some research has been performed to examine this interaction effect.[24]

6 Discussion

These results mark some of the first in-depth analysis of the recent tightening of the NAAQS ozone standards. This is an enormously important regulation that is claimed to have significant costs to workers and industries. This research suggests that, after accounting for selection concerns, the true effects are more complicated and that a major source of the costs of regulation have been overlooked. The first finding is that, at least in the ten years following the expansion, entering into nonattainment status for ozone does not appear to have resulted in employment declines. In fact, once an appropriate counterfactual has been created employment temporarily increased (or shrank less) in nonattainment counties relative to attainment counties. However, the regulation has notable effects on industry composition, with the number of establishments in affected regions falling and the remaining establishments growing in size. These empirical results are in line with research by Ryan (2012) which uses a structural model to recover the cost structure for a single industry and finds that the increased investment and entry costs associated with complying with the nonattainment standards, creates new fixed costs that result in larger firms, higher market concentration and reduced consumer surplus.[25] Falsification tests which run identical specifications on the six cleanest industries find no effect of the program, strengthening the validity of the estimated treatment effects.

The employment results are not in line with the industry view that these regulations are "job-killers." However, there are important consequences to the regulations that affect firms and are likely to affect consumers. By increasing fixed and entry costs, the regulations appear to change the market structure of the industry in a way that favors large firms. This shift is likely to increase market power of firms and result in lowered surplus for consumers. Past research has found that increases in these types of fixed costs have notable effects on new firm entry (List *et al.* 2003). However, these fixed costs can largely be recouped by incumbents

[24]See Fischer & Heutel (2013) for a review of this topic.

[25]Interestingly, the firm size estimates from the structural model in Ryan (2012) are very similar to the establishment size estimates obtained in this paper.

due to their now increased market power (Ryan 2012; Fowlie *et al.* 2016). The results of this paper fit with this more complex understanding of the costs of regulations.

7 Conclusion

This paper discusses in detail the process by which counties are selected into nonattainment and provides some of the first evidence on the recent expansions of the ozone nonattainment standards using econometric techniques that account for selection concerns. There remains much space for future research to be conducted. While overall employment was not affected there may still be important worker costs for the smaller establishments that were affected. Other outcomes such as business dynamism, changes in productivity and productivity dispersion and changes to the production process are all important to understand. Many of these outcomes will assist in our understanding of the true net benefits of the nonattainment standards.

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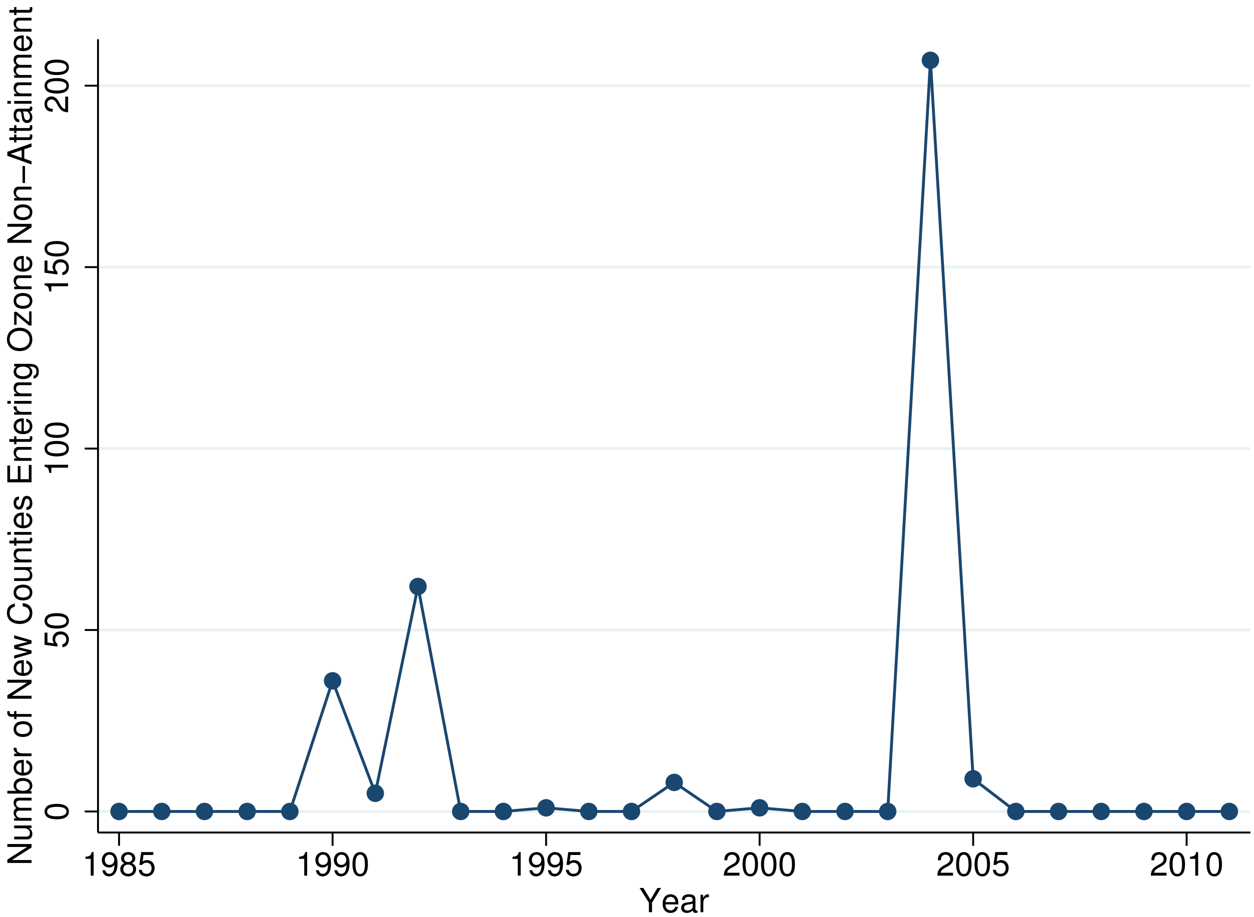
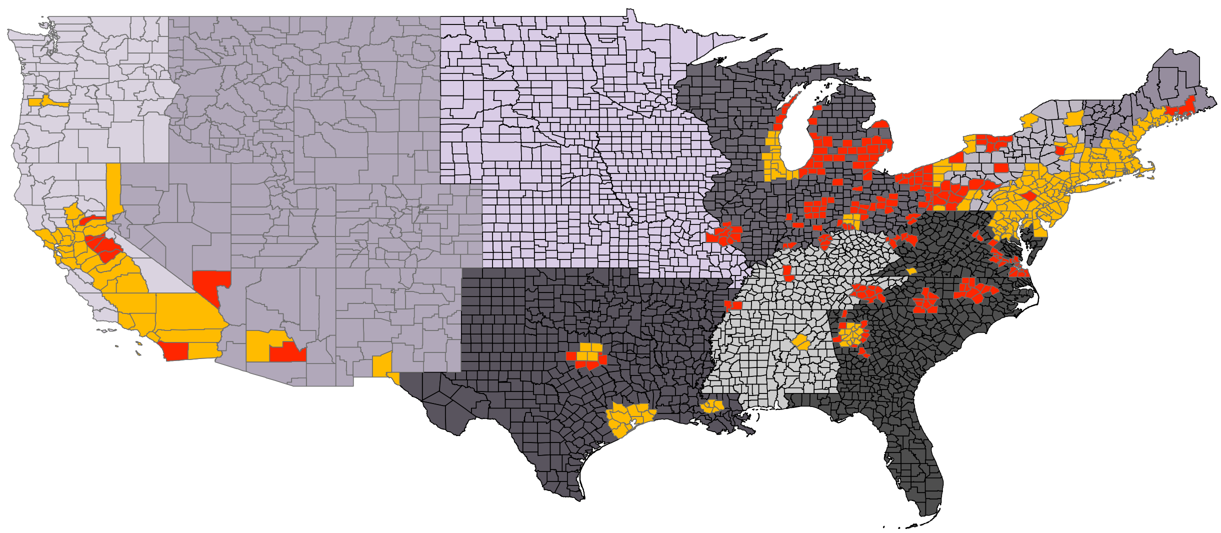
Figures and Tables

Figure 1: Newly Designated Ozone Nonattainment Counties by Year



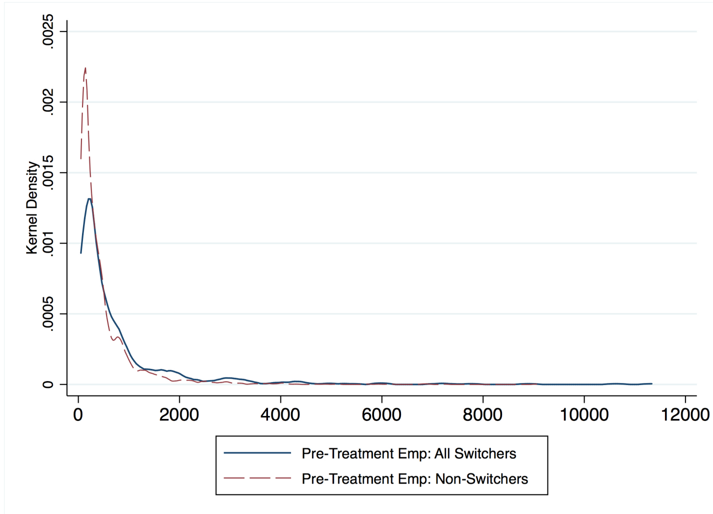
Note: The above figure shows the number of counties entering nonattainment for ozone in every year since 1985. Source: EPA's Greenbook

Figure 2: Ozone Nonattainment Counties and Census Divisions

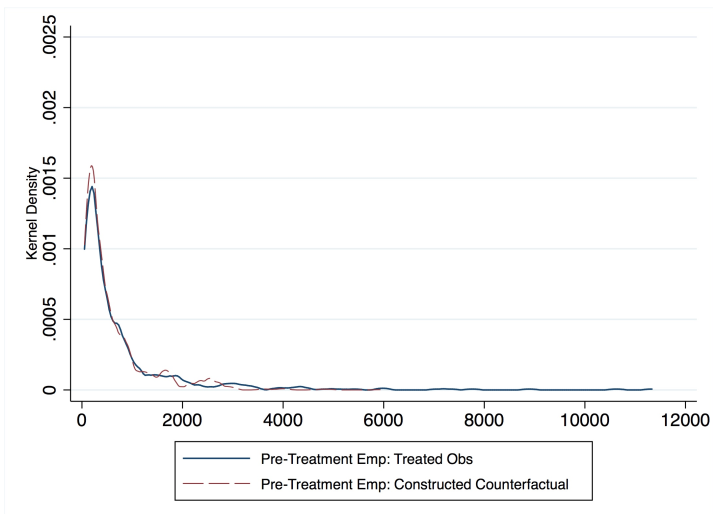


Note: The above figure shows the counties in nonattainment for ozone in 2003 and the counties newly designated as nonattainment in 2004. Counties shaded in yellow were in nonattainment prior to the 2004 expansion. Counties shaded orange entered nonattainment status in 2004. The nine Census Divisions are noted by different gray shades. The counterfactual(s) for each treated observation is required to come from the same Census Division and the same NAICS 3-digit industry.

Figure 3: Employment Distribution: Before and After Matching



(a)

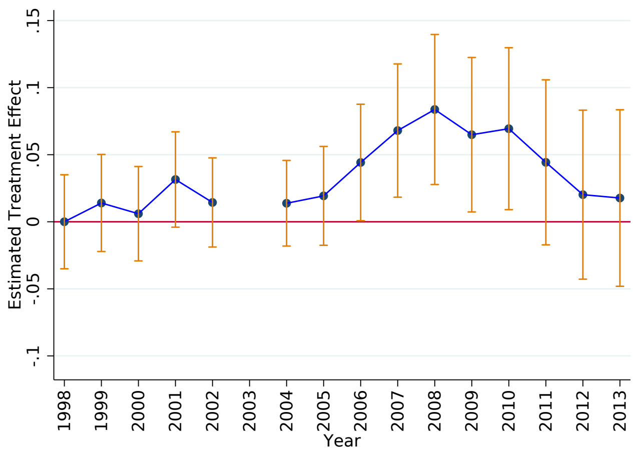


(b)

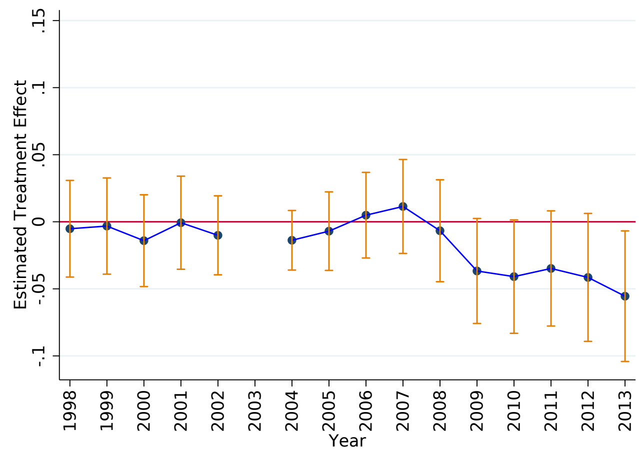
Note: Figure 3a plots the pre-treatment employment distribution of all observations that switched to nonattainment in 2004 and all observations that remained in attainment. Figure 3b plots the pre-treatement employment distribution of all observations defined as treated and the distribution of all observations in the constructed counterfactual.

Figure 4: Employment, Establishment and Establishment Size Response to Ozone NA Regulations

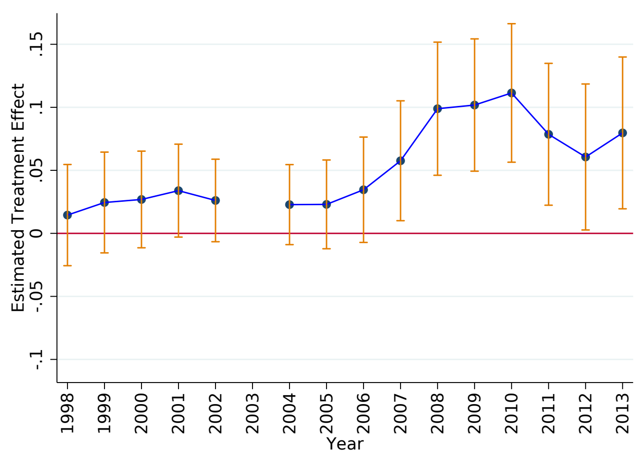
(a) Employment Response



(b) Establishment Response

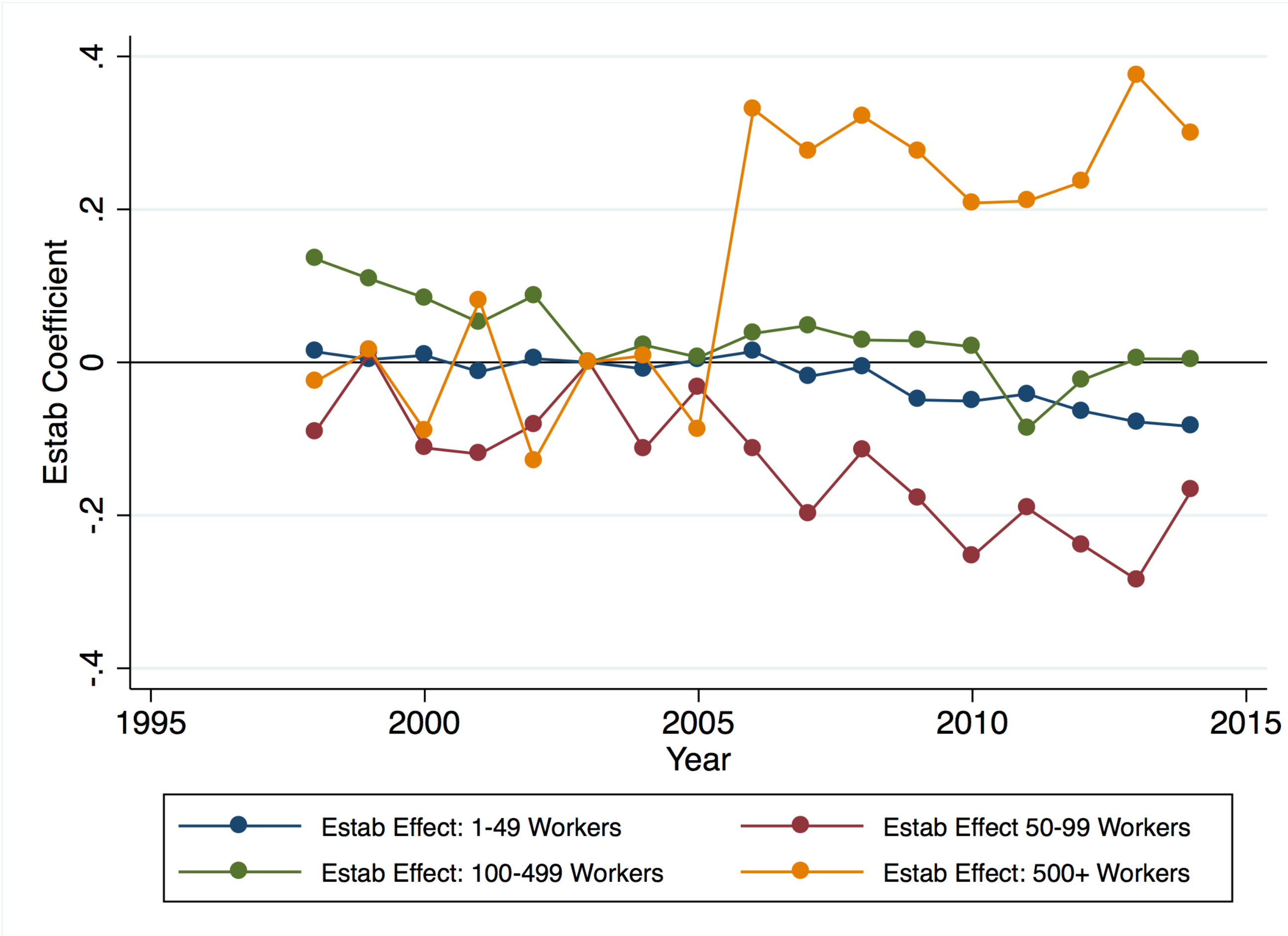


(c) Establishment Size Response



Note: These three figures display the dynamics of the response to entering ozone nonattainment status. The figures are based off of estimates in column 4 of Tables 3, 4 and 5.

Figure 5: Asymmetric Response to NAAQS by Establishment Size



Note: The above figure plots the coefficients on models where the outcome variable is the number of establishments in one of the four establishment size categories listed above. Note that the overall establishment results in Figure 4b are driven far more by the smaller establishment size categories than they are by the larger categories. Also, the results are not tracking the same establishments over time. Rather they are tracking the number of establishments in each size category relative to the constructed counterfactual. Again an increase in the number of large establishments is relative to constructed counterfactual and does not imply a net increase of the size of the coefficient.

Table 1: Summary Statistics: Ozone Switchers and Non-Switchers

	(1) Attainment	(2) Switchers	(3) All
Employment 2000	219.629	541.448	251.291
	(463.385)	(1049.207)	(557.644)
Establishments 2000	4.032	8.572	4.478
	(6.371)	(12.108)	(7.268)
Estab Size 2000	64.327	73.453	65.245
	(134.069)	(140.517)	(134.750)
NOx Emissions from Major Sources	98.551	146.919	103.310
	(576.768)	(789.599)	(601.170)
NOx-Emp Ratio	0.383	0.568	0.401
	(5.505)	(7.359)	(5.714)
MSA	0.277	0.865	0.335
	(0.447)	(0.342)	(0.472)
Percent Non-Dirty MFTG Emp change (2003-2013)	-0.002	-0.004	-0.002
	(0.176)	(0.167)	(0.176)
Percent Emp Change (1998-2003)	-0.294	-0.196	-0.285
	(0.880)	(0.742)	(0.868)
Percent Emp Change (2003-2013)	-0.262	-0.249	-0.260
	(0.968)	(0.810)	(0.953)
Percent Est Change (2003-2013)	-0.203	-0.199	-0.203
	(0.816)	(0.635)	(0.800)
Percent Estab Size Change (2003-2013)	-0.086	-0.080	-0.085
	(0.698)	(0.645)	(0.692)
Observations	7,148	780	7,928

Note: The above table provides summary statistics for all "dirty" county-industries that switched into nonattainment for ozone in 2004 and for all county-industries that were not subject to nonattainment between 2000 and 2013. The final column gives summary statistics for all county-industries.

Table 2: Naive Difference-in-Difference Estimator

Year	(1) ln(Emp)	(2) ln(Emp)	(3) ln(Emp)	(4) ln(Emp)
PostxSwitch	0.1334***	0.1415***	0.1334***	0.1221***
	(0.0334)	(0.0334)	(0.0338)	(0.0339)
Post	-0.5064***	-0.6168***		
	(0.0159)	(0.0307)		
Switch	0.4600***	0.0472***		
	(0.0516)	(0.0497)		
N	72,233	72,150	72,233	72,150
R^2	0.0367	0.1296	0.4472	0.4534
Year FE			Yes	Yes
Cnty FE			Yes	Yes
Controls		Yes		Yes

Note: The four columns report results for a naive Difference-in-Difference estimates where "dirty" industries in switching counties are compared to "dirty" industries in attainment counties before and after the regulation. ***, ** and * indicate significance at the 1,5 and 10 percent levels respectively. County level logged total employment and logged earnings are included as controls. The variables *Post* and *Switch* are absorbed by the inclusion of the fixed effects in columns three and four.

Table 3: Test of Balance: Treatment vs. Counterfactual

	(1) Treatment	(2) Control	(3) Difference of Means
Emp 2000	693.191	562.148	-131.042
	(1116.431)	(962.046)	(1423.135)
ln (Emp 2000)	5.877	5.694	-0.183
	(1.093)	(1.043)	(1.399)
Nox-Emp Ratio	0.272	0.406	0.134
	(1.437)	(5.387)	(1.769)
MSA	0.830	0.831	0.000
	(0.375)	(0.375)	(0.055)
% Emp Change 1998-2003	-0.172	-0.164	0.008
	(0.462)	(0.407)	(0.162)

Note: The above table provides summary statistics for all "Treatment" county-industries, the constructed counterfactual county-industries and the difference in means between the two groups. The difference in means is not statistically significant for any of the variables. Each treated observation, of which there are 557, is matched to its 10 nearest neighbors. But after an observation is selected as a control it re-enters the pool of potential controls for the next treated observation and therefore can be included as a control for multiple treated observations.

Table 4: Employment Results: Average Treatment Effect Using Nearest Neighbor Matching

Year	(1) Employment	(2) Employment	(3) Employment	(4) Employment
2004 (t=0)	.0084	.0082	.0095	.0138
	(0.0177)	(0.0189)	(0.0190)	(0.0194)
2005 (t=1)	.0099	.0134	.0157	.0193
	(0.0217)	(0.0226)	(0.0228)	(0.0224)
2006 (t=2)	.0266	.0436*	.0468*	.0442*
	(0.0266)	(0.0264)	(0.0266)	(0.0264)
2007 (t=3)	.0468	.0633**	.0698**	.068**
	(0.0301)	(0.0304)	(0.0305)	(0.0302)
2008 (t=4)	.0619*	.0748**	.085**	.0837**
	(0.0330)	(0.0342)	(0.0342)	(0.0340)
2009 (t=5)	.0455	.05	.0571	.0649*
	(0.0342)	(0.0349)	(0.0350)	(0.0350)
2010 (t=6)	.0385	.0461	.0536	.0694*
	(0.0355)	(0.0364)	(0.0364)	(0.0367)
2011 (t=7)	.0166	.0207	.028	.0443
	(0.0370)	(0.0374)	(0.0375)	(0.0374)
2012 (t=8)	.0001	.0054	.0105	.0202
	(0.0386)	(0.0388)	(0.0390)	(0.0383)
2013 (t=9)	.0238	.0151	.0191	.0177
	(0.0401)	(0.0405)	(0.0407)	(0.0400)
Treated Obs	557	557	557	557
Controls	3,974	3,974	3,974	3,974
Matching Vars				
Pre Trend	Yes	Yes	Yes	Yes
MSA	Yes	Yes	Yes	Yes
Emp 2000		Yes	Yes	Yes
NOx-Emp Ratio			Yes	
Post Cnty Emp Δ				Yes

Note: The four columns report results for four different sets of matching variables. ***, ** and * indicate significance at the 1,5 and 10 percent levels respectively.

Table 5: Establishment Results: Average Treatment Effect Using Nearest Neighbor Matching

Year	(1) Establishments	(2) Establishments	(3) Establishments	(4) Establishments
2004 (t=0)	-.0137	-.0178	-.0161	-.0142
	(0.0123)	(0.0131)	(0.0132)	(0.0135)
2005 (t=1)	-.013	-.0138	-.0108	-.009
	(0.0170)	(0.0180)	(0.0180)	(0.0179)
2006 (t=2)	.0024	.0028	.0064	.0031
	(0.0183)	(0.0185)	(0.0186)	(0.0193)
2007 (t=3)	.0009	.006	.0076	.0112
	(0.0206)	(0.0211)	(0.0211)	(0.0212)
2008 (t=4)	-.0042	-.0072	-.0047	-.0058
	(0.0223)	(0.0229)	(0.0229)	(0.0230)
2009 (t=5)	-.0382	-.0438*	-.0416*	-.0376
	(0.0244)	(0.0238)	(0.0239)	(0.0238)
2010 (t=6)	-.0472*	-.0469*	-.0448*	-.0404
	(0.0266)	(0.0260)	(0.0258)	(0.0259)
2011 (t=7)	-.0372	-.0401	-.0384	-.0339
	(0.0276)	(0.0263)	(0.0260)	(0.0263)
2012 (t=8)	-.0513*	-.0507*	-.0466	-.0453
	(0.0304)	(0.0287)	(0.0287)	(0.0290)
2013 (t=9)	-.0651**	-.0639**	-.0597**	-.0587**
	(0.0306)	(0.0294)	(0.0295)	(0.0295)
Treated Obs	557	557	557	557
Controls	3,974	3,974	3,974	3,974
Matching Vars				
Pre Trend	Yes	Yes	Yes	Yes
MSA	Yes	Yes	Yes	Yes
Emp 2000		Yes	Yes	Yes
NOx-Emp Ratio			Yes	
Post Cnty Emp Δ				Yes

Note: The four columns report results for four different sets of matching variables. ***, ** and * indicate significance at the 1,5 and 10 percent levels respectively.

Table 6: Establishment Size Results: Average Treatment Effect Using Nearest Neighbor Matching

Year	(1) Estab Size	(2) Estab Size	(3) Estab Size	(4) Estab Size
2004 (t=0)	.0182	.0201	.0198	.0231
	(0.0172)	(0.0186)	(0.0186)	(0.0193)
2005 (t=1)	.0215	.0242	.024	.0236
	(0.0204)	(0.0211)	(0.0213)	(0.0213)
2006 (t=2)	.0191	.0366	.037	.0363
	(0.0253)	(0.0253)	(0.0255)	(0.0253)
2007 (t=3)	.0424	.0586**	.0631**	.0565*
	(0.0291)	(0.0291)	(0.0291)	(0.0289)
2008 (t=4)	.0707**	.0837***	.091***	.0933***
	(0.0315)	(0.0324)	(0.0325)	(0.0321)
2009 (t=5)	.0796**	.0896***	.095***	.1016***
	(0.0323)	(0.0321)	(0.0320)	(0.0321)
2010 (t=6)	.0796**	.0923***	.0982***	.1102***
	(0.0333)	(0.0333)	(0.0333)	(0.0334)
2011 (t=7)	.0481	.0559	.0617*	.078**
	(0.0343)	(0.0345)	(0.0346)	(0.0342)
2012 (t=8)	.045	.0531	.0553	.0643*
	(0.0353)	(0.0359)	(0.0359)	(0.0352)
2013 (t=9)	.0894**	.0827**	.0822**	.0812**
	(0.0365)	(0.0373)	(0.0374)	(0.0364)
Treated Obs	557	557	557	557
Controls	3,974	3,974	3,974	3,974
Matching Vars				
Pre Trend	Yes	Yes	Yes	Yes
MSA	Yes	Yes	Yes	Yes
Emp 2000		Yes	Yes	Yes
NOx-Emp Ratio			Yes	
Post Cnty Emp Δ				Yes

Note: The four columns report results for four different sets of matching variables. ***, ** and * indicate significance at the 1,5 and 10 percent levels respectively.

Table 7: Falsification Test: Clean Industries

Year	(1) Employment	(2) Establishments	(3) Estab Size
2004 (t+0)	-.0137	-.0163	.0022
	(0.0185)	(0.0133)	(0.0174)
2005 (t+1)	-.0278	-.0172	-.0134
	(0.0240)	(0.0171)	(0.0215)
2006 (t+2)	-.0397	-.0119	-.0265
	(0.0275)	(0.0181)	(0.0259)
2007 (t+3)	-.0343	-.0137	-.0192
	(0.0293)	(0.0200)	(0.0276)
2008 (t+4)	-.0168	-.0017	-.019
	(0.0330)	(0.0227)	(0.0308)
2009 (t+5)	.0005	.0063	-.0009
	(0.0350)	(0.0229)	(0.0341)
2010 (t+6)	-.0042	.0016	-.0041
	(0.0354)	(0.0235)	(0.0346)
2011 (t+7)	.0057	.0084	.0005
	(0.0366)	(0.0247)	(0.0355)
2012 (t+8)	.0185	.0428*	-.0261
	(0.0373)	(0.0258)	(0.0359)
2013 (t+9)	.0085	.0285	-.0173
	(0.0376)	(0.0256)	(0.0369)
Treated Obs	557	557	557
Controls	3,974	3,974	3,974
Matching Vars			
Pre Trend	Yes	Yes	Yes
MSA	Yes	Yes	Yes
Emp 2000	Yes	Yes	Yes
NOx-Emp Ratio			
Post Cnty Emp Δ	Yes	Yes	Yes

Note: The above table performs a falsification test by running the same model on the cleanest six manufacturing industries. For brevity, the model is run only on the specification used in column four of tables 3, 4 and 5 which matches on employment, employment trends, MSA status and overall county level employment change. Tables in the appendix run each of the four models for each of the three outcomes for the clean industries only.

A Data Appendix

The text discusses two alternative identification strategies that could be used to identify the impact of the nonattainment standards. The purpose of this paper is not to critique past papers, but it is worth mentioning some broad concerns behind these identification strategies.

The data are at the county-industry-year level. A simple differences-in-differences strategy would look something like this:

$$y_{ckt} = \beta_T(Post_{ct} \times Switch_c \times Dirty_k) + \theta x_{ckt} + \delta_{ck} + \alpha_{kt} + \gamma_{ct} + \epsilon_{ckt} \tag{3}$$

Where Post equals one for every observation after 2004, Switch equals one for any county that enters nonattainment and Dirty equals one for any observation with an industry that is one of the six industries that is classified as "Dirty." A key identifying assumption of this (and any diff-in-diff) model, is that there are common trends prior to the treatment. As discussed in the Greensboro/Winston-Salem example and seen in Figure A1, observations entering nonattainment will in fact have differing trends than those that do not. These differences will not be captured by the industry-year, county-year or even state-industry year trends because the treatment is in part determined based on the trends of a particular county-industry. Results from this DDD estimate is reported in Table A4.

A second potential identification strategy is to exploit policy cutoffs either in geography, by comparing border counties, or in the air quality threshold, by comparing counties just above and just below the threshold. The key assumption behind these discontinuity strategies is that selection into the treatment is as good as randomly assigned around the cutoff. A look into the details of non-attainment designation raises questions about the validity of this assumption for the ozone nonattainment designation. While nonattainment is nominally based on the county's air quality, in practice the EPA is given significant leeway in determining which counties are designated into nonattainment. Areas not meeting the standard may be exempted if they successfully petition EPA that their air quality and emissions levels are trending downward. Conversely, counties which meet the standards may be designated as nonattainment if they emit substantial levels of NO_x and VOC's (the precursors to ozone) that contribute to other counties in their metro area not meeting the standard. As a result of this selection process, industries in counties designated as nonattainment are likely to have considerably different characteristics than those right across the border that are in attainment. In fact, if the two counties were similar in dirty manufacturing activity then both would have been designated as in nonattainment. Whether past papers are under or overstating the size of the effect depends on the exact methodologies they use but the selection process described in Policy section suggests that many of these papers could be understating the true size of the effect of the NAAQS as regulation is more likely to hit counties with larger and more stable industrial activity.

Another concern with any border discontinuity paper is that the policy causes spillovers across borders from the treated area to the control area and that these spillovers artificially inflate the size of the treatment effect. The constructed control group in the propensity score matching diff-in-diff strategy is far less likely to be impacted by spillovers.

Papers that employ these discontinuity methods (Kahn and Mansur) typically only exploit cross-sectional variation in nonattainment status. Like this paper, their data is at the county-industry-year level. But they do not include county-industry fixed effects in their model. Rather, they use county fixed effects. These county fixed effects control for time-invariant differences between overall county employment but do not account for time invariant differences between county-industries. This is important as a county's industrial composition will be determined in part by the degree of regulation the county is subject to. Their results therefore, do not examine *changes* that occur to a county-industry's employment after the county enters nonattainment. One final data difference between Kahn and Mansur and this paper is that this paper excludes northeastern states that were part of the Ozone Transport Region. All counties in these states were already regulated as if they were in moderate nonattainment status.

The diff-in-diff propensity score matching estimator overcomes the issues surrounding the straight-forward diff-in-diff methodology and the border discontinuity strategy. By matching on pre-treatment levels of manufacturing activity and pre-treatment trends in manufacturing activity, the matching estimator ensures that the constructed counterfactual observations are trending similarly to the treated observation.

Table A1: Full Falsification Test: Employment Results

Year	(1) Employment	(2) Employment	(3) Employment	(4) Employment
2004 (t=0)	-.0181	-.0143	-.0141	-.0137
	(0.0187)	(0.0190)	(0.0190)	(0.0185)
2005 (t=1)	-.0347	-.0254	-.0251	-.0278
	(0.0234)	(0.0237)	(0.0237)	(0.0240)
2006 (t=2)	-.0372	-.0318	-.0308	-.0397
	(0.0268)	(0.0273)	(0.0273)	(0.0275)
2007 (t=3)	-.0403	-.0352	-.0338	-.0343
	(0.0291)	(0.0299)	(0.0298)	(0.0293)
2008 (t=4)	-.0174	-.0177	-.0166	-.0168
	(0.0335)	(0.0334)	(0.0334)	(0.0330)
2009 (t=5)	.0135	.0143	.0152	.0005
	(0.0348)	(0.0355)	(0.0355)	(0.0350)
2010 (t=6)	.0083	.0101	.0109	-.0042
	(0.0349)	(0.0358)	(0.0358)	(0.0354)
2011 (t=7)	.0238	.0191	.0197	.0057
	(0.0363)	(0.0370)	(0.0370)	(0.0366)
2012 (t=8)	.0374	.0276	.0289	.0185
	(0.0371)	(0.0377)	(0.0377)	(0.0373)
2013 (t=9)	.0242	.0158	.017	.0085
	(0.0373)	(0.0381)	(0.0381)	(0.0376)
Treated Obs	557	557	557	557
Controls	3,974	3,974	3,974	3,974
Matching Vars				
Pre Trend	Yes	Yes	Yes	Yes
MSA	Yes	Yes	Yes	Yes
Emp 2000		Yes	Yes	Yes
NOx-Emp Ratio			Yes	
Post Cnty Emp Δ				Yes

Note: The four columns report results for four different sets of matching variables. Results are for the six cleanest industries. ***, ** and * indicate significance at the 1,5 and 10 percent levels respectively.

Table A2: Full Falsification Test: Establishment Results

Year	(1) Establishments	(2) Establishments	(3) Establishments	(4) Establishments
2004 (t=0)	-.0193	-.0168	-.0168	-.0163
	(0.0135)	(0.0134)	(0.0134)	(0.0133)
2005 (t=1)	-.0174	-.0192	-.019	-.0172
	(0.0171)	(0.0169)	(0.0169)	(0.0171)
2006 (t=2)	-.009	-.0125	-.0124	-.0119
	(0.0178)	(0.0181)	(0.0181)	(0.0181)
2007 (t=3)	-.0125	-.0143	-.0142	-.0137
	(0.0205)	(0.0212)	(0.0212)	(0.0200)
2008 (t=4)	.0043	-.0025	-.0022	-.0017
	(0.0238)	(0.0233)	(0.0233)	(0.0227)
2009 (t=5)	.0234	.0187	.0189	.0063
	(0.0229)	(0.0234)	(0.0234)	(0.0229)
2010 (t=6)	.0206	.0126	.0126	.0016
	(0.0242)	(0.0244)	(0.0244)	(0.0235)
2011 (t=7)	.0223	.0157	.0155	.0084
	(0.0251)	(0.0257)	(0.0258)	(0.0247)
2012 (t=8)	.0519**	.0492*	.0493*	.0428*
	(0.0261)	(0.0265)	(0.0266)	(0.0258)
2013 (t=9)	.037	.032	.0323	.0285
	(0.0261)	(0.0266)	(0.0266)	(0.0256)
Treated Obs	557	557	557	557
Controls	3,974	3,974	3,974	3,974
Matching Vars				
Pre Trend	Yes	Yes	Yes	Yes
MSA	Yes	Yes	Yes	Yes
Emp 2000		Yes	Yes	Yes
NOx-Emp Ratio			Yes	
Post Cnty Emp Δ				Yes

Note: The four columns report results for four different sets of matching variables. Results are for the six cleanest industries. ***, ** and * indicate significance at the 1,5 and 10 percent levels respectively.

Table A3: Full Falsification Test: Establishment Size Results

Year	(1) Estab Size	(2) Estab Size	(3) Estab Size	(4) Estab Size
2004 (t=0)	.0025	.0058	.0058	.0022
	(0.0174)	(0.0174)	(0.0174)	(0.0174)
2005 (t=1)	-.0174	-.005	-.005	-.0134
	(0.0205)	(0.0215)	(0.0215)	(0.0215)
2006 (t=2)	-.0246	-.0162	-.0159	-.0265
	(0.0254)	(0.0259)	(0.0259)	(0.0259)
2007 (t=3)	-.0226	-.0131	-.0123	-.0192
	(0.0273)	(0.0279)	(0.0279)	(0.0276)
2008 (t=4)	-.0248	-.0155	-.0149	-.019
	(0.0305)	(0.0309)	(0.0308)	(0.0308)
2009 (t=5)	-.0067	.0018	.0019	-.0009
	(0.0343)	(0.0344)	(0.0343)	(0.0341)
2010 (t=6)	-.0141	-.0057	-.0051	-.0041
	(0.0346)	(0.0345)	(0.0345)	(0.0346)
2011 (t=7)	.0013	.0052	.0053	.0005
	(0.0358)	(0.0357)	(0.0357)	(0.0355)
2012 (t=8)	-.0178	-.0197	-.0193	-.0261
	(0.0356)	(0.0359)	(0.0359)	(0.0359)
2013 (t=9)	-.0081	-.01	-.0098	-.0173
	(0.0368)	(0.0371)	(0.0371)	(0.0369)
Treated Obs	557	557	557	557
Controls	3,974	3,974	3,974	3,974
Matching Vars				
Pre Trend	Yes	Yes	Yes	Yes
MSA	Yes	Yes	Yes	Yes
Emp 2000		Yes	Yes	Yes
NOx-Emp Ratio			Yes	
Post Cnty Emp Δ				Yes

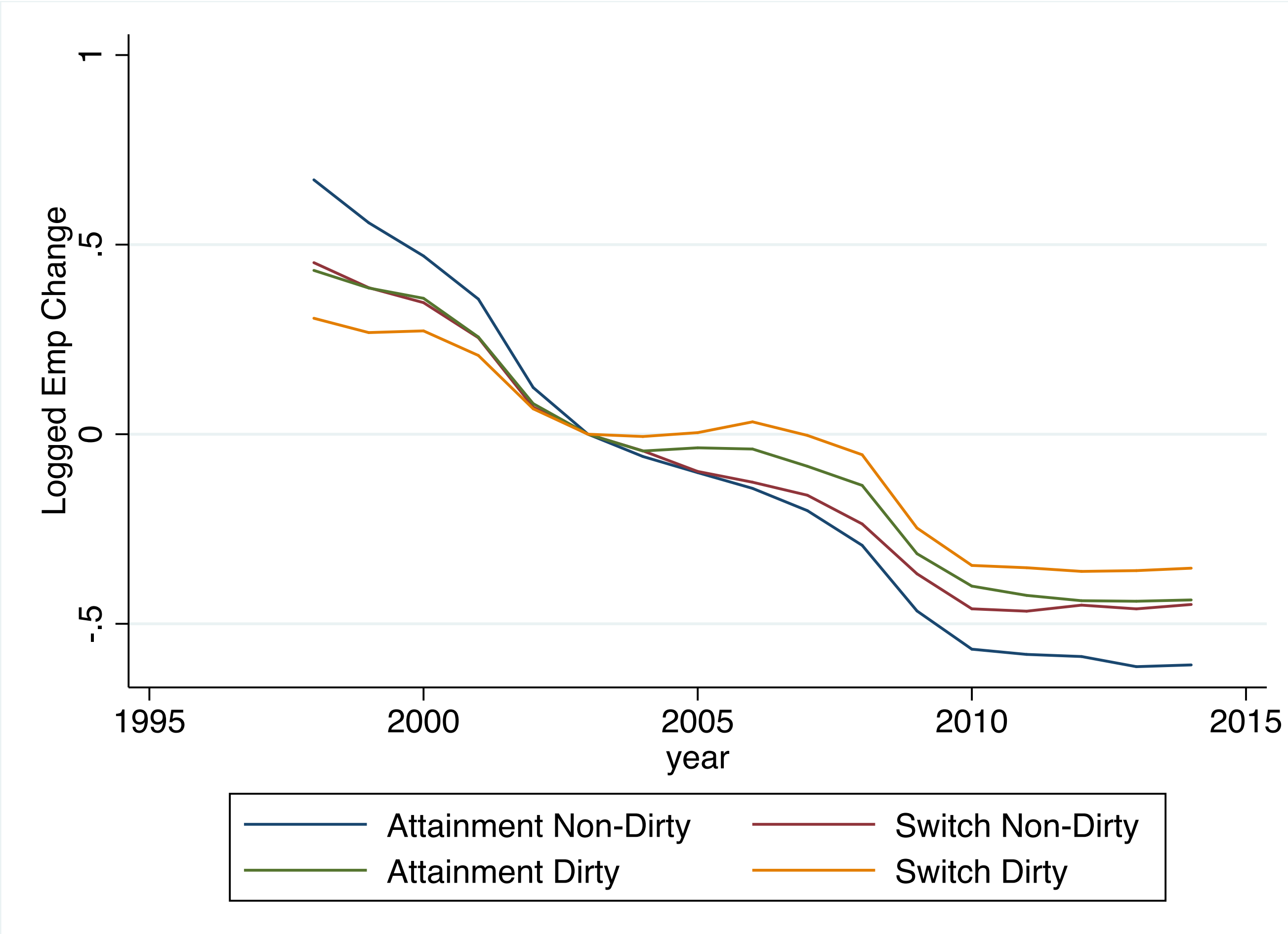
Note: The four columns report results for four different sets of matching variables. Results are for the six cleanest industries. ***, ** and * indicate significance at the 1,5 and 10 percent levels respectively.

Table A4: DDD Estimator

	(1) ln(Emp)	(2) ln(Emp)	(3) ln(Emp)	(4) ln(Emp)	(5) ln(Emp)	(6) ln(Emp)	(7) ln(Emp)
PostxDirtyxSwitch	-0.0592	-0.0385	-0.0312	-0.0467	-0.0451	-0.0136	-0.0121
	(0.0454)	(0.0436)	(0.0434)	(0.0468)	(0.0468)	(0.0440)	(0.0440)
N	257,924	257,924	257,685	252,637	252,458	252,637	252,458
R^2	0.7806	0.8032	0.8102	0.8257	0.8254	0.8446	0.8444
Cnty-Ind FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes						
Ind-Year FE		Yes	Yes			Yes	Yes
Cnty-Year FE				Yes	Yes	Yes	Yes
Controls			Yes		Yes		Yes

Standard errors in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Note: These results are obtained from a differences-in-differences-in-difference model that identifies the effect effect of nonattainment by comparing the relative change in dirty industries in switching and non-switching counties to the relative change in dirty and non-dirty industries in non-switching counties. Based on their differing pretrends (seen in Figure A1), it is difficult to justify using non-dirty industries as a relevant control group.

Figure A1: Employment Change for Attainment and Switching Counties



Note: This chart plots logged employment for four categories of manufacturing relative to their 2003 levels. Note that employment in dirty industries in counties switching to nonattainment is falling at the slowest rate of any of the other categories. It is also clear that non-dirty industries in switching counties are trending differently that dirt industries in switching counties.