The Employment Impact of Tightening the Ozone Non-Attainment Standards: Evidence from the 2004 Expansion
Work In Progress

Mark Curtis
Wake Forest University
curtisem@wfu.edu

November 2015

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 and a variety of limitations were placed on newly constructed and expanding plants. This paper examines the impacts of these regulations on manufacturing employment levels in regulated counties using a nonparametric differences-in-differences matching estimator. While most previous research has found environmental regulation to have a negative impact on employment levels, this paper finds that employment in polluting industries slightly rose in counties designated as nonattainment relative to control counties with similar attributes. The results demonstrate the importance of choosing an appropriate counterfactual when selection into the treatment is endogenous.

I thank Spencer Banzhaf, Stephen Holland, Ron Shadbegian, Olivier Deschenes, participants at the 2015 AERE Summer Conference and IZA Employment and the Environment Workshop for helpful comments. Computing resources were provided by Cornell's Social Science Gateway which is supported through NSF grant #1042181.

1 Introduction

The Environmental Protection Agency's ozone non-attainment standards 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 reducing levels of ground level ozone, industry groups decry them for the extra production costs they impose on pollution emitting establishments. Chief among the concerns raised by opponents of these standards is that they harm labor markets, destroy jobs and result in significant costs to workers and firms.

Economists have played an important role in identifying both the health benefits and the employment and productivity costs of these standards. The large majority of the research has thus far examined the original implementation of the NAAQS that began in the 1970's (Greenstone 2002; Greenstone *et al.* 2012; Isen *et al.* 2014) and a relatively small revision of the standards that occurred in 1990 (Walker 2013; 2011; Ferris *et al.* 2013). The more recent 2004 expansions of the NAAQS have received quite limited attention by researchers.[2] This despite the fact that nearly three times as many counties entered into ozone nonattainment status in 2004 than entered during the 1990 expansion. Understanding the costs and benefits of changing this standard is of considerable interest to policy makers who are required by law to reevaluate the standard every five years and are currently debating whether to further lower the ozone nonattainment standard below it's current level of 75 parts per billion (PBB). At levels currently being discussed by the EPA as many as 400 new counties may enter into nonatainment for ozone.[3]

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 is new or expanding plant, also must undergo a lengthy "New Source Review" process and is required to obtain offsets for every new ton of emissions they produced.

These standards were costly. Research using establishment level data has found that plants

[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] One important exception is (Kahn & Mansur 2013) which examines the role of electricity prices, right-to-work laws and the NAAQS on county employment levels.

[3] This is based off of a 65 PBB standard and the EPA's county-level 2012-2014 design values for ozone found at `http://www3.epa.gov/airtrends/values.html`.

in nonattainment counties had higher overall costs, spending considerable money on both capital and non-capital inputs in order to comply with these standards. 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.[4]

While it is clear that these regulations increased costs to regulated facilities, their impact on employment levels is theoretically ambiguous (Morgenstern *et al.* 2002; Berman & Bui 2001). To comply with these regulations plants must hire new workers to install and maintain their new capital and to monitor their now altered production process. However, as costs go up, plants may downsize and potentially relocate production to non-regulated areas. Empirical work is particularly important to determine which of these competing effects wins out. The sign and magnitude of regulation's impact on employment are important to economists and policy makers seeking to better understand the costs and benefits of the program.

However, empirically estimating the impact of nonattainment standards is complicated by a variety of endogeneity concerns. Counties that enter into nonattainment will most certainly have different characteristics than those that do not. As discussed later in the paper, nonattainment counties are more likely to be located in a metropolitan area and have a larger manufacturing presence than counties that are in attainment. They are also likely to have differing employment trends. Past research on the 1977 CAAA and the 1990 CAAA has used a differences-in-differences strategy with plant level data (Greenstone (2002), Greenstone *et al.* (2012), Walker (2013)). The key identifying assumption for these studies is that, conditional on a set of observable characteristics and fixed effects, inclusion in the treatment group is as good as randomly assigned.

The only research to explore the more recent expansion of the NAAQS is Kahn & Mansur (2013). This paper uses a border discontinuity method to understand the role of right-to-work laws, electricity prices and the ozone nonattainment standards in determining employment levels. Their strategy relies on the assumption that variation in these measures is as good as randomly assigned at the border. This assumption is quite reasonable when considering impacts of right-to-work laws and electricity prices which are largely determined by predefined state borders. However, nonattainment status is assigned at the county level and is often determined by the amount of emissions from plants in the county and emission trends. In fact, borders are specifically defined by the EPA so that counties with significant polluting activity are included and the counties without polluters are not.

[4]Publicly available PACE data shows that manufactures spend $8.6 billion every year on pollution abatement activities with 48% of those expenditures going towards worker wages.

This paper employs a different strategy to identify the impact of nonattainment status. Rather than rely on exogeneity around county borders or making parametric assumptions about common geographic or industry trends, I use a non-parametric differences-in-differences matching estimator that, for every observation entering nonattainment for ozone, finds observations that are "nearest neighbor(s)" to serve as the counterfactual. Nearest neighbors are chosen based on a variety of pre-treatment characteristics including pre-treatment employment levels, pre-treatment employment trends, level of emissions and geographic proximity to the treated observation.

This identification strategy overcomes endogeneity concerns by ensuring that the counterfactual observations closely mirror the treated observations. This is a major advantage over other potential identification strategies. Consider further the border-pairs strategy, where selection into the treatment is assumed to be randomly assigned at the border. 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. Metro 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 have considerably different characteristics than those that are in attainment.

The recent cases of the Greensboro / Winston-Salem and Atlanta metro areas exemplify this selection process. Based on their air quality readings the Greensboro / Winston-Salem metro area was originally slated to enter nonattainment status for ozone in 2004. However, the EPA, following a request by the state of North Carolina, determined that Winston-Salem-Greensboro would not be designated as nonattainment for ozone, specifically citing the downward trend in emissions of the area.

In Georgia the EPA did just the opposite. The state of Georgia requested to the EPA that only the four counties with ozone levels above the attainment standard be designated as nonattainment. However, the EPA designated 15 counties in the metro area as non-attainment for the new standard and explicitly stated that these counties were selected precisely because their high level of NO_x and VOC emissions contributed to a violation of the ozone NAAQS in the four counties which did not meet the standard (EPA 2008).

Not surprisingly, summary statistics display substantial differences between industries in non-attainment counties and the industries in the attainment counties. In short, employment, NO_x emissions and employment trends are significantly higher in industries in non-attainment counties compared to industries in the attainment counties that border them. Differences-in-Differences identifies the impact of the policy using different assumptions but is susceptible to a similar critique when using county-industry data. Control observations will have different characteristics than the treated observations which the inclusion of county and industry trends may not fully account for. Of most importance is accounting for trends occurring in a particular county-industry.

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).[5] I gather employment data from the County Business Patterns and NO_x emissions data from the National Emissions Inventory dataset. 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 employment size, pre-treatment 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 employment in county-industries that entered into non-attainment actually grew (shrank less) relative to employment in the constructed counterfactual. Using this methodology the results show that employment in NO_x-emitting industries in counties that entered non-attainment was conservatively 3-6% higher than their relevant counterfactual ten years after entering nonattainment. Previous work has shown that regulated plants face considerable capital and labor costs to comply with environmental regulations Becker & Henderson (2000); Becker (2005) an the potential for regulations to have a positive impact on employment has been previously discussed in work by Berman & Bui (2001) and 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 this is an important finding, caution should be taken in interpreting the results. First, this is a short to medium run effect that captures only changes that occurred in the 5-10

[5] This technique has been used more recently by Fowlie *et al.* (2012), Banzhaf & Walsh (2008), Gray *et al.* (2014) and Petrick & Wagner (2014) among others. Gray *et al.* (2014) and Petrick & Wagner (2014) use the technique to identify the impact of environmental regulation.

years following the regulation. 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. Next, this is only capturing the direct effect of nonattainment status on manufacturing employment. NO_x regulations will also have an indirect impact on manufacturing employment if the regulations increase electricity prices (Curtis 2014; Deschenes 2010).

There are a number of reasons why the 2004 expansion may have different impacts than previous expansions of the ozone nonattainment standards. Economic conditions in the manufacturing sector were quite different in 2004 than they were in 1977 and 1990, the other two periods of significant increases in the number of nonattainment counties. For example, in 2004 plants were operating at near full capacity and were perhaps less able to shift production to unregulated areas. The regulatory landscape was quite different in 2004 than in 1977 and 1990, such that falling into nonattainment for ozone in 2004 may have had different implications for regulated plants. Finally, labor markets in manufacturing looked quite different. Labor intensity had declined and job creation and destruction rates were far lower.

Despite these caveats, the finding of an increase in employment suggests that, at least in the short-run, concerns over regulation-induced job loss may be overstated. This adds important evidence to our understanding of how employment adjusts to increased environmental regulation and may be useful for those wishing to understand the impact of future changes to the ozone nonattainment standards.

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 must be offset from an existing source within the same county.[6]

Of the six criterion pollutants, the standards for ozone have been the most difficult to comply with. While the number of counties in nonattainment for most criterion pollutants has steadily declined since 1997, the ozone standards have been difficult for many 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. As a result of the 1997 standard over 350 counties were designated as nonattainment for ozone in 2004, 200 of which were in attainment for the previous ozone standard.

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 considerably more complicated and political. In practice,

[6] As discussed in detail in (Ferris *et al.* 2013), exact regulations vary based on the specific classification 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.

some 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. Many previous studies have assumed that selection into nonattainment is exogenous to polluting activity of a particular plant or even of a particular county. However, selection into nonattainment in recent years has been based on a number of additional factors.

First, counties whose ozone levels are above the attainment standard may appeal their status to the EPA. The EPA may grant "bump down" appeals to counties that are already experiencing sharp declines in emissions. 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 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. In this case, the counties were designated as attainment precisely because industrial activity in the region was declining.

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. 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 selection into nonattainment was whether a county's polluting activity was expected to contribute to the ozone levels in the Metro Atlanta area. For each county 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, only counties with high levels of emissions and 'dirty" production were designated as nonattainment, then the appropriate counterfactual should also have high levels of emissions and 'dirty" production. Constructing an appropriate counterfactual is discussed in the following section.

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 the 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 employment trend, pre-treatment employment level, 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 in the same industry that are located in the same Census Division.[7]

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. 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.

4 Data

The data used in this version of the paper comes 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.[8] Previous papers on the NAAQS have used either county level data (List *et al.* 2003; Kahn & Mansur 2013) 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 county level labor markets were impacted. 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 2000 and 2009. The outcome variable of interest is the employment level 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).

[7]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.

[8]`http://www.epa.gov/airquality/greenbook/data_download.html`

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.[9] The baseline analysis of this paper is performed on county-industry observations with a reasonable pre-treatment employment size. That is, observations with fewer than 100 (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. Thankfully, 85% of all manufacturing employment in NO_x emitting industries is located in county-industries with over 50 employees so 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).[10] 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.

Table 1 lists summary statistics for dirty county-industries that switched into nonattainment status for ozone in 2004 and county-industries which were not subject to nonattainment status between 2000 and 2009.[11] Not surprisingly, county-industries that switch into nonattainment are larger, have higher levels of NO_x emissions and are more likely to be located in an MSA. Importantly, employment is trending differently in switching counties than it is in

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

[10]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).

[11]Note that, unlike the data used in the analysis, these statistics contain all county-industries regardless of size.

attainment counties.

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 map in Figure 3 shows the counties that entered nonattainment in 2004. 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).

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 employment 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 employment 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.

Before moving to the results it is also important to note that the estimates are obtained for a specific set of observations. The results used data from county-industries with over 100 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 or 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 100 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 above, these observations look quite different based on key observables. Table 2 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 100 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 5 and 6. Figure 5 displays the kernel density of employment for universe of switchers and non-switchers and Figure 6 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 2. The model matches well on the remaining variables. Similar pre-treatment employment trends is 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 Table 3. The outcome variable for the baseline specification is the percentage change in employment between 2003 and 2009. Each column of the table corresponds to a different set of matching variables. Column 1 matches only on pre-treatment employment trend (Employment 2003 - Employment 2000) 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 emissions from the observation. Column 4 is identical to Column 3 but matches on NO_x-Employment ratio rather than NO_x emission levels. A treated observation is always matched to an observation in the same industry and same Census Division as mapped in Figure 4.

Specification A is the baseline specification and reports the nearest neighbor matching results where matches are made on the three nearest neighbors. The estimates range from 0.071 to 0.091 implying a 7.1-9.1% increase in employment for dirty industries once their county enters nonattainment. This number is of course relative to the constructed counterfactual which has determined based on the observations' pre-treatment similarities to the treatment group.

To further explore the baseline results, Figure 7 takes the control observations that were matched to the treated observations and plots out the percent employment change that occurred for both groups over the entire sample. By design, the two groups track each other closely until policy hits, at which point employment in the constructed control group falls faster than the treatment group. While designation occurs in 2004, employment in the nonattainment counties does not increase relative to the attainment counties until 2006. This is consistent with evidence from Sheriff *et al.* (2015) who demonstrate that emitters are not impacted by nonattainment status until two years after designation.

For reasons discussed in the data section, the baseline specification includes all observations with greater than 100 employees in the year 2000, our first year of data. Specification B is identical to A but includes all observations with greater than 50 workers in 2000. Note that the estimates shrink here and border on statistical significance. Specification C is identical to A but matches on the nearest ten rather than three neighbors. When choosing the number of matches, there is a trade-off between degrees of freedom and quality of the match. Increasing the number of matches increases the number of counterfactual observations as well as the degrees of freedom. However, doing so means that the new matches will be of worse quality and are less likely to be a reasonable counterfactual. Results from specification C suggest a 6.0-7.3% increase in employment and, like Specification A are statistically significant.

Table 4 runs the same set of regressions as Table 3 but, in addition to matching on the variables at the base of the table, also matches on the county's *post* employment change for the manufacturing sector as a whole. If counties entering nonattainment had differing overall employment trends in manufacturing then perhaps this could be creeping into the estimates if not directly accounted for in the matching process. Matching on overall manufacturing trends *after* the regulation hits helps alleviate these concerns. Results are similar across the board.

Table 5 runs the same set of specifications but extends the post period to look at employment changes out to the year 2013. The outcome variable is now the percentage change in employment between 2013 and 2003. Results are largely the same and suggest that counties in attainment did not catch back up after the initial five year period. Figure **??** confirms this story.

5.3 Falsification Tests

As a falsification test, I run Specification A again. But this time, the data consists of the six manufacturing with the lowest NO_x intensity rather than the six industries with the highest NO_x intensity. The idea here is to perform and indirect test of unconfoundedness. A remaining concern is that there are some unobserved characteristics that are driving employment trends in treatment counties to be different than employment trends in the constructed counterfactual. The results in Table 4 directly match on overall manufacturing changes in a county, but a falsification test specifically for clean industries bolsters the argument that the baseline result is capturing the treatment effect rather than the effect of an unobserved variable on manufacturing employment.

To do this, Table 6 runs the same specification found in Panel A of Table 3. The results show that employment in clean industries in nonattainment counties experience a small but statistically insignificant increase compared to the counterfactual. This allays concerns that all industries in regulated counties experienced a bump in employment after 2004 relative to unregulated counties. Even though the estimates are statistically indistinguishable from zero, given that clean industries in attainment counties experience a small increase of between 1 and 3%, it may make sense to adjust the baseline estimate of the employment impact on "dirty" industries downward by this amount. Therefore, this paper argues that based on the results of the empirical analysis, employment conservatively increased between 4 and 6%.

Figure 9 replicates Figure 7 using clean rather than dirty industries. Here, employment treatment and counterfactual groups are nearly identical both before *and* after the implementation of the nonattainment standards. This again lends credence to the baseline results of the paper.

6 Discussion

This paper is the first to take an in-depth look at the employment impacts of the recent tightening of the NAAQS ozone standards. This is an enormously controversial regulation that has been claimed to be a significant job-killer. This research finds that, at least in the short-run, entering into nonattainment status for ozone does not appear to have resulted in employment declines. Rather, once an appropriate counterfactual has been created employment actually increased (or shrank less) in nonattainment counties relative to attainment counties. This could be explained by NO_x intensive plants hiring workers to comply with these regulations. Falsification tests which run identical specifications on the six cleanest industries find a slightly positive impact that is statistically indistinguishable from zero. In an attempt to be particularly conservative about the interpretation of the main findings, it may make sense to slightly adjust the main findings downward by 2-3% based on the slightly positive findings of the falsification test. Even after this adjustment has been made there is still a positive employment impact of between 4 and 6%.

7 Conclusion

These findings stand in contrast to research that has studied the impact of previous expansions of the ozone nonattainment standards. Future research should attempt to better understand the structural differences in the manufacturing sector that caused industries in counties entering nonattainment to experience an increase in employment. The decline in business dynamism, changes in productivity and productivity dispersion and changes to the production process itself may all play a role in explaining the absence of a negative employment finding. Better understanding the heterogeneity of plants' and workers' response to environmental regulation will provide a more complete understanding of labor markets' response to regulation.

References

Abadie, Alberto, & Imbens, Guido W. 2006. Large sample properties of matching estimators for average treatment effects. *Econometrica*, **74**(1), 235–267.

Banzhaf, Spencer H, & Walsh, Randall P. 2008. Do people vote with their feet? An empirical test of Tiebout's mechanism. *The American Economic Review*, **98**(3), 843–863.

Becker, Randy, & Henderson, Vernon. 2000. Effects of Air Quality Regulations on Polluting Industries. *Journal of Political Economy*, **108**(2), 379–421.

Becker, Randy A. 2005. Air pollution abatement costs under the Clean Air Act: evidence from the {PACE} survey. *Journal of Environmental Economics and Management*, **50**(1), 144 – 169.

Berman, Eli, & Bui, Linda T. M. 2001. Environmental regulation and labor demand: evidence from the South Coast Air Basin. *Journal of Public Economics*, **79**(2), 265–295.

Curtis, E. Mark. 2014 (December). *Who Loses Under Power Plant Cap-and-Trade Programs?* Working Paper 20808. National Bureau of Economic Research.

Deschenes, Olivier. 2010 (June). *Climate Policy and Labor Markets*. NBER Working Papers 16111. National Bureau of Economic Research.

Dube, Arindrajit, Lester, T William, & Reich, Michael. 2010. Minimum wage effects across state borders: Estimates using contiguous counties. *The review of economics and statistics*, **92**(4), 945–964.

EPA. 2008. *Atlanta, Georgia: Area Designations for the Ozone National Ambient Air Quality Standards*.

Ferris, Ann, Shadbegian, Ron, & Sheriff, Glenn. 2013. *The 1990 Clean Air Act Ozone Rules and Labor Demand for Electricity Generation: Do Stringency, Implementation, and Timing Matter?*

Fowlie, Meredith, Holland, Stephen P, & Mansur, Erin T. 2012. What do emissions markets deliver and to whom? Evidence from Southern California's NOx trading program. *The American Economic Review*, **102**(2), 965–993.

Gray, Wayne B., Shadbegian, Ronald J., Wang, Chunbei, & Meral, Merve. 2014. Do EPA regulations affect labor demand? Evidence from the pulp and paper industry. *Journal of Environmental Economics and Management*, **68**(1), 188 – 202.

Greenstone, Michael. 2002. The Impacts of Environmental Regulations on Industrial Activity: Evidence from the 1970 and 1977 Clean Air Act Amendments and the Census of Manufactures. *Journal of Political Economy*, **110**(6), 1175–1219.

Greenstone, Michael, List, John A., & Syverson, Chad. 2012 (September). *The Effects of Environmental Regulation on the Competitiveness of U.S. Manufacturing*. Working Paper 18392. National Bureau of Economic Research.

Heckman, James J, Ichimura, Hidehiko, & Todd, Petra E. 1997. Matching as an econometric evaluation estimator: Evidence from evaluating a job training programme. *The review of economic studies*, **64**(4), 605–654.

Heckman, James J, Ichimura, Hidehiko, & Todd, Petra. 1998. Matching as an econometric evaluation estimator. *The Review of Economic Studies*, **65**(2), 261–294.

Isen, Adam, Rossin-Slater, Maya, & Walker, W. Reed. 2014 (January). *Every Breath You Take - Every Dollar You'll Make: The Long-Term Consequences of the Clean Air Act of 1970*. Working Paper 19858. National Bureau of Economic Research.

Kahn, Matthew E., & Mansur, Erin T. 2013. Do local energy prices and regulation affect the geographic concentration of employment? *Journal of Public Economics*, **101**(C), 105–114.

List, John A., Millimet, Daniel L., Fredriksson, Per G., & McHone, W. Warren. 2003. Effects of Environmental Regulations on Manufacturing Plant Births: Evidence from a Propensity Score Matching Estimator. *The Review of Economics and Statistics*, **85**(4), 944–952.

McCarthy, James E. 2015. *Ozone Air Quality Standards: EPA's 2015 Revision*. Tech. rept. Congressional Research Service.

Mian, Atif R., & Sufi, Amir. 2012 (February). *What Explains High Unemployment? The Aggregate Demand Channel*. Working Paper 17830. National Bureau of Economic Research.

Morgenstern, Richard D., Pizer, William A., & Shih, Jhih-Shyang. 2002. Jobs Versus the Environment: An Industry-Level Perspective. *Journal of Environmental Economics and Management*, **43**(3), 412 – 436.

Petrick, Sebastian, & Wagner, Ulrich J. 2014. The impact of carbon trading on industry: Evidence from German manufacturing firms. *Available at SSRN 2389800*.

Shapiro, Joseph S., & Walker, Reed. 2014. *Why is Pollution from U.S. Manufacturing Declining? The Roles of Trade, Regulation, Productivity and Preferences*. Yale Mimeograph.

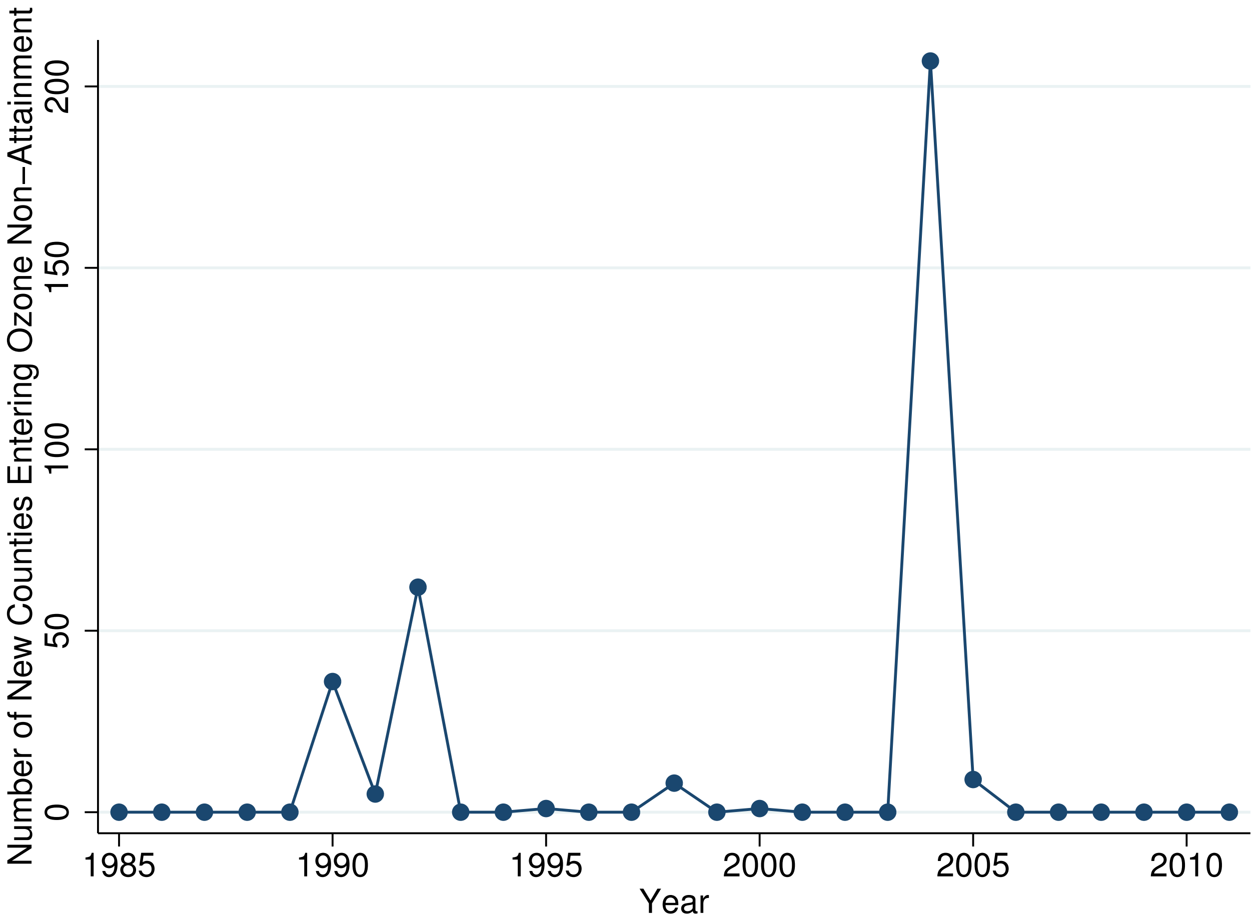
Sheriff, Glenn, Ferris, Ann E., & Shadbegian, Ron. 2015. *How Did Air Quality Standards Affect Emplyment and Generation at U.S. Power Plants? The Importance of Stringency, Geography and Timing*. NCEE Working Paper.

Walker, W. Reed. 2011. Environmental Regulation and Labor Reallocation: Evidence from the Clean Air Act. *American Economic Review: Papers and Proceedings*, **101**(3), 442–47.

Walker, W. Reed. 2013. The Transitional Costs of Sectoral Reallocation: Evidence From the Clean Air Act and the Workforce. *The Quarterly Journal of Economics*, **128**(4), 1787–1835.

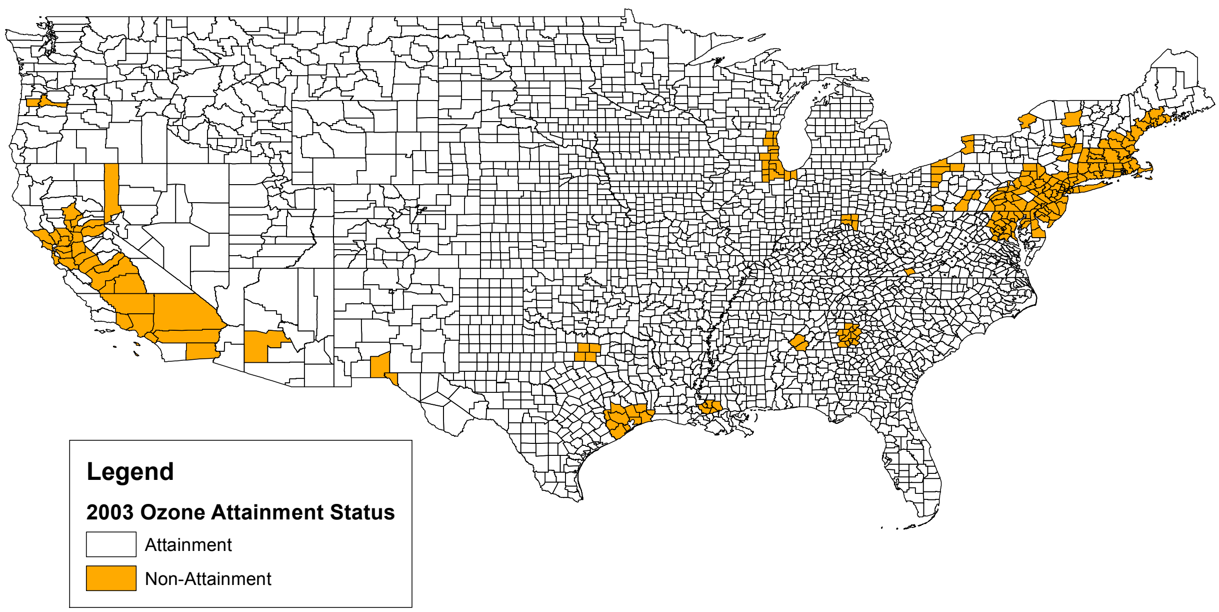
Figures and Tables

Figure 1: Newly Designated Ozone Nonattainment Counties by Year



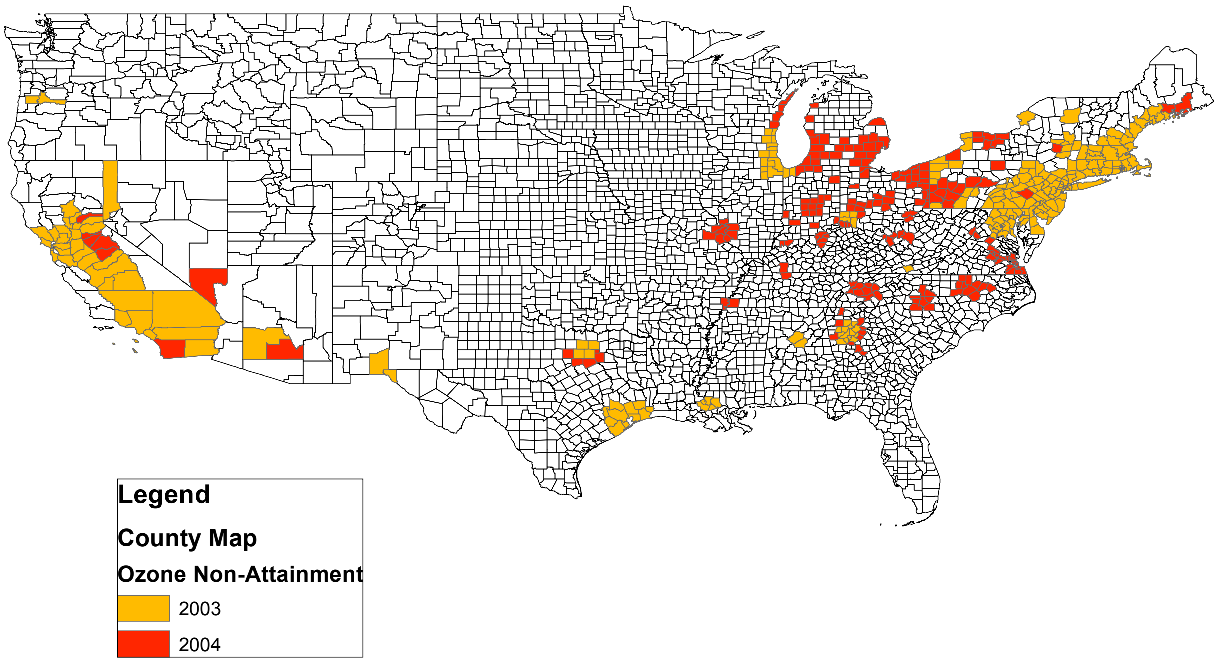
Note: The above figure shows the number of counties entering nonattainment for ozone in every year since 1985. Source: EPA's Greenbook

Figure 2: Ozone Nonattainment Counties



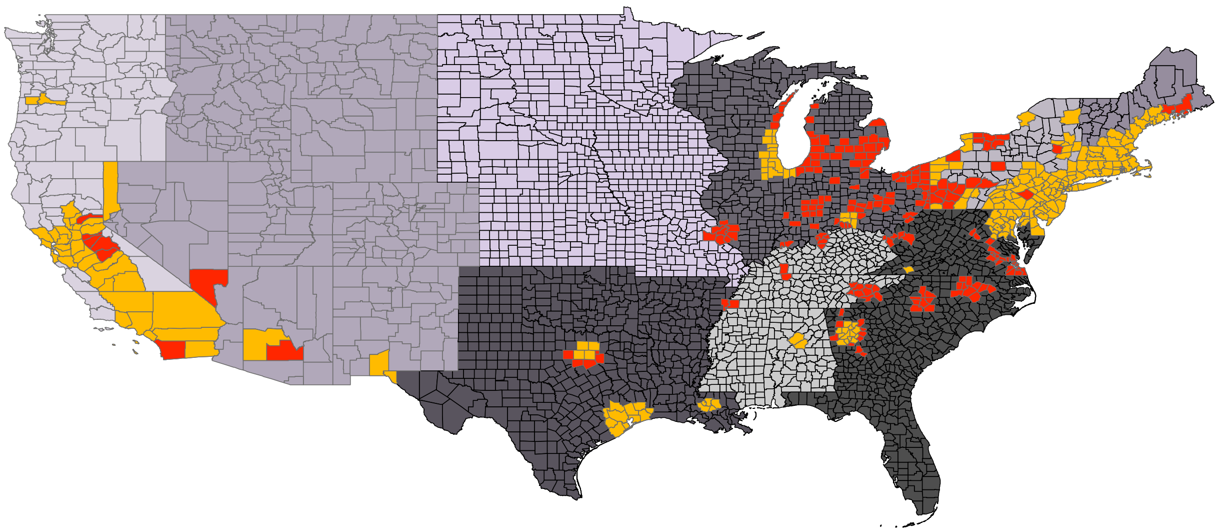
Note: The above figure shows the counties in nonattainment for ozone in 2003.

Figure 3: Newly Designated Ozone Nonattainment Counties



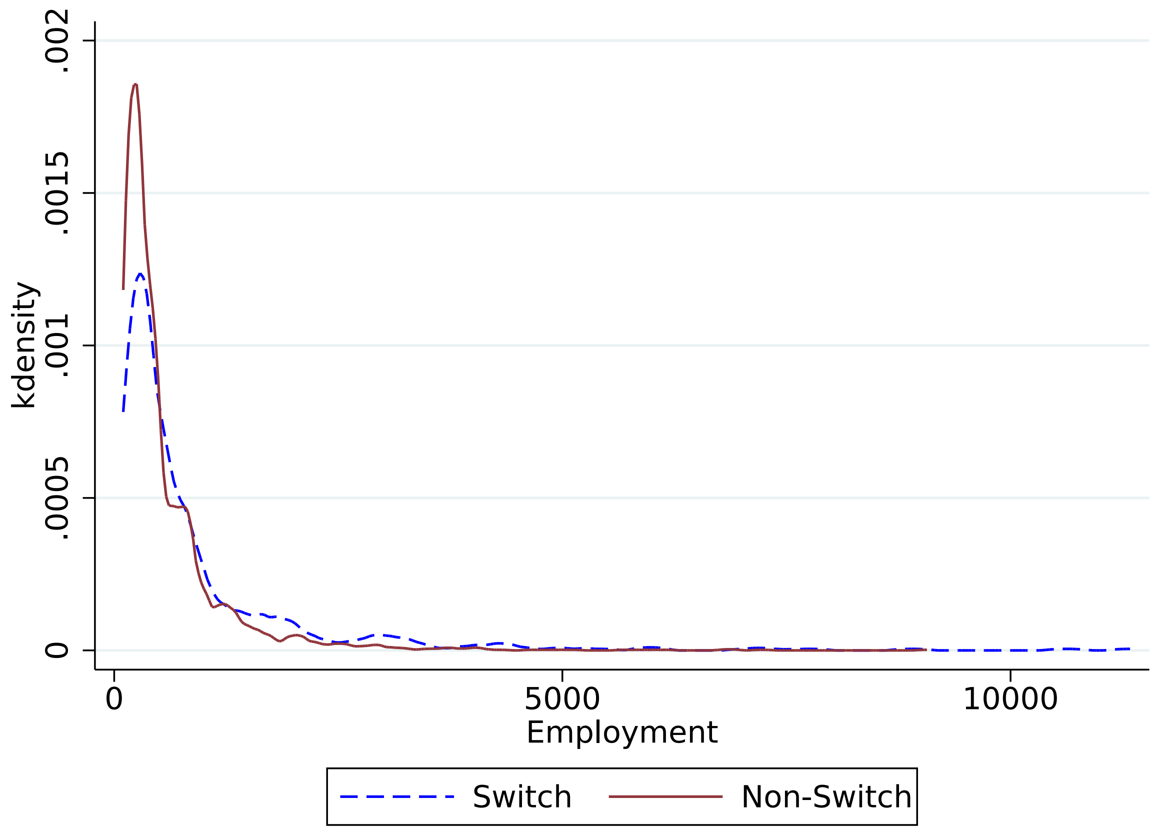
Note: The above figure shows the counties in nonattainment for ozone in 2003 and the counties newly designated as nonattainment in 2004.

Figure 4: Ozone Nonattainment Counties and Census Divisions



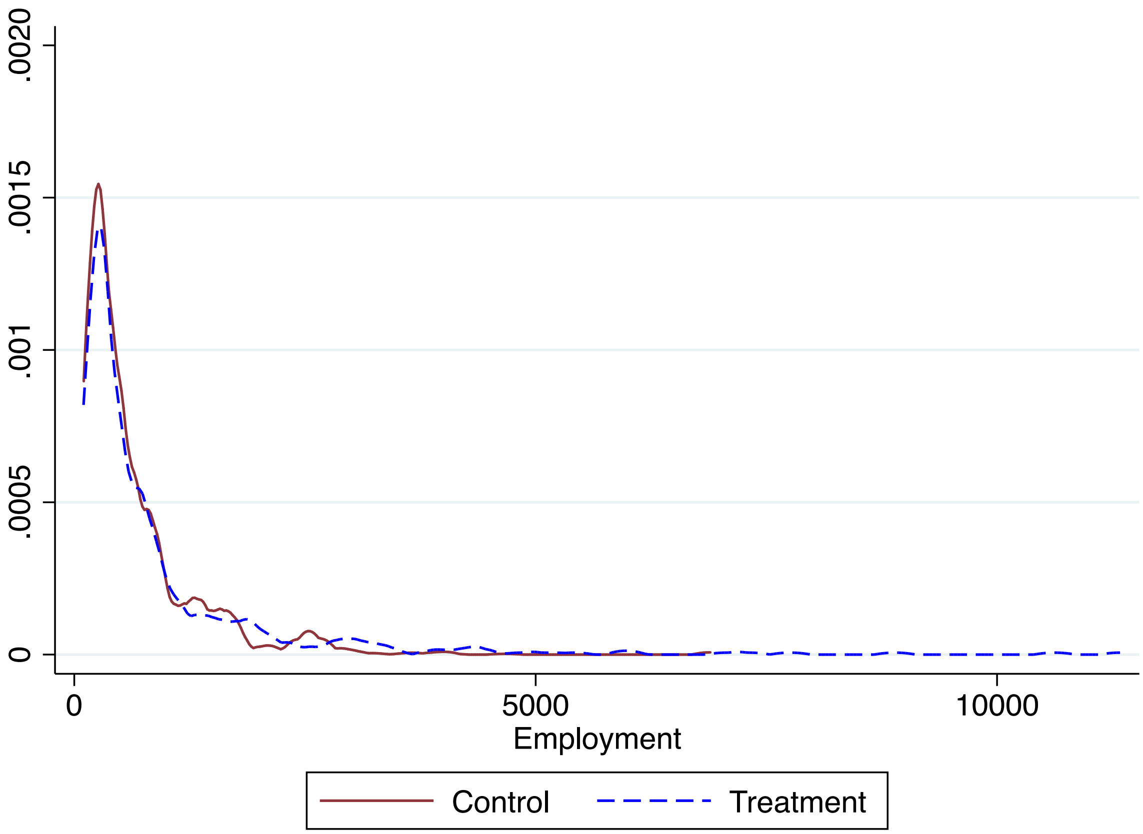
Note: The above figure shows the counties in nonattainment for ozone in 2003 and the counties newly designated as nonattainment in 2004. The shaded backgrounds are the nine Census Divisions. The counterfactual(s) for each treated observation is required to come from the same Census Division and the same NAICS 3-digit industry.

Figure 5: Employment Distribution: Switch vs. Non-Switch



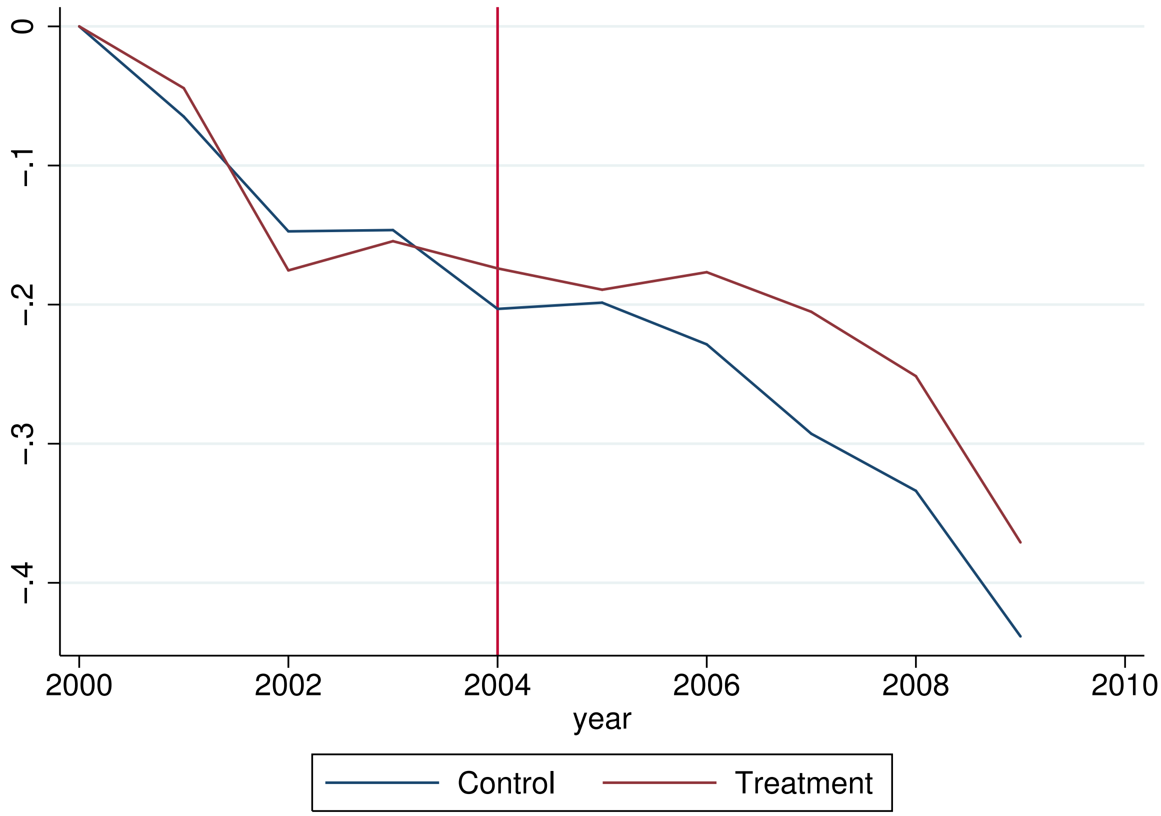
Note: The above figure shows displays the Employment level distribution for the Treatment and the Counterfactual groups.

Figure 6: Employment Distribution: Treatment vs. Counterfactual



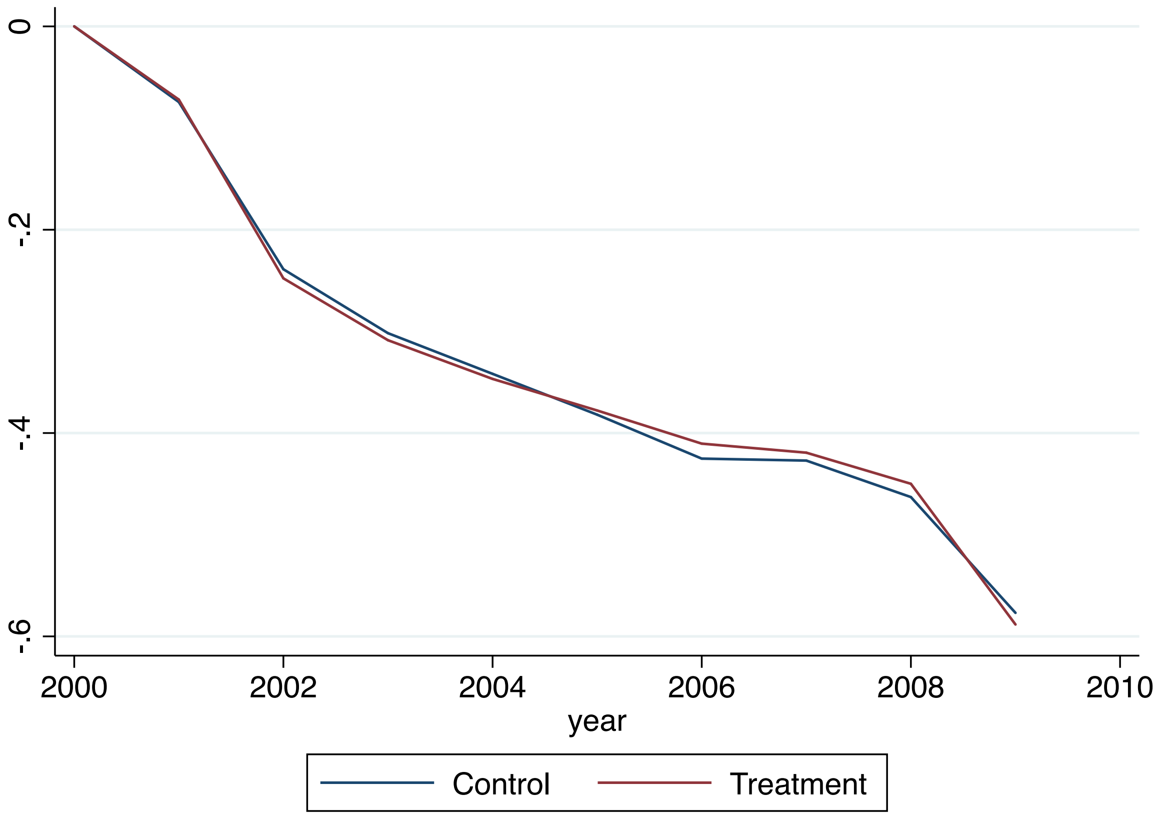
Note: The above figure shows displays the Employment level distribution for the Switchers and Non-Switchers with employment greater than 100.

Figure 7: Employment Distribution: Switch vs. Non-Switch in Treatment and Counterfactual



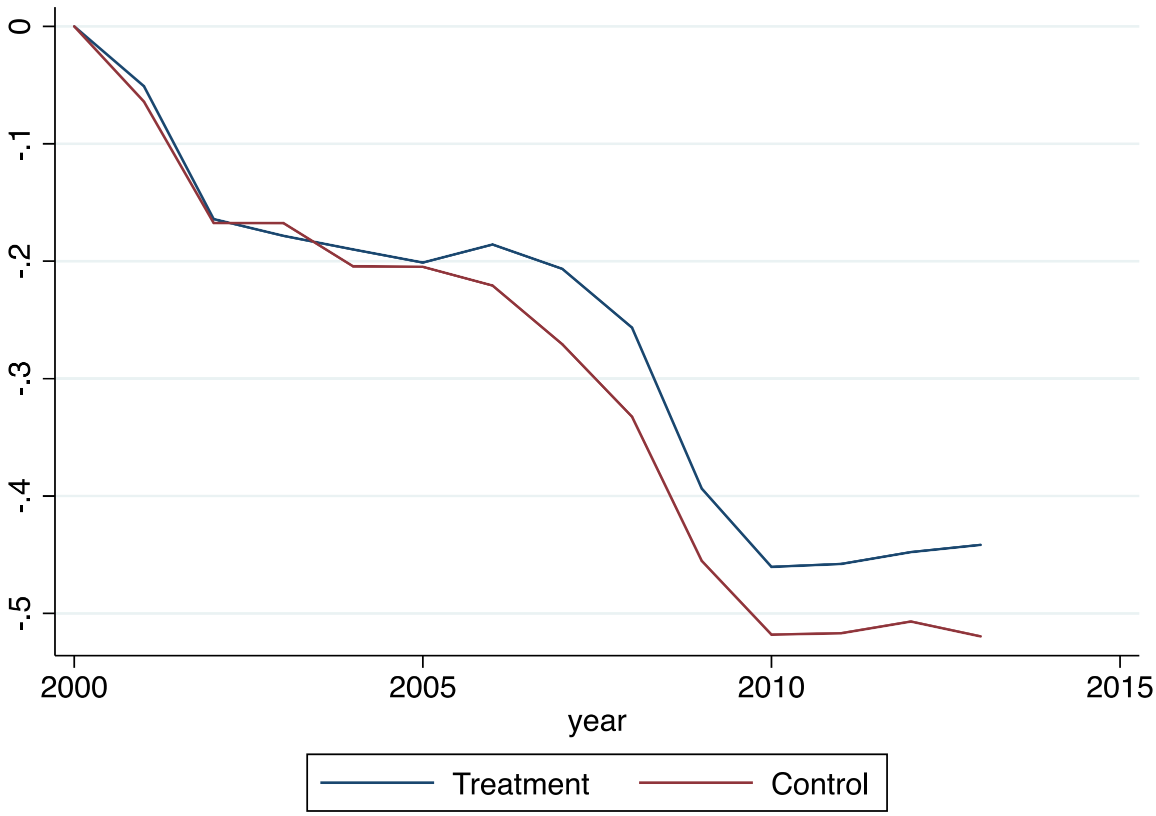
Note: The above figure shows displays the percent change in employment for both treated county-industries and the set of county-industries used as the control group for column 1 of specification A in Table 2. A vertical line is drawn at 2004, the year in which the counties entered nonattainment. By design, employment in the two groups track closely before the policy. After the policy employment increases in the nonattainment counties relative to the attainment counties.

Figure 8: Falsification Test: Treatment and Counterfactual



Note: The above figure shows displays the percent change in employment for both treated county-industries and the set of county-industries used as the control group for column 1 of specification A in Table 2. A vertical line is drawn at 2004, the year in which the counties entered nonattainment. By design, employment in the two groups track closely before the policy. After the policy, employment in clean industries in nonattainment counties continues along the same path as employment in attainment counties.

Figure 9: Employment Percent Change by Year in Treatment and Counterfactual



Note: The above figure shows displays the percent change in employment for both treated county-industries and the set of county-industries used as the control group for column 1 of specification A in Table 2. A vertical line is drawn at 2004, the year in which the counties entered nonattainment. By design, employment in the two groups track closely before the policy. After the policy, employment in clean industries in nonattainment counties continues along the same path as employment in attainment counties.

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

	(1)	(2)	(3)
	Attainment	Switchers	All
Employment 2000	216.9	547.7	253.5
	(459.9)	(1059.5)	(568.3)
NOx Emissions from Major Sources	78.4	118.1	82.8
	(435.8)	(585.7)	(454.9)
NOx-Emp Ratio	0.436	0.379	0.430
	(7.083)	(3.378)	(6.773)
MSA	0.274	0.869	0.340
	(0.446)	(0.337)	(0.474)
Percent Emp Change (2000-2003)	-0.306	-0.210	-0.295
	(0.835)	(0.665)	(0.818)
Percent Emp Change (2003-2009)	-0.198	-0.221	-0.200
	(0.894)	(0.716)	(0.876)
Observations	7202	896	8098

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 2009. The final column gives summary statistics for all county-industries.

Table 2: Test of Balance: Treatment vs. Counterfactual

	(1)	(2)	(3)
	Treatment	Control	Difference of Means
Emp 2000	892.265	662.994	-229.271
	(1273.022)	(677.506)	(952.784)
ln (Emp 2000)	6.265	6.145	-0.121
	(0.950)	(0.808)	(0.441)
Nox-Emp Ratio	0.215	0.184	-0.031
	(0.782)	(0.950)	(0.837)
MSA	0.9	0.885	-0.015
	(0.300)	(0.319)	(0.136)
% Emp Change 2000-2003	-0.172	-0.167	0.005
	(0.337)	(0.275)	(0.182)

Note: The above table provides summary statistics for all "Treatment" county-industries, the constructed counterfacual county-industries and the difference in means between the two groups. The difference in means is not statistically significant for any of the variables.

Table 3: Employment Results: Average Treatment Effect Using Nearest Neighbor Matching

	(1) % Emp Change	(2) % Emp Change	(3) % Emp Change	(4) % Emp Change
		Spec A Emp > 100 , m = 3		
Coeff	0.071*	0.079**	0.091**	0.077**
SE	(0.037)	(0.037)	(0.038)	(0.037)
Treated Obs	557	557	557	557
Controls	1889	1889	1889	1889
		Spec B Emp > 50 , m = 3		
Coeff	0.040	0.064*	0.046	0.061*
SE	(0.036)	(0.035)	(0.036)	(0.036)
Treated Obs	681	681	681	681
Controls	2288	2288	2288	2288
		Spec C Emp > 100 , m = 10		
Coeff	0.060*	0.071**	0.062*	0.073**
SE	(0.033)	(0.034)	(0.034)	(0.034)
Treated Obs	557	557	557	557
Controls	3974	3974	3974	3974
Matching Vars				
Pre Emp Trend	Yes	Yes	Yes	Yes
MSA	Yes	Yes	Yes	Yes
Emp 2000		Yes	Yes	Yes
NOx Emissions			Yes	
NOx-Emp Ratio				Yes

Note: Specification A reports the nearest neighbor matching results where matches are made on the three nearest neighbors. Only observations with greater than 100 workers in 2000. Specification B expands the dataset to include observations with greater than 50 workers in 2000. Specification C is identical to A but matches on the nearest 10 neighbors. ***, ** and * indicate signifcance at the 1,5 and 10 percent levels respectively.

Table 4: Robustness Check: Matching on County's 2003-2009 Total Manufacturing Employment Change

	(1) % Emp Change	(2) % Emp Change	(3) % Emp Change	(4) % Emp Change
		Spec A Emp > 100 , m = 3		
Coeff	0.088**	0.086**	0.075**	0.085**
SE	(0.037)	(0.036)	(0.038)	(0.037)
Treated Obs	557	557	557	557
Controls	1889	1889	1889	1889
		Spec B Emp > 50 , m = 3		
Coeff	0.056*	0.064*	0.051	0.066*
SE	(0.037)	(0.036)	(0.036)	(0.036)
Treated Obs	681	681	681	681
Controls	2288	2288	2288	2288
		Spec C Emp > 100 , m = 10		
Coeff	0.068**	0.074**	0.073**	0.076**
SE	(0.033)	(0.034)	(0.034)	(0.034)
Treated Obs	557	557	557	557
Controls	3974	3974	3974	3974
Matching Vars				
All Cnty Post Trend	Yes	Yes	Yes	Yes
Pre Emp Trend	Yes	Yes	Yes	Yes
MSA	Yes	Yes	Yes	Yes
Emp 2000		Yes	Yes	Yes
NOx Emissions			Yes	
NOx-Emp Ratio				Yes

Note: Specification A reports the nearest neighbor matching results where matches are made on the three nearest neighbors. Only observations with greater than 100 workers in 2000. Specification B expands the dataset to include observations with greater than 50 workers in 2000. Specification C is identical to A but matches on the nearest 10 neighbors. ***, ** and * indicate signifcance at the 1,5 and 10 percent levels respectively.

Table 5: Employment Results: 2003-2013

	(1) % Emp Change	(2) % Emp Change	(3) % Emp Change	(4) % Emp Change
		Spec A Emp > 100 , m = 3		
Coeff	0.072*	0.087**	0.086**	0.078*
SE	(0.041)	(0.041)	(0.042)	(0.042)
Treated Obs	557	557	557	557
Controls	1889	1889	1889	1889
		Spec B Emp > 50 , m = 3		
Coeff	0.032	0.053	0.040	0.046
SE	(0.040)	(0.040)	(0.041)	(0.042)
Treated Obs	681	681	681	681
Controls	2288	2288	2288	2288
		Spec C Emp > 100 , m = 10		
Coeff	0.048	0.045	0.040	0.040
SE	(0.038)	(0.038)	(0.038)	(0.038)
Treated Obs	557	557	557	557
Controls	3974	3974	3974	3974
Matching Vars				
Pre Emp Trend	Yes	Yes	Yes	Yes
MSA	Yes	Yes	Yes	Yes
Emp 2000		Yes	Yes	Yes
NOx Emissions			Yes	
NOx-Emp Ratio				Yes

Note: Specification A reports the nearest neighbor matching results where matches are made on the three nearest neighbors. Only observations with greater than 100 workers in 2000. Specification B expands the dataset to include observations with greater than 50 workers in 2000. Specification C is identical to A but matches on the nearest 10 neighbors. ***, ** and * indicate signifcance at the 1,5 and 10 percent levels respectively.

Table 6: Employment Results: Falsification Test

	(1) % Emp Change	(2) % Emp Change	(3) % Emp Change	(4) % Emp Change
Coeff	0.039	0.007	0.021	0.013
SE	(0.049)	(0.050)	(0.050)	(0.050)
Treated Obs	512	512	512	512
Controls	1747	1747	1747	1747
Matching Vars				
Pre Emp Trend	Yes	Yes	Yes	Yes
MSA	Yes	Yes	Yes	Yes
Emp 2000		Yes	Yes	Yes
NOx Emissions			Yes	
NOx-Emp Ratio				Yes

Note: This table runs specification A for the six industries with the least amount of NOx emissions. See Table 2 for additional notes.