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ABSTRACT

This paper tests how a major cap-and-trade program, known as the NOx Budget Trading Program (NBP), impacted labor markets in the regions where it was implemented. The cap-and-trade program dramatically decreased levels of NOx emissions and added substantial costs to energy producers. Using a triple-differences approach that takes advantage of the geographic and time variation of the program as well as variation in industry energy-intensity levels, I examine how employment dynamics changed in manufacturing industries whose production process requires high levels of energy. After accounting for a variety of flexible state, county and industry trends, I find that employment in the manufacturing sector dropped by 1.3% as a result of the NBP. Young workers experienced the largest employment declines and earnings of newly hired workers fell after the regulation began. Employment declines are shown to have occurred primarily through decreased hiring rates rather than increased separation rates, thus mitigating the impact on incumbent workers.

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1 Introduction

The Environmental Protection Agency's regulation of the energy sector is a highly contentious topic in the public sphere. Proponents of the federal regulations emphasize the health benefits that accrue to society when fewer pollutants are emitted, while critics claim that the current regulations harm the economy by imposing significant costs on industry and workers. Job loss in particular is cited as a primary means by which regulation inflicts damage on the economy and has gained special attention in recent years as the EPA has proposed new energy sector regulations at a time when labor markets are still recovering from the Great Recession and middle class jobs are disappearing.[1]

The federal law that lies at the heart of this controversy is the Clean Air Act Amendments (CAAA), which vests authority in the EPA to regulate the emissions of polluting industries. Broadly speaking, there are two types of regulations that the CAAA imposes on pollution-emitting establishments. The first, known as the National Ambient Air Quality Standards (NAAQS), began in the 1970's as a result of the 1970 CAAA and required polluting plants in counties with poor air quality to adopt "lowest available emission rate" technology. The labor market impact of the NAAQS and its subsequent expansions has been studied extensively by economists over the past decade (Greenstone 2002; Kahn & Mansur 2013; Walker 2011; 2013). These studies examine changes in manufacturing employment in counties that fail to meet NAAQS attainment standards and are thus subject to tighter regulations.

Since the passage of the 1990 CAAA, however, a second and wider reaching policy has taken form with the intent of regulating interstate air pollution. As counties began to realize that their own air quality was affected not only by local polluters but also by polluters located in upwind states, there was a push for the regulation of all establishments whose air pollutants crossed state boundaries. In 1990, the Acid Rain Program established a national cap-and-trade program for Sulfur Dioxide (SO_2) and in 2003 and 2004 the Nitrogen Oxide (NO_x) Budget Trading Program (NBP) was established in nineteen states east of the Mississippi. Despite the size and far-reaching impact of these two cap-and-trade programs, there has been no empirical research that has sought to evaluate their labor market implications. Understanding their impacts is particularly important in light of the recently proposed Clean Power Program, whose CO2 emissions reductions may take place through regional trading programs similar to the NBP.

[1]See *Wall Street Journal* July 26, 2011 op-ed "The Latest Job Killer from the EPA", "Getting Ready for a Wave of Coal-Plant Shutdown" *Washington Post* "Wonkblog" Aug 19, 2011, "Obama Outlines Ambitious Plan to Cut Greenhouse Gases" *New York Times* June 25, 2013 and Autor & Dorn (2013).

From an efficiency standpoint, cap-and-trade programs are preferable to the command and control style regulations that began in the 1970's. However, the market-based solution they provide may still have potentially severe redistributional implications as firms which previously paid nothing to pollute now face a new input cost. In response, firms are likely to see an increase in their marginal costs and will re-optimize their input bundles, including labor, so as to minimize total costs.

While the Acid Rain Program was the first such large scale cap-and-trade program in the United States, given that it applied to all large power plants in the country, empirically estimating its impact on labor markets has been difficult due to the lack of a valid counterfactual.[2] However, for a variety of reasons, the attributes of the recently implemented NBP make for a policy whose impacts are both important and possible to identify. First, the NBP had a major impact on energy production. The regulation of nearly 900 gas, oil and coal-fired electric generating units plus 270 large manufacturing plants forced NO_x emitting firms to make difficult and costly decisions on how to comply with the cap-and-trade scheme. Overall, complying with the NBP was expected to add $2.15 billion dollars of annual costs to electric utilities (Palmer *et al.* 2001) and it represented a sizable implicit pollution tax on regulated manufacturing industries (Shapiro & Walker 2014). Second, this policy was implemented in nineteen eastern states over a period of two years. Many states were not exposed to the NBP and, under certain assumptions discussed below, may be considered a valid counterfactual after controlling for preexisting differences. Finally, industries in the NBP region that require high levels of energy in their production process would be expected to be most affected by the regulation. These sources of geographic, time and industry heterogeneity form the basis for the identification strategy used to determine the impact of the NBP on labor markets. To the best of my knowledge, this is the first such credible study of the labor market effects of any major EPA cap-and-trade program.

One reason why empirical methods may prove particularly useful is that economic theory gives no clear intuition regarding the effect of energy sector regulation on manufacturing employment outcomes (Berman & Bui 2001). Environmental regulation that causes an exogenous shock in the price of energy is likely to lead to two competing employment effects on an establishment's intensive margin. First, given that capital and energy are complements and capital and labor are substitutes, a positive shock in the price of energy may lead plant managers to employ more labor and less capital. How-

[2]For this reason, researchers have generally used structural models to estimate the costs and benefits of the Acid Rain Program (see Burtraw *et al.* (1998)).

ever, an increase in the price of energy production will also increase marginal costs and decrease the demand for labor due to a decline in production. Manufacturing plants that require onsite energy production and are subject to regulation will take the costs of regulations into account as they decide whether to expand or contract. Furthermore, firm owners may adjust the extensive margin as they take production costs into account when determining plant location. Plants in areas with increased energy prices are more likely to be shut down and newly constructed plants are more likely to be built in regions that did not experience a positive shock to energy prices and are not subject to the direct regulation of the program.[3]

While qualitative predictions from theory may be somewhat ambiguous, recent empirical research studying the impact of environmental regulation on employment has shown either no change (Berman & Bui 2001; Martin *et al.* 2014) or a decrease in the employment levels of regions where regulation has been implemented (Greenstone 2002; Kahn & Mansur 2013; Walker 2011). Furthermore, the broader literature on the impact of environmental regulation has consistently found a negative impact of regulation on other manufacturing outcomes such as productivity, new plant openings and investment (Becker & Henderson 2000; Dean *et al.* 2000; List *et al.* 2003; Henderson 1996; Hanna 2010; Greenstone *et al.* 2012).[4]

This paper takes advantage of the heterogeneity in industry energy intensity levels to perform a triple differences (DDD) analysis that estimates the NBP's impact on manufacturing employment levels and other labor market outcomes. The econometric model borrows features from both Deschenes *et al.* (2012) which looks at the impact of the NBP on pollution and health outcomes and Kahn & Mansur (2013) which examines the geographic determinants of manufacturing employment. Using County Business Patterns (CBP) and Quarterly Workforce Indicators (QWI) data as well as NBER's Productivity Database, I am able to account for important state, county, industry and year controls as

[3]Carlton (1983) and Kahn & Mansur (2013) find strong empirical evidence that electricity prices are a major determinant of manufacturing establishment location and employment decisions. Of the 270 regulated manufacturing plants 93% are in the top tercile of energy intensive industries (Author's calculation based on records from EPA's website). The aggregate nature of the employment data used in the paper prevent estimating a separate effect for the manufacturing plants whose energy production was directly regulated. The need for a separate estimate is mitigated as energy production is regulated whether or not it occurs within the boundary of the firm. Energy-intensive plants that choose to purchase electricity, rather than produce energy on-site, will also be impacted as the cost of production of a major input increases.

[4]Of course there is also a large literature on the benefits of regulation and air quality improvements. See Chay & Greenstone (2003); Jerrett *et al.* (2009); Deschenes *et al.* (2012); Zivin & Neidell (2012) for a few representative examples.

well as state, county and industry trends. These controls will prove important, as industries tend toward regional agglomeration and as the manufacturing sector as a whole has experienced general geographic shifts in recent years. Performing the DDD analysis with a broad set of controls, I first find that employment in industries with an additional percentage point of energy intensity decreased 1.23% in the region that was impacted by the NBP. This represents a loss of roughly 1.3% of manufacturing employment or 110,000 jobs in the NBP region during this period. To place this figure in context, this is equivalent to about 3.1% of the total job loss in the manufacturing sector in the NBP region between 1998 and 2009.

This employment impact is an important outcome, particularly to policy makers looking to protect jobs in particular industries. However, the employment effect will not capture the full impact of regulation on labor markets. To provide a richer understanding of the impact of regulation on labor markets this paper explores worker flow and earnings variables found in the QWI, a data source which includes additional labor market outcomes and further disaggregates the data by worker age. Analysis of QWI variables finds that firms reduced their employment levels primarily through a reduction in hiring rather than an increase in separations. Young workers, who have high turnover rates, experienced the largest employment declines. Furthermore, wage offers, as measured by new hire earnings, declined by as much as 4.2% for industries in the top quartile of energy intensity as compared to the bottom quartile. The ability to observe new hire earnings is important as firms may be unable to immediately adjust earnings of incumbent workers when they are hit by a regulatory shock. The results suggest that unions and firing costs may play a key role in how firms react to environmental regulation.

This paper adds to the literature in two important ways. It is the first paper to empirically estimate the impact of any major EPA cap-and-trade program on labor markets. Given the size of the cap-and-trade programs, as well as the current policy debate over additional energy sector regulations, a better understanding of their impact is greatly needed. Second, it provides evidence of which workers were affected; uses worker flows to examine how the employment adjustments occurred; and examines the impact on worker earnings by focusing on the earnings of new hires, the margin on which earnings changes will most quickly adjust. Finally, while recent work by Walker (2013) has found long term earnings losses for existing workers in newly regulated plants, this paper suggests that the method of employment adjustment may have dampened the impact on incumbent workers at the expense of workers hired after the regulation.

The remainder of the paper is organized as follows. Section 2 presents a brief history of the Clean Air Act Amendments and how the NBP came to be implemented. Section 3 details important aspects of the data used in the analysis. Section 4 provides the econometric model, results and specification checks. Section 5 discusses the results and Section 6 concludes.

2 Background

Originally passed in 1963, the Clean Air Act (CAA) is the main federal law that seeks to control air pollution throughout the United States. The CAA has been amended multiple times including 1966, 1970, 1977 and 1990. Perhaps the most researched of the regulations brought upon by CAA and its amendments is the NAAQS. The NAAQS were established following the 1970 CAAAs and required polluting establishments located in counties that failed to achieve certain air quality levels to meet stricter emissions standards than establishments located in counties whose air quality was deemed acceptable.

The 1977 amendments, in addition to strengthening the NAAQS, included Section 126, a provision that allowed the EPA to regulate interstate air pollution and limit the environmental harm that downwind states could impose on upwind states. The EPA did not immediately enforce Section 126, however, choosing instead to focus regulation efforts on establishments whose pollutants impacted the air quality of their local community rather than those impacting regions outside their immediate geographic region. In fact, between 1977 and 1998, the EPA never granted a petition filed under the interstate air pollution clause found in Section 126 of the CAA.

The passage of the 1990 Clean Air Act Amendments strengthened the language of Section 126 and established the first cap-and-trade programs. It was passed in response to the continued failure of many northeastern regions to meet air quality requirements despite having already restricted emissions in their local region. Title IV of the 1990 CAAA established a cap-and-trade program for SO_2. This would become known as the Acid Rain Program and in 1995 the EPA began Phase I for the dirtiest 110 power plants.

In 1998 the EPA granted its first petition under Section 126, paving the way for a cap-and-trade program for NO_x, an important precursor of ground-level ozone. The granting of this petition came as the result of two factors. First, the 1990 amendments had strengthened the interstate pollution protection law, calling for "reasonably available control technology" throughout an ozone transport region. Second, numerous lawsuits filed against the EPA by northeastern states requested that the EPA regulate NO_x emissions from states whose emissions directly contributed to their own levels of smog and ozone. These upwind states argued that NO_x pollution from downwind sources not only had negative health impacts on their citizens but also prevented them from meeting the NAAQS ozone nonattainment standards. In these lawsuits, a large body of scientific evidence was presented showing that NO_x gases can in fact be transported significant distances by wind currents and that NO_x emissions should therefore be subject to Section 126 of the CAAA. By granting the petition of the northeastern states, the EPA agreed to regulate and reduce the amount of NO_x emitted by electric generating units (EGU's) and large industrial plants in southern and central states.[5]

The NBP cap-and-trade program formally began for eight states and the District of Columbia in 2003 (see Figure 1). States and utilities in the Midwest and Southeast continued to fight legal battles against the EPA with varying outcomes, but in 2004 eleven additional states began compliance with the NBP, for a total of nineteen states.[6] The program would regulate nearly 900 EGU's and 270 large industrial units that produced energy and heat for their own consumption.

As can be seen in Figure 2, EGU's dramatically decreased their output of NO_x on May 31, 2004, the first day in which all nineteen states began participating. Regulated establishments could choose to reduce their NO_x emissions in a variety of ways. One option was fuel switching, whereby establishments would shift away from coal and towards alternative energy sources such as natural gas that release far less NO_x into the atmosphere. Despite the additional production costs brought on by the NBP, most coal remained a cheaper source of energy than the alternatives (Fowlie 2010). Because electricity production from coal fired plants remained inframarginal, utilities largely continued to burn coal and found alternative ways to comply with the NBP. These compliance costs could be fixed and upfront or they could be variable and spread out over time. Plants opting for the high upfront cost option installed selective catalytic reduction (SCR) technology. This technology cuts NO_x emissions by up to 90% but costs the average plant $40 million dollars (Linn 2008). On the other end of the spectrum, about 30% of NBP regulated establishments chose to make no capital adjustments and simply purchased permits for every unit of NO_x they emitted (EPA 2009).[7] Regardless of which

[5]The smaller and less restrictive program known as the Ozone Transport Commission NO_x Budget Program (not to be confused with the NBP) began for 11 northeastern states in 1999.

[6]Through negotiations and court battles, Missouri delayed compliance until 2007. Georgia, originally slated to also begin in 2007, was eventually ruled exempt from the program altogether. Additionally, deals were struck in Missouri, Alabama and Michigan which limited compliance to only certain counties.

[7]SCR is both the most expensive and most effective technology in reducing NO_x emissions (Fowlie 2010). There are a variety of less expensive and less effective technologies that utilizes chose to install. Selective non-catalytic technologies cost the average plant $10 million but only reduces NO_x by 35 per-

reduction technique they choose, the production costs of electric utilities will increase as a result of the NBP. Three estimates have been made that calculate the cost of the NBP to utilities. Palmer *et al.* (2001) estimated the program's total costs to utilities at about \$2.1 billion per year. Deschenes *et al.* (2012) use the market price of permits to estimate the cost at \$400-700 million per year and Linn (2010) examines utility stock prices to estimate a *total* (rather than annual) drop in expected utility profits of up to \$25 billion dollars. Palmer *et al.* (2001) argue that the costs of the NBP will be passed on to the consumer in the form of higher electricity prices. Indeed, the EPA (1999) estimated that electricity prices would rise by 1.6% as a result of the NBP and a later report by Platts Research & Consulting (2003) predicted a \$1-\$3/MWh increase in the price of wholesale electricity.[8]

Carlton (1983) and Kahn & Mansur (2013) document that electricity prices are a major determinant of where manufacturing firms choose to locate their workers. Given the NBP's substantial impact on electricity and energy production costs more generally, firms that require high levels of energy in their production process may be expected to re-optimize their input mixture in response to a change in the expected costs of a crucial input. Additionally, Shapiro & Walker (2014) find that the NBP's direct regulation of manufactures represented a sizable tax in and of itself. Using a DDD approach, I test whether firms with high energy requirements did in fact respond to the NBP by decreasing employment levels after the implementation of the NBP, relative to low-energy firms and relative to non-NBP control areas.

Since it began in 2003 and 2004, the NBP has changed names but the market for NO_x allowances continues to exist. In 2008 the NBP became part of the Clean Air Interstate Rule (CAIR) and in 2011 the EPA announced it would replace and expand the regulations of CAIR with the new Cross-State Air Pollution Rule (CSAPR).[9]

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cent. Additional pre-combustion and combustion technologies can decrease emissions between 15 and 50 percent depending on the specifications of the plant. Fowlie shows that regulated utilities are more likely to pursue capital intensive solutions than deregulated utilities.

[8]Using average wholesale prices in the Northeast ISO and the PJM in 2003 this is equal to an electricity price increase of between 2.47% and 7.41%. The Platts analysis was performed in light of an unexpected rise in the price of natural gas that took place just prior to the NBP. This rise made shifting to natural gas more expensive than had previously been expected.

[9]In 2005 the EPA announced that the Clean Air Interstate Rule (CAIR) would replace the NBP's regulation of NO_x emissions in 2009 and SO_2 in 2010. CAIR was intended to expand the number of covered states to twenty-five and further tighten emissions standards. Given that there was significant legal uncertainty surrounding CAIR when it was announced, it is unlikely that manufacturing industries would have immediately reacted. Because CAIR is a continuation of the NBP cap-and-trade program it is difficult and perhaps even unnecessary to disentangle the impact of the CAIR announcement from the implementation of NBP. In short, the interpretation of the evidence is influenced only slightly by CAIR, with all results still attributable to the overall cap-and-trade policy.

The regional cap-and-trade program continues to be greatly debated. On August 21, 2012 the D.C. Circuit Court of Appeals vacated CSAPR leaving the future of both the SO_2 and NO_x cap-and-trade programs in doubt, but on April 29, 2014 the Supreme Court reversed the ruling by the Court of Appeals. This decision clears the way for the EPA to move forward with the program's expansion. Furthermore, it has sparked renewed interest among supporters of GHG regulations, who see the NBP as a template of a successful environmental regulation that can be implemented under the existing Clean Air Act (Burtraw *et al.* 2014).

3 Data

3.1 County Business Patterns

The two employment data sets used to analyze the impact of the NBP on manufacturing employment are the Census Bureau's County Business Patterns (CBP) and the Quarterly Workforce Indicators (QWI). The CBP is a yearly data product released by the Census Bureau that provides sub-national economic data by industry. Data can be obtained at the national, state, county and metropolitan levels and include the total number of workers by industry in a geographic area. 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. Using CBP data from 1998-2009, I create panel data sets at both the state-industry and county-industry level.

In 1998 the Census Bureau switched its industry classifier variable to account for the changing face of the American economy. The Standard Industrial Classification (SIC) system was abandoned in favor of the newer North American Industry Classification System (NAICS). The change from SIC codes to NAICS codes in 1998 creates some difficulties in consistently estimating industry employment across time periods. Given that the NBP was implemented in 2003, it is logical to use 1998 as a starting year for the data and avoid any inconsistencies that may arise from merging previous years with different industry definitions. All data between 1998 and 2009 use NAICS codes which are consistent across time periods. 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 distinct 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 information of any single establishment. Therefore, if very few establishments are located in a particular county-industry or state-industry cell, then employment data will be suppressed for that observation. The primary results of this paper use state-industry data which has limited cell suppression for employment. In the state-industry data set 76% of state-industry cells are observed directly. These cells represents 93% of all manufacturing employment in the United States. For those cells that are suppressed, I perform an imputation method similar to that used by Kahn & Mansur (2013) and Mian & Sufi (2012), which takes advantage of the CBP's establishment-size cell count variables and imputes employment for the suppressed cells by multiplying the number of establishments in each establishment-size cell by the midpoint establishment size of that category.[10] The same imputation method is used for the county-industry level data.[11]

3.2 Quarterly Workforce Indicators

Like the CBP, the QWI is a publicly available data set that contains sub-national employment data by industry. The underlying microdata for the QWI is the Longitudinal Employer Household Dynamics (LEHD) program at the U.S. Census Bureau, which uses state unemployment insurance data as its primary input (see Abowd *et al.* (2006) for a complete description of the QWI and the LEHD). In recent years economists have begun to use this data to evaluate the labor market impacts of the housing crash, changes to minimum wage laws and workplace mandates (Abowd & Vilhuber 2012; Gittings & Schmutte 2012; Dube *et al.* 2011; Curtis *et al.* 2013). The QWI has both strengths and weaknesses compared to the CBP but there are two primary advantages that the QWI provides. First, it contains detailed cuts of the data by worker characteristic. That is, the QWI provides not only total employment within a state-industry, but also breaks down this employment by age group and gender. These data can then be used to understand the heterogeneity of the treatment effect along a number of dimensions that are not available in the CBP. The second advantage is that the QWI provides data not only on employment levels but also on worker flows (hires and separations). On the earnings

[10] All state and county-industry observations contain the number of establishments in narrowly defined employee size categories (1-4, 5-9, 10-19, ..., 5,000+). For the 5,000+ category employment is top coded at 6,000. See (Kahn & Mansur 2013) for a full explanation of the imputation method.

[11] As discussed later in the paper, regressions weight state-industry observations by their pre-NBP employment level. This is common in the literature and mitigates concerns about imputation related bias as imputed cells are smaller and thus given less weight in the regressions.

side, the QWI records monthly average earnings for new hired workers as well as for all workers in the state-industry. Earnings of newly hired workers is a particularly nice feature as firms responding to negative shocks may have more flexibility to adjust earnings of new hires than incumbent workers. A final benefit of the QWI is the quarterly nature of the data which provides more frequent snapshots of employment variables and thus gives a better feel for the dynamics at play and the impact of the program over time.

The disadvantages of the QWI lie in its coverage. Most states have now agreed to share UI data with the LEHD system but the historical data they provide differs by state. As a result, I use data from the 40 states whose data goes back until at least 2000 so that there is a reasonable pre-treatment period for each state. The states used in the analysis are listed in the Data Appendix. The QWI also takes a different approach to disclosure concerns. For cells with few observations noise is infused to ensure confidentiality. This allows for the QWI to report data for a higher percentage of cells than the CBP. Employment is reported for 97.5% of cells in the QWI and number of hires is reported for 96.2%. Despite the differences in the CBP and QWI samples the employment regression results are similar using both data sets. This lends credence to the use of the QWI as valid source for exploring other outcome variables not observed in the CBP. See the data appendix for additional details on both the QWI and the CBP.

3.3 NBER Productivity Database

After obtaining annual (or quarterly) state-industry labor data, I merge in three-digit-industry energy intensity data from the 1998 NBER Productivity Database. This database contains total energy expenditure by industry in the given year and is based on the Census of Manufactures and the Annual Survey of Manufactures. To construct an energy intensity index for the 21 different 3 digit manufacturing industries, I divide total industry energy expenditure by total value of shipments for the industry.[12] As seen in Table 1, energy intensity in the manufacturing sector varies from a low of 0.6% in the computer and electronic product industry to a high of 4.8% in the primary metal manufacturing industry.

[12]Because the NBP regulated NO_x emissions from heat, steam and electricity production I use energy intensity rather than electricity intensity.

4 Econometric Model and Results

In order to motivate the econometric analysis, provide summary statistics and preview the results, it is informative to begin by viewing the raw employment data between 1998 and 2009. Based on the energy-intensity index in Table 1, I split the 21 industries into three separate groups. The seven industries with the highest energy intensity measures are defined as "high intensity industries", the seven industries with the lowest energy intensity measures are defined as "low intensity industries" and the middle seven are defined as "medium intensity industries".

Figure 3 plots out the NBP - Non-NBP employment difference for each industry energy intensity grouping from 1998-2009. Specifically, the figure plots the percentage change in employment in the NBP region minus the percentage change in employment in the non-NBP region for each industry grouping, using 1998 as the baseline year. This plot suggests a potential effect of the NBP on employment, but it also reveals that pre-existing trends may be present that, if unaccounted for, could bias DDD estimates. The plot shows that following 2003 the NBP - Non-NBP employment difference falls most prominently for the high intensity industries while there is little change in the NBP - Non-NBP difference for the low-energy industries. Employment in medium intensity industries tracks closely with high intensity for the first four years, but starting in 2003, the NBP - Non-NBP employment difference begins to fall for high energy industries relative to medium energy industries. This gap widens substantially between 2004 and 2008. Vertical lines are drawn in 2001, the year after the courts determined the NBP's legality, and in 2003, the start date of the program. Overall, the raw data suggest that the NBP - Non-NBP employment difference fell more in high energy industries following the implementation of the program. A regression model which exploits the industry, geographic and timing variation of the policy *and* controls for other factors potentially driving remaining differences is presented below.

4.1 Baseline Regression Specification

The identification strategy of this paper takes advantage of the geographic, time and industry heterogeneity found in the data. As a first step towards exploiting this heterogeneity, I consider the following DDD model:

$$y_{gkt} = \beta_T(Post_{gt} \times NBP_g \times EnInt_k) + \theta x_{gkt} + \delta_{gk} + \alpha_{kt} + \gamma_{gt} + \epsilon_{gkt} \tag{1}$$

In this model, y_{gkt} is the employment outcome of interest (logged employment, hiring rate, separation rate, etc.) in state (or county) g, in industry k in period t. $Post_{gt}$ is an indicator variable equal to one if the date is after the start of the NBP.[13] The variable NBP_g is set equal to one for all states whose electric utility provider is impacted by the NBP. [15] Figure 4 provides a map of the treated region and provides additional details.[16] Finally, the variable $EnInt_k$ is a time-invariant measure of the industry's energy intensity as defined by total energy expenditure divided by total value of shipments for the entire industry in 1998. The primary employment specifications will be at the state level such that gkt is state-industry-year data. Other specifications will use county level data where a unit of observation is at the county-industry-year level and g refers to county rather than state.

The main coefficient of interest is β_T, which captures the triple interaction of an observation being in the NBP region, after the treatment has been applied and allowing for differences by industry based on their energy-intensity level. The variables δ_{gk}, α_{kt} and γ_{gt} represent full sets of state-industry, industry-year and state-year fixed effects in order to control for time-invariant differences across state-industries and non-parametric trends for each state and industry.[17] A vector of variables, represented by x_{gkt} is included to control for other factors that might differentially impact labor market outcomes for high energy industries in the NBP region.

Possible factors that may drive differential changes in labor market outcomes by both

[13]Results using QWI assign the start time likewise as the quarter following the NBP. Linn (2010) performs a similar analysis in which he estimates the impact of the NBP on electric utility profits. His empirical work, which examines the impact on utility stock prices, uses 2000, the year the D.C. Court of Appeals ruled in favor of the EPA in *Michigan et al. vs. EPA et al.* The program began in May of 2003 in eight states and in May of 2004 for eleven states. For the eight states beginning in mid 2003, $Post_{gt}$ equals one for years 2004 and later. For all other states $Post_{gt}$ equals one for years 2005 and later.[14] As can be seen in Figure 2, electricity production itself was not altered until the NBP was implemented. For the purposes of this analysis I assume that manufacturers did not react to the policy until electricity production was actually altered and the price of permits had been established. More flexible event-study models presented in Section 4.5 fail to detect any anticipatory action by firms prior to 2003.

[15]The baseline specification uses a broad definition of the variable NBP. The treated region extends to include regions which purchase electricity from utilities that are regulated by the NBP. For example, the treated region extends to the Midwest because manufactures in these states participate in a common day-ahead electricity market run by the Midwest Independent System Operator, many of whose electric utilities were regulated by the NBP. Conversely, in the southeast, electricity markets do not cross state borders. The results are shown to be insensitive to alternate specifications that restrict the treated region to only those states and counties that were directly regulated.

[16]Note that the concern over defining the treated region also precludes the use of border discontinuity methods to identify the impact of the NBP as there is no clearly defined geographic cutoff.

[17]Note that the variables $Post_{gt} \times NBP_g$, $NBP_g \times EnInt_k$ and $Post_{gt} \times EnInt_k$ as well as state, industry and year indicator variables are absorbed by higher order two-way fixed effects.

region *and* industry include fuel prices and other environmental regulations. If a change in, for example, the price of oil, raises energy prices uniformly across the country, then this will be picked up by the industry-year fixed effects. However, some regions rely more heavily on certain fuels for energy and electricity than others. To account for the possibility that regional electricity prices may be differentially impacted by changing relative fuel prices, I obtain average oil, natural gas and coal prices for the years 1998-2009 as well as the percent of electricity that is derived from that source in each North American Electric Reliability Corporation (NERC) region in the country. Interacting the fuel price with the percent of electricity derived from that fuel in the state's NERC region and the industry indicator variables allows for the fact that certain industries in certain regions may be particularly sensitive to a change in fuel prices.[18]

A second potential driver of labor market outcomes is other environmental regulations that disproportionately impact high energy industries in the NBP region. One important regulatory change was a tightening of the NAAQS ozone nonattainment standards which resulted in 408 counties across the country entering nonattainment status in 2004. These counties were disproportionately located in the NBP treated region, and nonattainment designation is likely to differentially impact industries based on their energy intensity. To control for this, I examine county-industry data and allow for high energy industries in new NAAQS nonattainment industries to experience a separate employment effect. Results are shown to be robust to including these controls in the regressions.

Before discussing the identification assumptions and results, a few important details bear mention. First, as is common in the literature, observations are weighted by their pre-treatment (1998) employment levels to ensure that state-industries with little or no employment do not drive the results (Greenstone 2002; Walker 2011). Second, while using aggregate state-industry data reduces the computational burden and accounts for *some* of the inference concerns raised by (Bertrand *et al.* 2004), given that the identifying variation occurs at a higher geographic level than the state, it is crucial to account for serial and spatial correlation of the error term to avoid understating the size of the standard errors. The standard errors are clustered at the NBP region - Industry level to

[18] Annual oil, natural gas and coal prices are the Brent Price, the Henry Hub Natural Gas spot price and the EIA total average coal price and were downloaded from `http://www.eia.gov/totalenergy/data/annual/showtext.cfm?t=ptb0709`. NERC region resource mix data comes from EPA's eGRID summary tables. Georgraphic boundaries of NERC regions do not always correspond to state boundary lines. For those states which belong to more than one NERC, I assign them a fuel-intensity level equal to a weighted sum of the fuel-intensity level of the NERCs to which they belong where the weight is the percent of manufacturing employment in the state that lies in that NERC region.

address these concerns.[19] The error term is represented by ϵ_{gkt} and must be uncorrelated with the triple interaction term conditional on the set of control variables and fixed effects. Specifically, the assumption is that

$$E[\epsilon_{gkt} \times (Post_{gt} \times NBP_g \times EnInt_k) | x_{gkt}, \delta_{gk}, \alpha_{kt}, \gamma_{gt}] = 0.$$

A final feature to note is that the model assumes a linear effect in energy intensity. When logged employment is the outcome variable, the triple difference coefficient should be interpreted as the percentage change in employment that occurs for every additional percentage point in energy intensity. While there are strong theoretical reasons to believe that the most energy intensive industries will experience the greatest impact from the NBP, deciding exactly how to model this differential impact is not immediately straightforward. Imposing a linear assumption allows for results to be obtained in a single DDD coefficient which is easily interpretable for the manufacturing sector as whole. However, I provide additional results which calculate the impact of the NBP separately on each of the 21 industries. This is done by replacing the triple interaction variable with 21 separate industry-specific triple interaction terms. Results from this non-parametric approach, presented later, reveal that the linearity assumption is not unreasonable.

4.1.1 Identification

While the model's fixed effects control for industry and state-specific shocks that occur in a particular year, for β_T to identify the causal effect of the policy, industries in the NBP region must have similar trends to their counterparts in the Non-NBP region prior to the regulation. Two strategies are employed to examine potential pre-existing trends. The first directly controls for this possibility by allowing each industry to have it's own region specific linear trend. Equation 2 represents the set of estimated models with these

[19] In separate results not presented here, standard errors were clustered at a variety of other levels including state-industry, state and NBP region. Clustering at the NBP region-Industry level accounts for spatial and serial correlation within an industry over time and proved the most conservative of all clustering methods.

region specific industry trend controls.

$$y_{gkt} = \beta_T(Post_{gt} \times NBP_g \times EnInt_k) + \theta x_{gkt} + \delta_{gk} + \alpha_{kt} + \gamma_{gt} + \\ + \sum_{k=1}^{K} \beta_e^k[trend_t \times NBP_g \times 1(Ind_i = k)] + \sum_{k=1}^{K} \beta_w^k[trend_t \times NonNBP_g \times 1(Ind_i = k)] + \epsilon_{gkt} \tag{2}$$

Apart from the summation terms, the model is identical to equation (1). For each industry $k = 1$ through K, that industry's indicator variable is interacted with the region dummy (NBP_g), and a linear trend variable ($trend_t$). These summation terms account for pre-existing trends by directly including them in the model.

A separate way to examine the presence of trends is to use an event-study framework which allows for the impact of the regulation to evolve over time. This model, discussed more formally in Section 4.5 makes no assumptions about the start date of the program, rather it estimates the differential time-path of the outcome variable for the affected observations relative to the unaffected observations in every period of the data using the start date of the program as the reference period. Using data prior to the start of the program allows for the detection of any trend in the outcome variable that was present before the program's implementation. Results from the event-study models suggest the presence of only a small pre-existing trend.

Identification of causal estimates for this class of differences-in-differences models also rests on the stable unit treatment value assumption. While impossible to fully test for, the multiple sources of variation in the data do allow for a test to determine the extent to which particular spillovers may be driving the results. One concern is the presence of geographic spillovers whereby firms in high energy industries relocate production to the unregulated region. If the policy were to result in firms shifting labor from plants in regulated to unregulated states then a more parsimonious differences-in-differences model can be used which exclusively exploits changes in employment across industries within affected states. If, on the other hand, local labor market spillovers are a concern, whereby workers in affected industries leave to work in the unaffected industries, then models can be run which only exploit the across state variation in labor market outcomes for high energy industries. Results from these models are found in Table A4.

4.2 Employment Results Using County Business Patterns

The main employment results of the paper are found in Table 2 and use employment data from the CBP, which contains data at the state-industry-year level between 1998 and 2009. The top row of the table reports results for β_T, the primary coefficient of interest. The columns progressively add sets of controls leading to the preferred specification in Column 6. This is the model provided in equation 2 and includes state-industry, state-year and industry-year fixed effects as well as separate industry by region trends. Tables 3 and 4 report a variety of different specifications using a similar table layout but alternate samples and control variables. In each table the coefficient on the primary triple difference regressor $Post_{gt}\text{x}NBP_g\text{x}EnInt_k$ is reported as well as the coefficient on the $Post_{gt}\text{x}NBP_g$ variable for those specifications in which it is not absorbed by higher order fixed effects. The coefficient on the triple difference variable signifies the percentage change in employment that occurred for industries with an additional percentage point of energy intensity after the policy was enacted, in the states to which it applied. The coefficient on the variable $Post_{gt}\text{x}NBP_g$ represents any change that occurred to all manufacturing employment in the NBP region relative to the non-NBP region conditional on the differential impact by energy intensity and other controls. This coefficient, when it is not absorbed by state-year fixed effects, is close to zero in all of the specifications, thus supporting the case that the primary mechanism through which the policy impacted employment was through a heterogeneous treatment effect that varied by the industry's energy intensity.

Column 1 of Table 2 reports a very basic model where only state-industry and year fixed effects are included. The results suggest a large but statistically insignificant impact of the NBP on manufacturing employment in higher energy industries. The imprecise nature of these estimates is unsurprising given that much of the employment change within a state industry is likely to be driven by exogenous state and industry-specific trends which are not controlled for in this base model.

Column 2 of Table 2 accounts for these trends by including linear state trends and industry-year fixed effects. Column 3 relaxes the assumption of linear state trends by using more flexible state-year fixed effects. The inclusion of industry-year indicator variables accounts for any industry-specific shock that is common to all states in a given year while the inclusion of state-year indicator variables accounts for any shock that is common to all manufacturing employment in a state in a given year. While the non-parametric specification provides the model with a high degree of flexibility, the large set of indicator variables, particularly the set of state-year dummies, are quite demanding of the data and reduce the degrees of freedom in the regression.[20] Results using the fully non-parametric approach with sets of state-year and industry-year indicator variables are given in Column 3 of Table 2. The triple difference coefficient is negative and statistically significant, implying that employment in high energy industries fell relative to low energy industries and relative to other high energy industries in non-treated states.

Columns 4-6 repeat the specifications used in Columns 1-3 but now include separate region-by-industry trends to account for the possibility that, for example, high energy industries in the NBP region may have been trending differently than their counterparts in the Non-NBP region before the start of the program. If high energy industries in the NBP region are trending down faster than high energy industries in the geographic control group then failing to account for these trends will overstate the impact of the policy. While including these trends is essential to accurately estimating the impact of the policy, it is also important to note that if the NBP lead high energy industries to experience a change in employment levels as well as a change in trends, then including these trend variables may pick up some of the impact of the program thus masking its full effect.

The size of the estimates in Columns 4-6 are lower than their counterparts in Columns 1-3 and suggest that accounting for these separate region by industry trends is important. The new coefficients, now ranging from -1.38 to -1.23 imply a smaller but still sizable impact of the NBP. The coefficient in Column 6, which includes the full set of 588 state-year indicator variables is statistically significant at the 10% level. If we are willing to replace these state-year fixed effects with a more parsimonious set of forty-nine state-specific trend variables then the coefficient becomes statistically significant at the 5% level with little impact on its magnitude. This result is reported in Column 5. The coefficient for the primary specification (found in Column 6 of Table 2) is -1.23. This implies that for each additional point of energy intensity, an industry's employment level declined by 1.23 percent.

Interpreting the results requires consideration of the assumptions at hand and the identifying variation used in the analysis. Most environmental regulation papers assume that some group of industries are unaffected by the regulation and thus the overall impact can be calculated by comparing the employment change in "clean" industries

[20]The data set used in the main specification contains 12,348 observations. Including state-year indicator variables adds 588 additional variables to the regression. Using linear instead of non-parametric state trends reduces the number of state-trend variables from 588 to 49.

to the employment change in "dirty" industries. This paper, by using energy intensity rather than a binary clean / dirty industry measure does not provide an immediate control group against which to compare the loss. One simple way to calculate the overall impact that is consistent with other work is to use the coefficient to compare employment changes in the most energy intensive industries relative to changes in the least energy intensive industries. Ninety-three percent of manufacturing plants that were directly regulated by the NBP were in industries in the top quartile of the energy intensity index. The average energy intensity of industries in the top quartile is .0389 while the average energy intensity of industries in the bottom quartile is .0067. Multiplying this difference times the triple difference coefficient would imply a 3.9% employment loss in high energy industries relative to low energy industries. This is the equivalent of a loss of 46,804 jobs. This is a quite conservative interpretation of the overall loss as it assumes no impact on industries in the middle two quartiles of the energy intensity index. A more realistic approach would calculate an employment loss for every industry relative to a theoretical baseline industry that was not impacted by the regulation. If we assume that there was no overall shift in manufacturing employment (a reasonable assumption based on small $Post_{gt}\text{x}NBP_g$ coefficient) and that all losses are relative to the least energy intensive industry then an employment loss figure could be calculated by multiplying the pre-regulatory employment level in each industry in the NBP region times the difference between the energy intensity of that industry and the baseline industry and then times the triple difference coefficient. This would imply an employment loss of 110,024 jobs. Specifically, Emp Loss = $\sum_{k=1}^{K}(emp_k \text{ x } (EnInt_k - .0059) \text{ x } 1.226)$ = 110,024 where emp_k is the employment level in the NBP region in 2002, .0059 is the energy intensity of the baseline industry and 1.226 is the triple difference coefficient in Column 6 of Table 2.

4.3 Alternative Employment Specifications

To examine the robustness of the result in the preferred specification, I consider models which: (1) vary the specific controls in the regression model; (2) more narrowly define the treated region; (3) examine county-level rather than state-level data and (4) relax the assumption of a linear employment effect in energy intensity. Results from a separate set of event study models are presented in Section 4.5.

Panel A of Table 3 reports results from identical models as Table 2 but controls for exogenous changes in fuel prices that may impact the energy costs of certain regions more than others. Any shock in energy prices that is common to the entire country will be picked up in the industry-year fixed effects but if, for example, there is a shock to the price of oil and certain regions heavily rely on oil for electricity, then it is possible that these regions will see an increase in the cost of energy that is unrelated to the NBP. To ensure that shocks to oil, natural gas and coal prices are not driving the results I gather data on annual fuel prices for each of these fuels and interact these prices with the percent of electricity that is derived from that fuel in the NERC region to which that state belongs. This variable is then interacted with the energy intensity variable to control for the fact that high energy industries in certain regions may be particularly sensitive to certain fuel price shocks. The results are shown to be insensitive to these controls.

Panel B of Table 3 uses an alternate definition for the treated region. Rather than use the broad definition described in Figure 4, this specification defines the NBP region as only those states that were directly impacted by the program. Using this narrower definition, North Dakota, Minnesota, Iowa, Wisconsin, Maine, New Hampshire and Vermont are dropped from the sample. Results using this new definition of the treated region find the coefficient on the triple difference variable to be -1.24 and statistically significant at the 5% level.

Table 4 provides new estimates of the NBP's employment impact using county rather than state level data. The use of county level data comes with both benefits and costs. The primary reason why county data may be useful is that it allows the specifications to control for county level changes to the NAAQS nonattainment standards that occurred to 408 counties in 2004, the first year that the NBP was fully implemented. These counties were located across the United States but a disproportionate number were located east of the Mississippi. Using county data also allows for additional robustness checks that allow the NBP treatment group to be defined along county rather than state lines. While the NBP was a state level program, certain counties in both Alabama and Michigan were ruled exempt of the regulation and can be excluded from the treated region when using county level data.[21] The major downside of the county level data is the increased prevalence of data suppression. Due to disclosure concerns, employment for 49% of all observations is suppressed.[22] When suppressed, employment is imputed by assigning the midpoint of the employment range provided in the data as done in (Kahn

[21]As discussed earlier these counties were likely still impacted by the policy as their utility providers produced electricity in NBP regions.

[22]Cells for which data are reported contain 72% of all manufacturing employment. Suppression of the state data occurs in only 7% of cells, with those 7% being imputed using the same method described in the text.

& Mansur 2013). This imputation process reduces the true employment variation in the data and raises other concerns regarding when cells may fall into and out of suppression status. Panel A of Table 4 reports results using a nearly identical set of regression specifications in Table 2 but at the county rather than state level. County-industry fixed effects control for any time invariant differences in employment levels and industry, state and industry by region trends are controlled for in an analogous manner. Reassuringly, Panel A reveals the county level results to be of similar magnitude to their state level counterparts. Results are no longer statistically significant for Columns 2 and 3 but are statistically significant at the 5% level in Column 6, the preferred specification which includes separate region by industry trends.

Panel B of Table 4 restricts the treated region to only the counties that were directly impacted by the NBP. The results for the preferred specification suggest a slightly smaller impact than suggested by the state level counterpart, but the coefficient falls well within the confidence interval of the state level results. The final important use of the county level data is to control for changes in county level NAAQS nonattainment standards. To isolate the impact of the NBP from any impact of the NAAQS, I include a term in the econometric model which interacts 2004 county NAAQS nonattainment status with the $Post$ and $EnInt$ variables.[23]

As a final specification, I return to the state-level data and consider the effect that the NBP had separately on each specific manufacturing industry. To do this, the main triple difference interaction variable is replaced with 21 different industry-specific interaction variables.[24] The coefficients on the industry specific interaction variables represent the estimated effect of the NBP on each industry. Figure 5 plots each of the resulting coefficients on the y-axis and the industry's energy intensity on the x-axis. As expected, the higher the energy intensity of the industry, the greater it was impacted by the NBP. The apparent linearity of the relationship between an industry's energy intensity and their NBP related employment change supports the identification strategy used in the triple difference models.

[23]Specifically, a NAAQS nonattainment indicator variable is set equal to one for all counties that enter nonattainment for any criterion pollutant in 2004. This variable is then interacted with the $PostxNBP$ variable and included in the model. Panel C shows that the results in the preferred specification are robust to including these controls.

[24]That is, I drop the $Post_{st} \times NBP_s \times EnInt_k$ variable and replace it with $Post_{st} \times NBP_s \times Ind1_k$, $Post_{st} \times NBP_s \times Ind2_k$, $\ldots$, $Post_{st} \times NBP_s \times Ind21_k$ where $Post_{st} \times NBP_s$ has been separately interacted with each of the twenty-one industry indicator variables ($Ind1_k, \ldots, Ind21_k$).

4.4 Worker Flows and Earnings by Age Group: Results from the Quarterly Workforce Indicators Data

While the finding of an employment decline is an important and policy relevant outcome, estimating the NBP's impact on industry employment levels does not fully capture the impact of the regulation on worker welfare or labor markets. The cost of employment declines will vary based on which workers lose their jobs and how these employment adjustments occur. The remainder of this paper attempts to gain insights into these questions by using the QWI, a relatively new data set which observes all hires, separations, the earnings of the newly hired workers and the demographic characteristics of the labor force.

Learning the effects of the policy on these important outcomes provides policy makers and economists with a far richer understanding of the impact of environmental regulation on labor markets. A primary reason these outcomes are important is that they shed light on the extent of the transitional costs that workers face when their industry is hit by regulation. If firms respond to regulations by laying off long-time employees there will be a significant negative impact on incumbent workers. On the other hand, if firms respond by reducing hiring rates and allowing employment reductions to occur through retirements and quits then incumbent worker will largely be protected while new and potential new workers will be impacted.[25]

Past research has shown transitional costs can be large for workers that experience involuntary separations (Jacobson *et al.* 1993) and these costs should be given particular attention when considering the effects of regulation (Walker 2013).[26] One final yet very important margin on which firms may adjust to regulation is through worker earnings. A change in earnings will clearly have implications for worker welfare. Using data from the QWI on the earnings of newly hired workers allows for these changes to be captured in a way that previous research has been unable to observe. Summary statistics for these variables are found in Table 5.

[25]In a general equilibrium framework equilibrium framework there are a variety of mechanisms through which incumbent workers will be harmed even if all employment reduction occurs through a decrease in new hiring. For example, if the shock is "large" a reduction in hiring will slow job-to-job transitions, thus cutting off an important path through which workers "climb the ladder" (Moscarini & Postel-Vinay 2013).

[26]Workers that involuntarily separate from a firm are shown to experience extended periods of unemployment, long run earnings losses and even increased mortality rates (Jacobson *et al.* 1993; Sullivan & von Wachter 2009; Davis & von Wachter 2011; Farber 2012).

4.4.1 Worker Flows and Labor Adjustment

Before reporting results on worker flows it is important as a validity check to begin by replicating the primary employment regression described in equation (2) using employment data from the QWI. Column 1 of the first row of Table 6 shows the DDD estimate using QWI data is smaller (-0.871 as compared to -1.23) but it remains negative and statistically significant at the 5% level. That these two estimates differ is not altogether surprising for two reasons. First, employment is measured differently in the QWI than the CBP. The QWI calculates "stable" employment, which is defined as the number of workers who have been present at the firm for at least three months, rather than the point-in-time employment measure used by the CBP. Second, the QWI data contain only the forty states for which there is employment data back to 2000. These QWI employment results suggest that industries in the top quartile of the energy intensity distribution lost 2.8% of employment relative to industries in the bottom quartile of the energy intensity index.

The first row of Columns 2 and 3 of Table 6 estimates the impact of the regulation on worker flows, using the same specification as that described in equation (2). For Column 2 the dependent variable, y_{gkt} in equation (2) is now defined as the hiring rate rather than the natural log of employment. Specifically, the hiring rate is the number of new quarterly hires divided by the total employment. The coefficient reported in Column 2 of the first row in Table 6 is -0.225. This implies that for every additional percentage point in energy intensity, an industry decreased their hiring rate by .225 percentage points. As seen in Table A2 the average quarterly hiring rate for all manufactures over this time period was 7.6 per 100 employees. This coefficient would imply that there were 1.24 fewer quarterly hires per 100 workers in the most energy intensive industry. The program's impact on separation rates points negative as well but is not precisely estimated.

The hiring and separation results suggest firms may have lowered employment through reductions in hiring rather than increases in layoffs. If true then we would expect to see no concomitant rise in the periods of non-employment for workers separating from a high energy industry following the NBP. Column 4 of Table 6 report these results. The estimates reported in row 1 of Column 4 show there to be no clear evidence that the NBP increased the average period of non-employment for workers who separated from high energy industries following the NBP.[27]

[27] Over the recession, average quarters of non-employment for separating workers jumped from 1.6 to 2.1 (author's calculation). If workers who separated from regulated industries see no increase in periods of

Table 6, in addition to reporting the impact on periods of non-employment for separating workers, reports the major findings for employment and each of the key flow variables in the QWI by age group. The ability to match worker characteristics to the firm at this level of detail is a feature that is unique to the QWI and allows us to explore the heterogeneity of the impact by worker age. Each cell reports the coefficient from a separate regression using a different outcome-age group combination. All regressions use the main specification listed in equation (2). The first row of Table 6 uses the entire population of workers. Breaking these results down by worker age grouping reveals interesting patterns. The heterogeneous impact of the NBP on employment and periods of non-employment following a separation are especially important. If firms decrease their hiring rates then employment declines will first occur for worker groups with high levels of turnover. For example, young workers are both hired and separate from firms at a faster clip than other workers. When firms stop or reduce their hiring, the flow of new workers into the firm declines. However, the flow of workers out of the firm (separations) continues to be higher for the young than the old.[28] Therefore, the employment drop will be greatest for young workers. This story is consistent with the change in employment flows that are observed during macroeconomic downturns (Davis & Haltiwanger 2014).

Column 1 reports the employment change by age-group. The coefficient for the age 22-24 category is -2.12 while the coefficient for age 55-64 category is only -0.30. This shows a far greater drop in employment for younger workers than for older workers. The dynamics of the age-group specific employment drop are explored more in Section 4.5. Following the NBP, hiring rates fall in high energy industries for nearly all age groups but employment declines are largest for the youngest groups. Consistent with the story that firms adjusted through natural separations rather than layoffs is the lack of any observed increase in separations for middle aged workers. Furthermore, there is no observed increase in periods of non-employment for those middle-aged workers that do separate. This is important because middle-aged workers are a group with significant industry and firm specific capital and past research has shown that the costs of non-voluntary separations for this group are particularly large (Jacobson *et al.* 1993; Walker

non-employment, this suggests they may have shifted quickly to other jobs. An increase in this variable, particularly for middle aged workers, suggest regulation may have forced people into unemployment. This finding is explored more in the following section. See the Data Appendix for more details on this variable.

[28]Figure A2 displays worker separation rates by age. Rates are significantly higher for younger workers than older workers.

2013). That middle aged workers experience no clear increase in separations or periods of non-employment provides further evidence that firms' method of employment adjustment had minimal costs to incumbent workers.

4.4.2 Worker Earnings: Incumbents vs. New Hires

The QWI is also able to shed light on the NBP's impact on workers' earnings in a way that no other publicly available data set can. Evaluating the earnings impact of regulations is crucially important to an understanding of how the NBP impacted labor markets. However, due to existing contracts, firms are often unable or unwilling to immediately adjust the hours and wages of incumbent workers. As a result, changes in average worker earnings occur slowly because they are dominated by incumbent workers. New hire earnings provide a margin over which firms are most likely to be able to adjust (Curtis *et al.* 2013). The QWI provides data not only on average earnings of all workers, but also on the average earnings of all new hires. Using the econometric model from the main specification described in equation (2), I estimate the impact of the NBP on worker earnings by defining y_{gkt} as the natural log of worker earnings. While the NBP appears to have no distinguishable effect on earnings of all workers, there is a statistically significant decline in the earnings of new hires. The DDD coefficient is -1.277 for the *all* age category and the decline is relatively consistent across age groups. This implies a decline in new hire earnings of 4.2% for industries in the top quartile of the energy intensity index compared with those in the bottom quartile. No decline is observed when examining average earnings of all workers which consists primarily of incumbent workers whose contracts were negotiated before the regulation. The ability to change the earnings of the newly hired is an important adjustment margin for firms. This adjustment has not previously been captured due to a lack of data.[29] While not modeled explicitly in this paper, the drop in new hire earnings could be driving the observed decline in turnover rates as workers will be less likely to leave their existing jobs if their outside options are made less attractive.

4.5 Event-Study Models

To understand how the effects of the policy evolved over time it is useful to perform an event-time study. In the context of the above models, an event-time study tracks how the

[29] Importantly, the new hires earnings decline may represent a decline in either wages or a decline in hours.

coefficient on the variable $NBP_g \times EnInt_k$ changes throughout the study period. I use the second quarter of 2003 as the start date and normalize the coefficient for this period to be zero. Specifically I estimate the following model:

$$y_{gkt} = \sum_{t=-m}^{M} \beta_T^t [1(Qtr_q = t) \times NBP_g \times EnInt_k)] + \theta x_{gkt} + \delta_{gk} + \alpha_{kt} + \gamma_{gt} + \epsilon_{gkt} \quad (3)$$

This model mirrors that of equation (2) but removes the industry specific NBP - Non-NBP trends and replaces the triple interaction variable with forty-four event-time coefficients. This model allows us to view trends both before and after the implementation of the NBP.

Figure 6 plots the event-time coefficients on the logged employment model. The coefficients before the policy are slightly positive but none are statistically different from zero. The coefficients become negative after the NBP's implementation with zero falling outside of their confidence intervals. The slight downward slope prior to the NBP suggests that employment in high energy industries in the NBP region may have been trending downward prior to its implementation. Regression results from the main specification of the paper in Table 2 include separate region by industry trends to control for the differing trends but it is also useful to simply visualize the extent to which they impact the results. Furthermore, this allows for a visual inspection as to whether firms anticipated the regulation and began responding before its implementation. The relatively flat line in the quarters leading up to the law's implementation suggest that firms did not react to the law until it began in full.

Figure 7 shows the event-study results for five different age groups of workers. Rather than use the logged value of total employment, each regression examines the impact on employment levels of a particular age group. For clarity, the plot removes confidence intervals and examines only the main working age categories. From the plot, it is clear that employment of young workers experienced the largest fall while employment of older workers was largely unaffected.

As a comparison, Figure 8 reports the same coefficients where the left hand side variable is periods of non-employment for separating workers. The pattern here is quite distinct from that seen in Figure 6. Here, the coefficients consistently hover around zero and are not statistically significant before or after the NBP. The NBP appears to have had no noticeable impact on periods of non-employment for separating workers. This analysis is a particularly important compliment to the the DDD results for this outcome variable. DDD models take the average of the pre-period and compares it to the average outcome in the post period. This model may not uncover an impact of the policy if the impact is short lived. By contrast event-time studies capture quarter-specific policy effects. The plot in Figure 8 shows that there was no clear positive spike in quarters on non-employment for separating workers in any of the quarters following the NBP.

5 Discussion

Taken as a whole, the results paint a picture of how labor markets in energy intensive industries responded to the NBP. The findings show that employment in industries in the top quartile declined 3.9% compared to manufacturing employment in the bottom quartile and that under certain assumptions discussed at the end of Section 4.2, employment declined by 110,024 jobs.

In light of this finding it is useful to revisit the causal mechanisms that connect the NBP to manufacturing employment. First, NO_x emissions from large manufacturing plants that produced their own energy were directly regulated by the program. This direct regulation may have led existing firms to decrease employment and discouraged new firms from locating in the regulated region. Second, firms may have adjusted their production location decisions and / or their input bundles in response to an increase in the price of electricity or uncertainty regarding the future price of electricity.

These causal mechanisms suggest two strands of literature that may be helpful in interpreting the magnitude and plausibility of the findings. Given that part of the effect came through the direct regulation of plants, it is useful to simply compare the labor market impacts with findings from previous work on the impact of other major environmental regulations. Greenstone (2002) finds that the NAAQS resulted in a loss of 590,000 manufacturing jobs while Walker (2011) finds that the portion of the 1990 CAAA that changed attainment standards resulted in a 15% loss in employment in regulated sectors.

The second way to interpret the magnitude of the findings is to make the (likely extreme) assumption that the entire effect occurred through an increase in electricity prices and then determine the electricity price change that would be required to cause the observed decline in employment.[30] A few recent papers have examined the employment energy price elasticity (Davis & Haltiwanger 2001; Deschenes 2010; Kahn & Mansur 2013). The most relevant to this paper is Kahn & Mansur (2013) which estimates

[30] Employment may also respond to increased uncertainty over future electricity prices but this subject has yet to be explicitly studied by economists.

employment-electricity price elasticities for manufacturing industries at the three-digit NAICS level. Kahn & Mansur (2013) find an employment electricity price elasticity ranging between -0.15 for the computer products industry to -1.17 for the energy intensive primary metals industry. Based on the Kahn & Mansur (2013) elasticity, a 5.9% increase in electricity prices would be needed to account for the observed employment decline. This figure is likely to substantially overstate the actual "required" electricity price increase because it assumes that employment changes occurred entirely through electricity price increases and that there was no impact of the direct regulation.

To the best of my knowledge, only two ex-ante simulations of the NBP estimated the impact it would have on electricity prices and there have been no ex-post studies. The first, performed by the EPA (1999) predicted an increase in electricity prices of 1.6%. The second, performed by Platts Research and Consulting titled "The NO_x Challenge", predicted an increase of $1-$3/MWh in the price of wholesale electricity or an increase of between 2.47% and 7.42%. Fowlie (2010) notes that in every state where electricity is regulated, firms successfully petitioned for rate base adjustments in order to cover the compliance costs of the NBP. A thorough ex-post evaluation of the pass-through emissions costs to electricity prices is beyond the scope of this paper but Appendix A5 reports results from straight-forward differences-in-differences model that use state-year electricity price data to test for whether electricity prices rose in NBP states following the regulation. In short, the main DD coefficients are positive and of similar magnitude to ex-ante estimates but they are not precisely estimated.

Past research on the employment effect of environmental regulation and electricity price increases add credence to the main employment findings. But of perhaps more importance are the changes to other labor market outcomes. Hiring rates in energy intensive industries are shown to have fallen while periods of non-employment for separating workers did not increase. This finding suggests that firms may have reduced employment through voluntary separations, such as retirements and job-to-job transitions rather than involuntary transitions (layoffs). Given the presence of unions and labor adjustment costs, such as severance packages and increased unemployment insurance taxes, it is not surprising that firms will choose to adjust to small demand shocks primarily through natural separations. Figure A1 shows the difference in the employment path between a firm that adjusts through natural separations and a firm that adjusts through layoffs.[31] Although not explicitly modeled here, it is clear to see that employment adjustment

[31] There is a large literature on firm adjustment costs and their implications. See Hamermesh (1989), Caballero & Engel (1993), Bloom *et al.* (2007) and Cooper & Willis (2009) for a few examples.

decision of a firm will depend on the cost of layoffs and the size of the shock. Distinguishing between layoffs and voluntary separations is important as a variety of studies have found that workers who experience mass displacement events experience extended periods of unemployment, large earnings losses and increased mortality rates (Sullivan & von Wachter 2009; Davis & von Wachter 2011; Farber 2011). Regulation's impact on hiring also provides a potential explanation for the aging of the manufacturing workforce in the United States. Young workers have high levels of turnover and separate from firms at a much faster rate than old workers (see Figure A2). Therefore, when a firm stops hiring, their employment of young workers will shrink faster than their employment of old workers. Unions, to the extent that they require shrinking firms to dismiss workers based on seniority will also exacerbate this aging effect of regulation.

On the earnings side, we observe a drop in the earnings of new hires but no such drop in the overall average earnings. This drop in new hire earnings is important for two reasons. First, it could explain the decrease in worker turnover in energy intensive industries. If wage offers decline then workers will be less likely to leave their existing job for a new one. This will decrease worker turnover rates for impacted industries. Second, recent research has found a one-to-one correspondence between new hire wages and productivity (Haefke *et al.* 2013), suggesting that the observed decline in new hire earnings may be indicative of regulation induced productivity declines such as those found by (Greenstone *et al.* 2012).

The earnings results, together with the worker flow findings, paint a more nuanced picture of the impact of environmental regulation on labor markets. Incumbent workers do not bear the brunt of regulation's impact. The effects are felt by future workers who receive lower earnings, and potential future workers, who are no longer hired due to the regulation. These effects are important to labor markets and the economy but are quite distinct from the traditional job loss story that portrays long-time incumbent workers as the only losers from environmental regulation.[32]

6 Conclusion

This paper has examined the impact of the NO_x Budget Trading program on a variety of labor market outcomes in order to better understand the costs associated with envi-

[32]The results may also be interpreted in light of the endowment effect which would suggest it is more painful for an incumbent worker to lose their job than it is for a potential worker to not receive a job. In such case, incumbent workers should be given extra consideration when considering the costs of environmental regulation.

ronmental regulation of energy producers. As a result of the program, manufacturing industries in the top quartile of the energy intensity index are seen to have reduced their employment levels by 3.9% relative to industries in the bottom quartile. Under certain assumptions discussed above this represents an employment loss of roughly 110,000 jobs. This employment decline however, occurred largely as a result of firms decreasing hiring rates rather than increasing separation rates. As a result, the impact of the NBP on incumbent workers was mitigated as there is no evidence of increased separations or increased periods of non-employment for separating workers. While there was a decline in earnings, this decline was isolated to workers hired after the regulation began. Overall, these findings suggest that firms primarily reduced employment through voluntary separations, thus mitigating the impact on incumbent workers. The labor market impacts of the NBP were felt primarily by future workers who earn less and the potential future workers who would have been hired in the absence of the regulation.

Looking forward, future research should perform ex-post evaluations of environmental regulations to better understand their impacts on different demographic groups. A more detailed understanding of the relationship between regulation, energy prices and employment would help guide policy makers and allow them to target workers that are particular vulnerable. Furthermore, data distinguishing between voluntary and involuntary separations would allow for a better understanding of the types of worker flows that occur following regulation.

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Figures and Tables

Figure 1: NBP Compliance Region

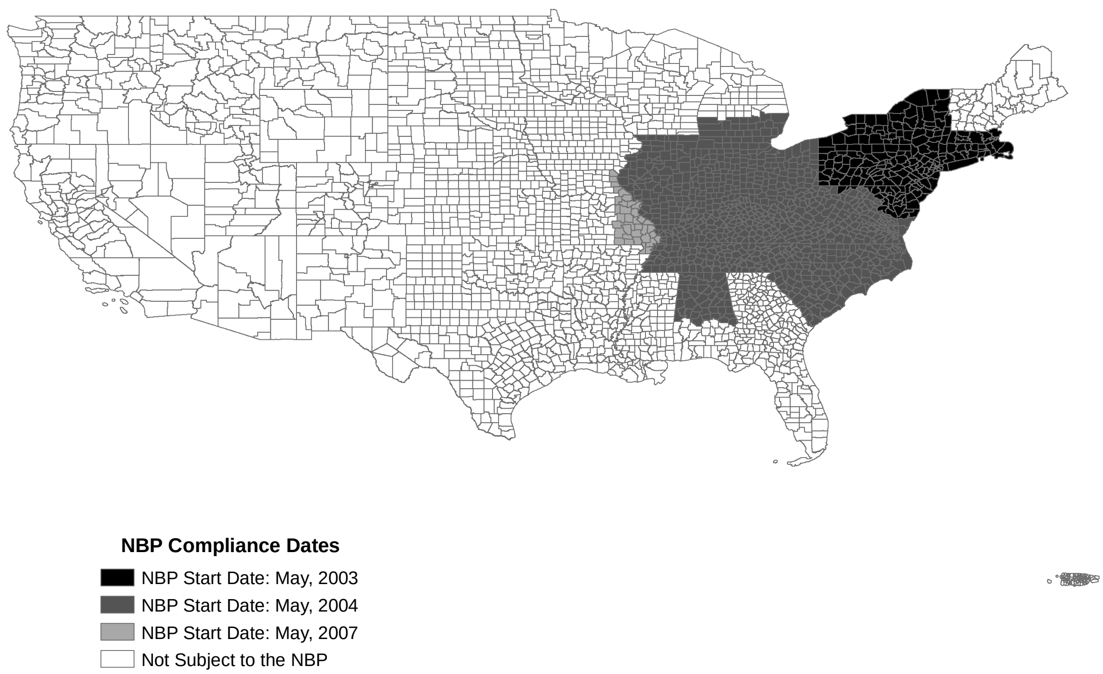
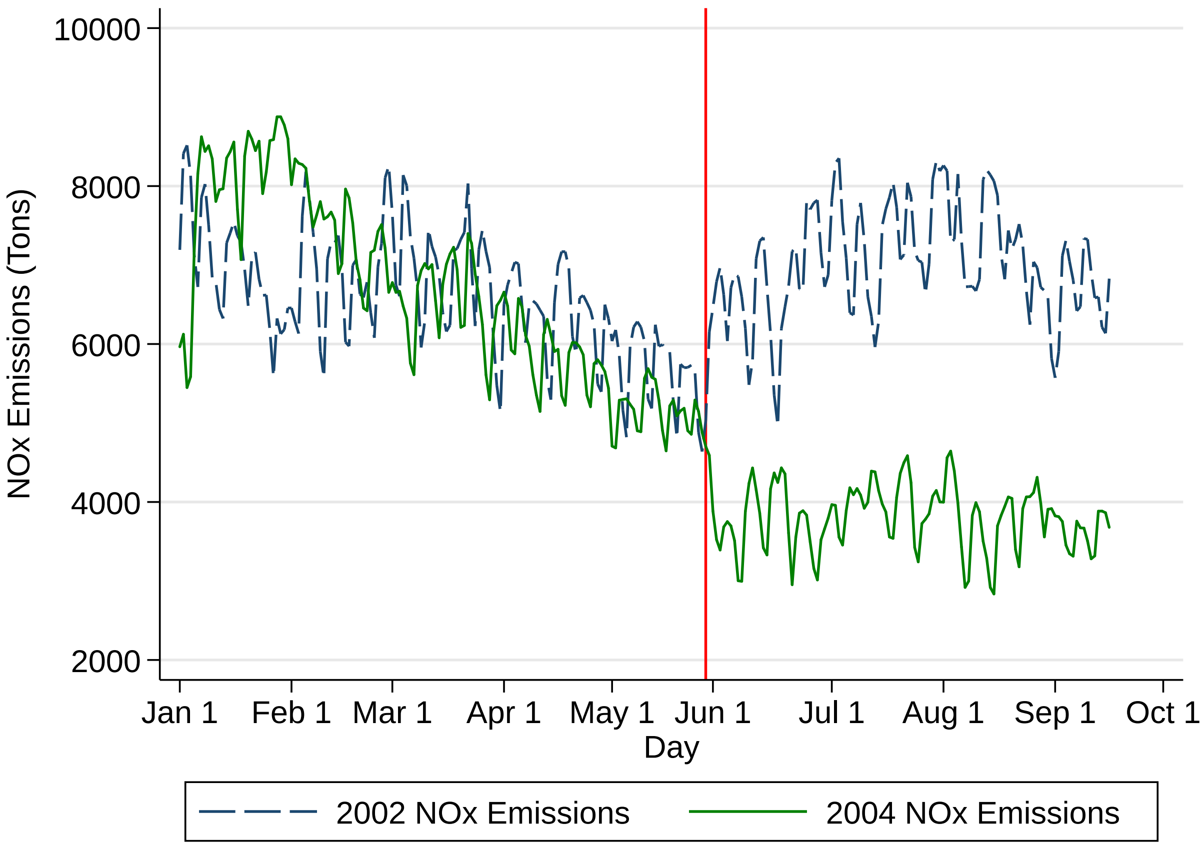
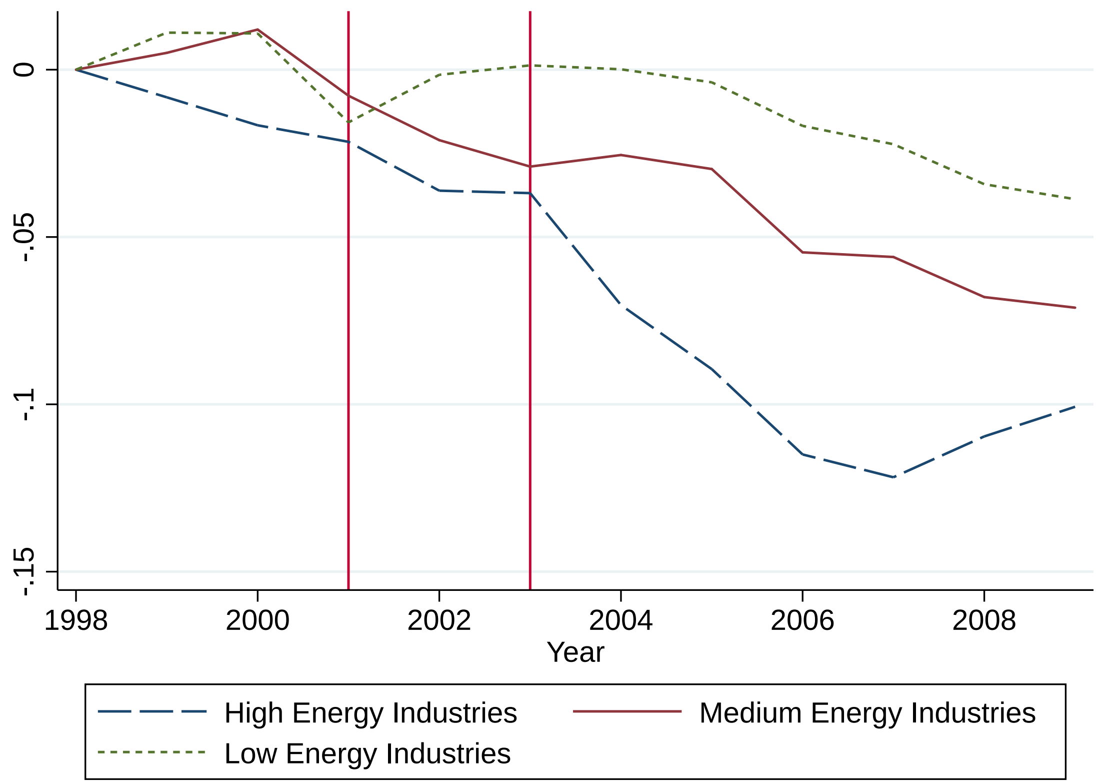


Figure 2: NO_x Emissions From all NBP Affected States



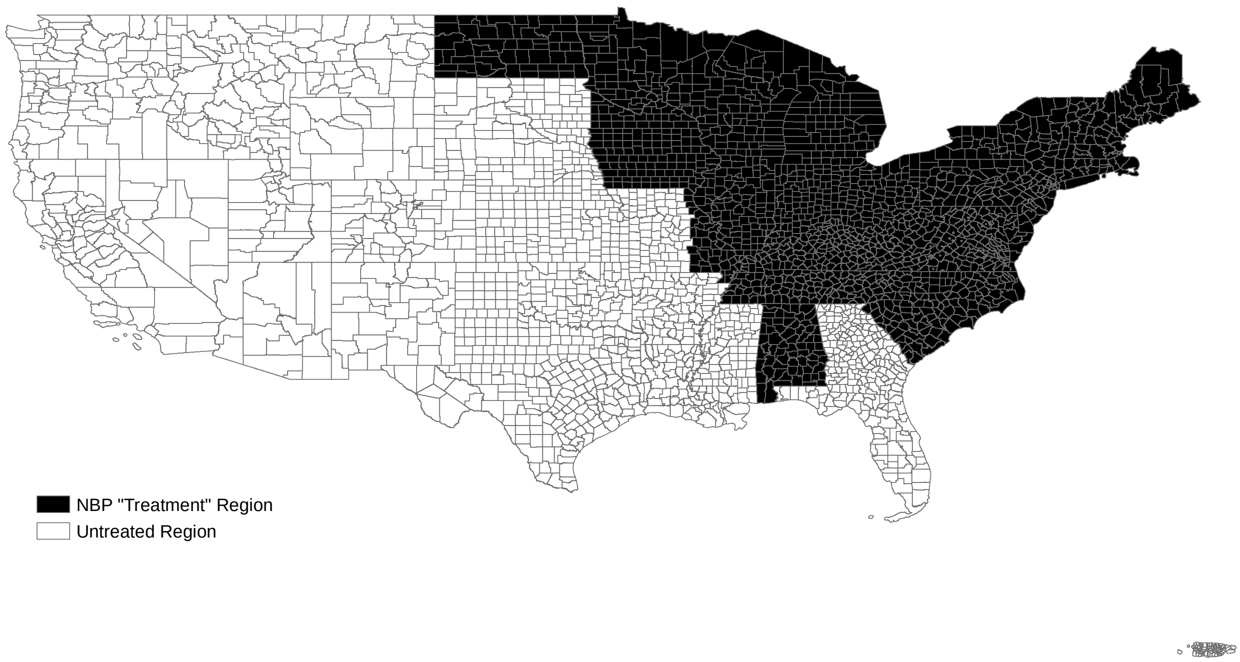
While some northeastern states began in 2003, the program started in full on May 30, 2004. This graph plots daily NO_x emissions in 2002, when no states were participating, and 2004 for the nineteen participating states. There is a visible reduction in the amount of NO_x emissions in NBP states beginning on the start date that is not present in 2002.

Figure 3: East-West Difference in Employment by Energy-Intensity Grouping



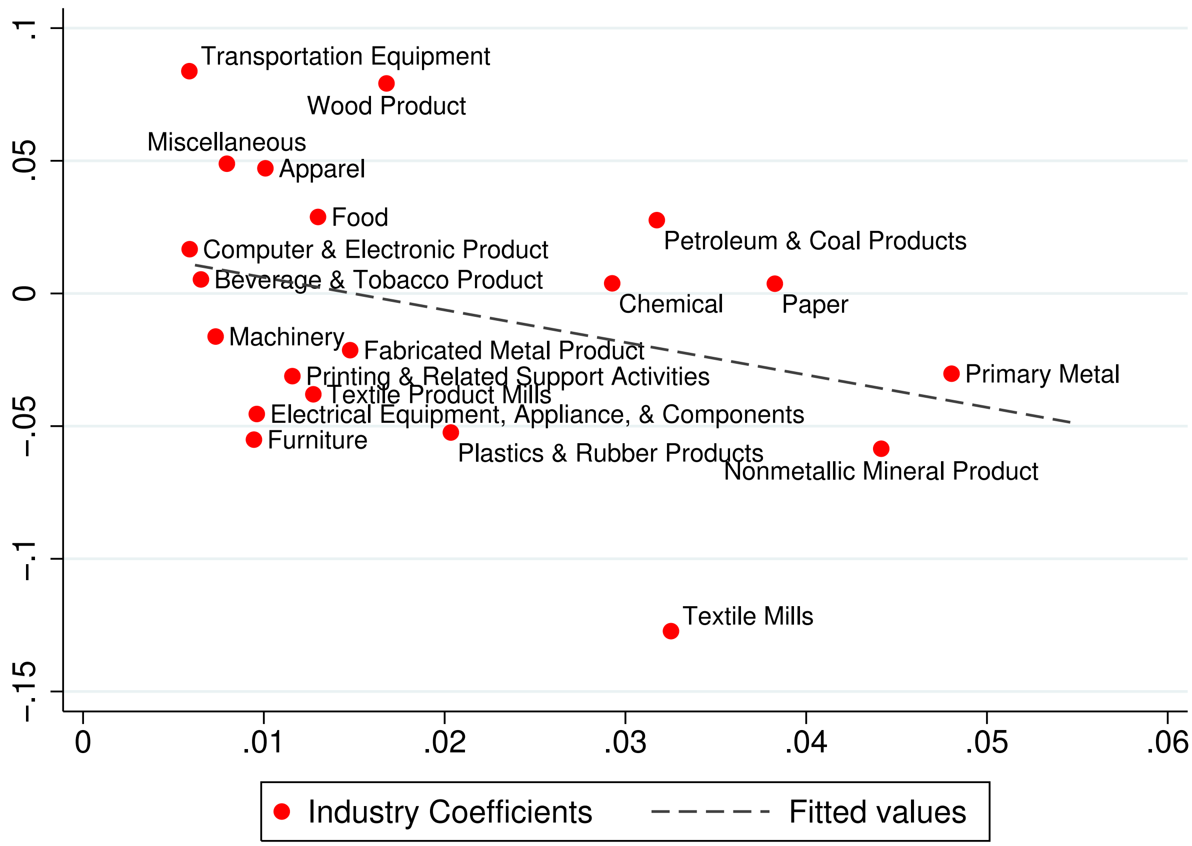
Note: Each point is calculated by $\frac{emp_{g,t,east}}{emp_{g,98,east}} - \frac{emp_{g,t,west}}{emp_{g,98,west}}$. This shows the percent employment change in the east minus the percent employment change in the west. Here t is the year (1998-2009), g is energy intensity group (low, medium, high) and $emp_{g,t,east}$ is the total employment for industry grouping g in east (treated) region in year t. All east-west differences assume the 1998 difference to be the baseline difference, set to zero, against which future differences can be compared. The vertical lines are drawn at 2001, after the policy was approved and 2003, the year the NBP went into effect.

Figure 4: NBP Treated Region



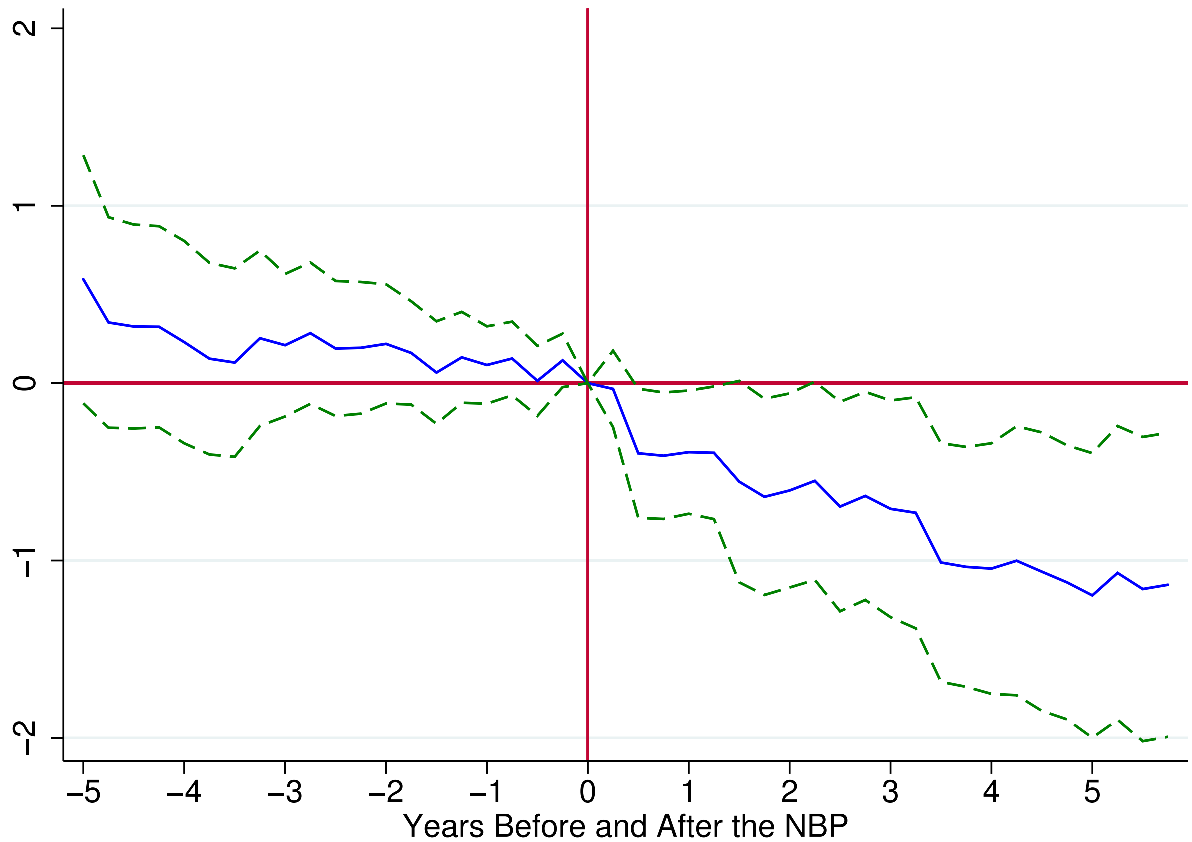
Note: An area is defined as treated if its electricity provider is part of an ISO with coal-burning power plants that were subject to the NBP. For example, Iowa, Minnesota, Wisconsin and North Dakota, while not part of the geographic area of the NBP are part of the Midwest Independent System Operator whose geographic region includes Indiana, Illinois and Michigan. Boundaries of ISO's are notoriously fuzzy as they are defined according to utilities rather than a set geographic region. Furthermore the impact of the NBP will be dissipated for regions whose manufacturing establishments are not directly regulated. Because there is no clean geographic discontinuity to the policy, this makes border discontinuity methods an unattractive methodology for identifying the impact of the NBP.

Figure 5: Industry Coefficients Vs. Energy Intensity



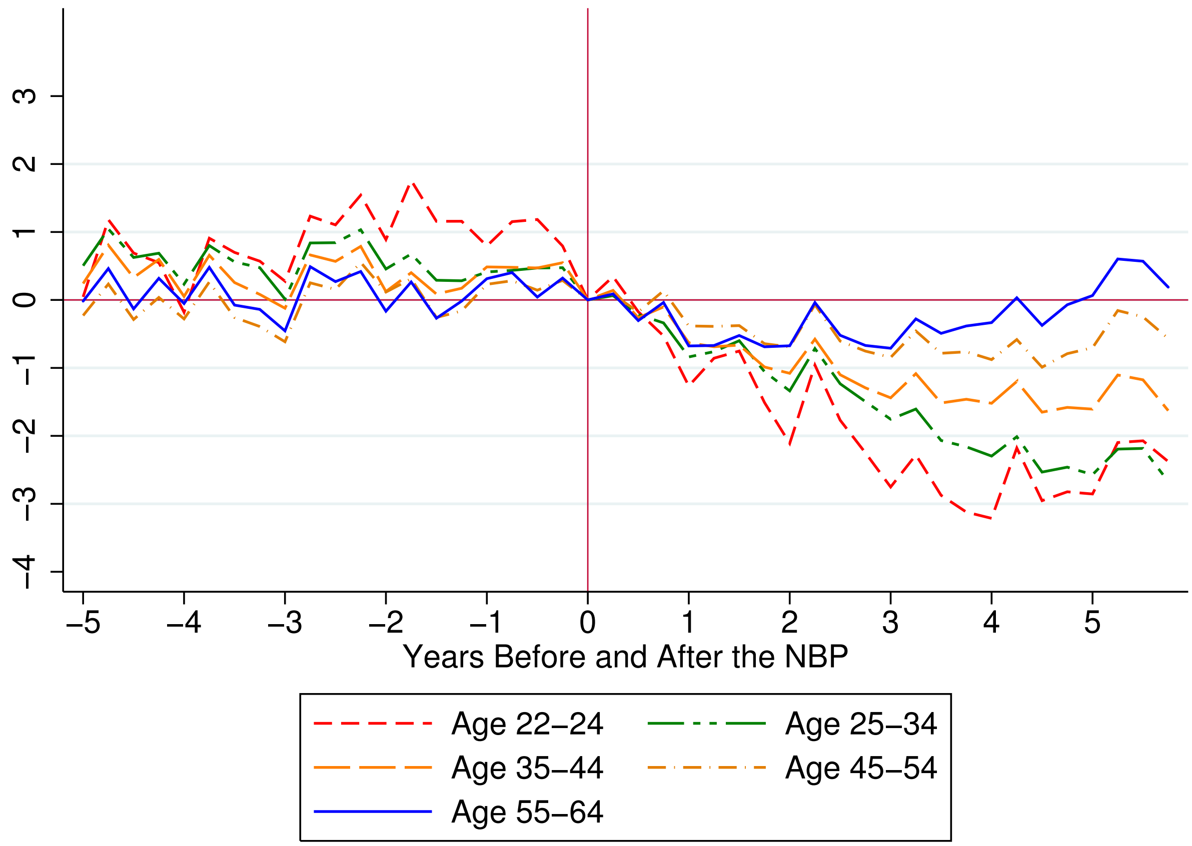
Note: This chart plots the each industry specific triple difference coefficient against that industry's energy intensity measure. The coefficients are obtained by replacing the $PostxEastxEnInt$ variable with 21 industry specific triple interaction variables ($PostxEastxInd1$, $PostxEastxInd2, \ldots, PostxEastxInd21$) and then running the specification described in equation (2). Other model specifications yield similar plots.

Figure 6: Impact of NBP on Employment by Quarter



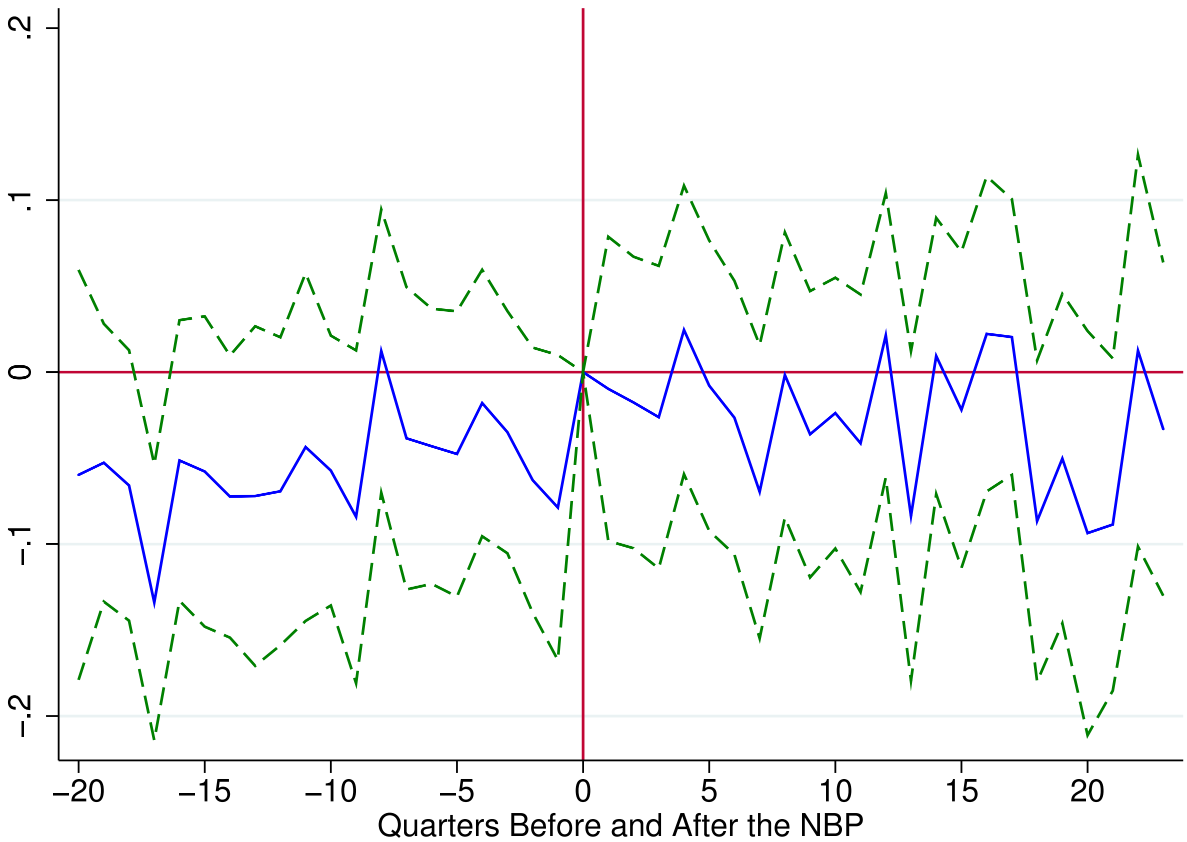
Note: This chart plots the coefficient estimates from a version of equation 3 where the outcome variable is logged employment. Specifically, it plots the coefficients on the event time indicator variables which demonstrate how the impact of the policy on employment evolved over time. The dashed lines represent the 95% confidence intervals. Before the policy the coefficients are slightly above zero but statistically insignificant. After the policy the coefficients fall below zero and their confidence intervals do not include zero.

Figure 7: Impact of NBP on Employment by Quarter by Age Group



Note: This chart plots event-time coefficient estimates from five different regressions. The specific model is shown in equation 3. The five different lines plot the differential dynamic impact of the policy on employment in five different age categories.

Figure 8: Impact on Periods of Non-employment for Separating Workers by Quarter



Note: This chart plots the coefficient estimates from a version of equation 3 where the outcome variable is periods of non-employment for separating workers. Specifically, it plots the coefficients on the event time indicator variables which demonstrate the impact of the policy on periods of non-employment for separating workers evolved over time. The dashed lines represent the 95% confidence intervals. The coefficients are always close to zero and are never statistically distinguishable from zero.

Table 1: Energy Intensity of 3-Digit NAICS Manufacturing Industries

NAICS 3-Digit Code	Industry Description	Energy Intensity
336	Transportation Equipment Manufacturing	0.59%
334	Computer & Electronic Product Manufacturing	0.59%
312	Beverage & Tobacco Product Manufacturing	0.65%
333	Machinery Manufacturing	0.73%
339	Miscellaneous Manufacturing	0.79%
316	Leather & Allied Product Manufacturing	0.87%
337	Furniture & Related Product Manufacturing	0.94%
335	Electrical Equipment, Appliance, & Component Manufacturing	0.96%
315	Apparel Manufacturing	1.01%
323	Printing & Related Support Activities	1.16%
314	Textile Product Mills	1.27%
311	Food Manufacturing	1.30%
332	Fabricated Metal Product Manufacturing	1.48%
321	Wood Product Manufacturing	1.68%
326	Plastics & Rubber Products Manufacturing	2.03%
325	Chemical Manufacturing	2.93%
324	Petroleum & Coal Products Manufacturing	3.17%
313	Textile Mills	3.25%
322	Paper Manufacturing	3.83%
327	Nonmetallic Mineral Product Manufacturing	4.42%
331	Primary Metal Manufacturing	4.80%

Note: The energy intensity measure is created by dividing the industry's total energy expenditure by their total value of shipments. These variables are obtained from the NBER Productivity Database and use 1998 values.

Table 2: Employment Results: County Business Patterns

	(1) ln(Emp)	(2) ln(Emp)	(3) ln(Emp)	(4) ln(Emp)	(5) ln(Emp)	(6) ln(Emp)
PostxNBPxEnInt	-3.908	-2.209**	-2.235**	-1.383	-1.329**	-1.226*
	(3.085)	(0.891)	(0.983)	(0.829)	(0.597)	(0.662)
PostxNBP	-0.00183	0.0134		0.00959	0.00294	
	(0.0828)	(0.0166)		(0.0173)	(0.0155)	
State-Ind FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes			Yes		
State Linear Trend		Yes			Yes	
Ind-Year FE		Yes	Yes		Yes	Yes
State-Year FE			Yes			Yes
Ind-Region Trends				Yes	Yes	Yes
Observations	12,348	12,348	12,348	12,348	12,348	12,348
R-Squared	0.982	0.993	0.993	0.992	0.993	0.993

Standard errors in parentheses

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

Note: This table reports the main employment results using versions of equations (1) and (2). Robust standard errors are reported in parentheses and are clustered at the NBP Region-industry level. Inference is not sensitive to clustering at other levels including, but not limited to, state, industry and state-industry. Column 1 gives the results using equation (1). Column 2 includes industry-year and state-year fixed effects and Column 3 includes state linear trends and industry-year fixed effects. Columns 4 through 6 repeat the specificatios in Columns 1 through 3 but now each industry is allowed to trend differently based on it's location. For example, steel industries in the NBP region have a separate trend than steel industries in the non-NBP region. The coefficient on the $PostxNBP$ variable drops whenever State-Year fixed effects are included.

Table 3: Employment Results: State Robustness Checks

	(1) ln(Emp)	(2) ln(Emp)	(3) ln(Emp)	(4) ln(Emp)	(5) ln(Emp)	(6) ln(Emp)
Panel A: Fuel Price / Composition Controls						
PostxNBPxEnInt	-3.783	-2.322**	-2.391**	-1.055	-1.346**	-1.303**
	(3.150)	(0.908)	(1.104)	(0.852)	(0.569)	(0.639)
PostxNBP	-0.00315	0.0139		0.00391	0.00215	
	(0.0829)	(0.0166)		(0.0173)	(0.0154)	
Observations	12,348	12,348	12,348	12,348	12,348	12,348
Panel B: Restricted NBP Region						
PostxNBPxEnInt	-3.974	-2.172***	-2.150**	-1.610*	-1.499***	-1.238**
	(3.418)	(0.800)	(0.865)	(0.883)	(0.520)	(0.602)
PostxNBP	-0.0151	0.0115		0.0104	0.00250	
	(0.0931)	(0.0135)		(0.0194)	(0.0131)	
Observations	10,080	10,080	10,080	10,080	10,080	10,080
State-Ind FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes			Yes		
State Linear Trend		Yes			Yes	
Ind-Year FE		Yes	Yes		Yes	Yes
State-Year FE			Yes			Yes
Ind-Region Trends				Yes	Yes	Yes

Standard errors in parentheses

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

Note: These results use the same specifications as those reported in table 2. Panel A controls for changes in fuel prices that may disproportionately affect certain regions. Panel B limits the NBP region to only states which are directly regulate. States which were part of the NBP region but are not directly regulated are dropped from the specification. See the text for additional details.

Table 4: Employment Results: County Robustness Checks

	(1) ln(Emp)	(2) ln(Emp)	(3) ln(Emp)	(4) ln(Emp)	(5) ln(Emp)	(6) ln(Emp)
Panel A: Base Regressions						
PostxNBPxEnInt	-3.223	-1.456	-1.510	-1.546*	-1.250**	-1.465
	(2.847)	(0.932)	(0.898)	(0.896)	(0.597)	(0.889)
PostxNBP	0.0156	-0.0003		0.0108	-0.0033	
	(0.0803)	(0.0159)		(0.0161)	(0.0153)	
Observations	374,356	374,356	374,356	374,356	374,356	374,356
Panel B: Restricted NBP Region						
PostxNBPxEnInt	-3.270	-1.318	-1.416	-1.689*	-1.211*	-1.550
	(2.997)	(0.913)	(0.897)	(0.955)	(0.655)	(0.943)
PostxNBP	-0.0293	-0.0084		0.00980	-0.0102	
	(0.0852)	(0.0156)		(0.0168)	(0.0161)	
Observations	320,323	320,323	320,323	320,323	320,323	320,323
Panel C: NAAQS Controls						
PostxNBPxEnInt	-3.326	-1.523	-1.573*	-1.540*	-1.345**	-1.651*
	(2.825)	(0.935)	(0.914)	(0.915)	(0.593)	(0.912)
PostxNBP	-0.0161	0.0083		0.0096	-0.0025	
	(0.0806)	(0.0162)		(0.0157)	(0.0152)	
Observations	374,356	374,356	374,356	374,356	374,356	374,356
County-Ind FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes			Yes		
State Linear Trend		Yes			Yes	
Ind-Year FE		Yes	Yes		Yes	Yes
State-Year FE			Yes			Yes
Ind-Region Trends				Yes	Yes	Yes

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ Note: See text for additional details.

Table 5: QWI Descriptive Statistics

	NBP Region			Non-NBP Region		
Variable	Low	Mid	High	Low	Mid	High
Emp	22819	15315	15463	22229	11751	9076
	(27310)	(20744)	(18277)	(46884)	(22706)	(15403)
Hire Rate	0.061	0.073	0.058	0.082	0.093	0.078
	(0.038)	(0.047)	(0.036)	(0.052)	(0.049)	(0.054)
Sep Rate	0.066	0.086	0.066	0.084	0.099	0.081
	(0.056)	(0.136)	(0.055)	(0.101)	(0.075)	(0.068)
Avg Monthly Earn	4050	3136	3912	3750	2919	3583
	(1172)	(1283)	(1177)	(1367)	(1138)	(1132)
New Hires Avg	3012	2238	2960	2715	2094	2545
Monthly Earnings	(1053)	(834)	(1073)	(1027)	(928)	(923)
Avg Qtrs of Non-Emp	1.815	1.799	1.844	1.734	1.766	1.780
for Seperators	(0.363)	(0.366)	(0.382)	(0.324)	(0.373)	(0.398)

Note: Observations in the QWI data are at the State – 3-digit NAICS level. Sample means report averages of the observations in low, medium and high energy intensity groupings in both the NBP and non-NBP regions. Standard deviations are reported in parentheses below the means. Hiring and separation rates are quarterly and the average monthly earnings variables are in nominal dollars. Sample period is from 1998Q1 to 2009Q4. See Section 3.2 and the data appendix for more information on the QWI.

Table 6: Employment, Flows and Earnings by Age Group

	(1) ln(Emp)	(2) Hires Rate	(3) Seps Rate	(4) Seps: Qtrs of Non-Emp	(5) ln(New Hire Earnings)	(6) ln(Avg Earnings)
All	-0.871**	-0.225**	-0.187	-0.516	-1.277***	0.435
	(0.411)	(0.0999)	(0.128)	(0.636)	(0.470)	(0.299)
Age 19-21	-1.939**	-0.0559	-0.477**	0.719	-0.689*	-0.387
	(0.722)	(0.254)	(0.179)	(0.585)	(0.352)	(0.274)
Age 22-24	-2.121***	-0.288*	-0.358**	0.688	-0.737*	-0.417
	(0.647)	(0.171)	(0.163)	(0.738)	(0.400)	(0.287)
Age 25-34	-0.860	-0.144	-0.154	0.0354	-0.982**	0.141
	(0.588)	(0.122)	(0.126)	(0.699)	(0.436)	(0.217)
Age 35-44	-0.861**	-0.222**	-0.149	-0.170	-1.214**	0.370
	(0.398)	(0.0868)	(0.142)	(0.723)	(0.482)	(0.332)
Age 45-54	-0.718	-0.156	-0.0986	-0.724	-0.887	0.434
	(0.521)	(0.117)	(0.136)	(1.216)	(0.550)	(0.327)
Age 55-64	-0.304	-0.113	-0.214	-0.984	-1.877**	0.303
	(0.387)	(0.105)	(0.138)	(1.370)	(0.723)	(0.328)
Age 65-99	-0.748*	-0.0391	-0.0635	-1.829	-1.664	0.361
	(0.400)	(0.0927)	(0.0888)	(1.111)	(1.267)	(0.484)

Standard errors in parentheses

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

Note: Each cell reports the regression coefficient on the triple interaction variable using a different outcome variable - age group combination from the model in equation (2). The columns are the outcome variables and the rows are the age group. The hiring / separation employment identity need not hold for the age specific results as employment declines in these groupings also occur as workers age out of the group.

A Data Appendix

County Business Patterns does not report observations for which there is no employment in a given year. If firms shutdown and employment in a state-industry goes from 1,000 one year to zero the following year then this drop will not be observed because there will be no record for the zero employment (the same issue could occur in reverse. whereby employment growth that is attributable to new establishments locating in a state-industry with zero previous employment will not be captured because the zero employment wasa not observed in the prior years. To address this concern, I create a balanced panel of every state-industry between 1998 and 2008 by adding zeros when there is no record listed in the CBP. Data that is suppressed for disclosure purposes is imputed by the method used by Kahn & Mansur (2013) as described in the text. Other methods of imputation were explored, but results were not sensitive to the use of other methods.

Data suppression at the county-industry level is far more common due to disclosure concerns. Employment is directly observed for 51% of county-industry pairings. These observed cells contain 72% of all employment in the U.S. A large percent of the overall employment remains because employment is only suppressed for observations with few establishments. Also, some noise is infused in observations for which there are few establishments, but these noise infusions are always less than 5%, sum to zero at the state level and are made in fewer than 5% of the observations.

The QWI is the seond source of data in this paper. It is built from state Unemployment Insurance records and contains 98% of all private-sector, non-agriculture employment at high levels of demographic, geographic and industry detail. Importantly, it contains data on job and worker flows. A job is a relationship between a worker and an establishment where the worker receives positive earnings from that establishment in a quarter. Unlike the CBP, there is no way to impute suppressed data.

Quarterly Workforce Indicator data is also suppressed at times though this is likely to bias against finding a result. If small drops in employment lead to an observation becoming suppressed, then it is possible that employment declines resulting from the NBP will not be observed if the observation becomes suppressed. This would bias against finding a result. The ten states not included in the QWI results are Alabama, Arkansas, Arizona, Kentucky, Massachusetts, Michigan, Mississippi, Missouri, New Hampshire and Wyoming.

Hires and Separations variables refer to stable jobs. A job is considered stable if the worker receives positive earnings from the establishment for three consecutive quarters. Periods of non-employment for separating workers is obtained by tracking each worker that separates from their firm for the following four quarters. If they are not observed working at any other employer then they are assigned four quarters of non-employment. If, in the quarter following their separation they are observed working at another firm, then they are assigned zero quarters of non-employment. Currently, the QWI is only able to track workers if they find employment in the same state in which they separated. Using state-industry fixed effects accounts for, among other things, time-invariant differences that may arise in this measure due to a state's size. Plotting this variable over time reveals differences by state that are consistent over time.

EPA's website provides a list of regulated plants in the NBP. Based on the author's calculation, 93% of regulated manufacturing facilities are in the high intensity industry grouping as defined in section 4.

Table A1: Employment Results: Dropping 2009 Data

	(1) ln(Emp)	(2) ln(Emp)	(3) ln(Emp)	(4) ln(Emp)	(5) ln(Emp)	(6) ln(Emp)
PostxNBPxEnInt	-3.861	-2.301**	-2.293**	-1.458*	-1.385**	-1.164
	(2.991)	(0.966)	(0.869)	(0.798)	(0.631)	(0.729)
PostxNBP	0.0024	0.0155		0.0113	0.0043	
	(0.0799)	(0.0166)		(0.0151)	(0.0164)	
State-Ind FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes			Yes		
State Linear Trend		Yes			Yes	
Ind-Year FE		Yes	Yes		Yes	Yes
State-Year FE			Yes			Yes
Ind-Region Trends				Yes	Yes	Yes
Observations	11,319	11,319	11,319	11,319	11,319	11,319
R-Squared	0.985	0.994	0.994	0.993	0.994	0.994

Standard errors in parentheses

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

Note: This table reports results for the same set of regression models found in table 2 but having dropped the year 2009 from the data. As with table 2, the results are from versions of equations (1) and (2). Robust standard errors are reported in parentheses and are clustered at the NBP Region-industry level.

Table A2: QWI Age Group Summary Statistics

Age	Emp	Hire Rate	Sep Rate	Avg Monthly Earn	New Hire Avg Monthly Earn	Qtrs Non-Emp for Seperators
All	15503	0.076	0.083	3413	2479	1.789
	(26094)	(0.048)	(0.096)	(1287)	(1014)	(0.370)
Age 19-21	456	0.233	0.183	1672	1569	1.620
	(734)	(0.094)	(0.130)	(431)	(458)	(0.509)
Age 22-24	675	0.156	0.140	2191	1964	1.609
	(1071)	(0.072)	(0.103)	(592)	(637)	(0.527)
Age 25-34	3234	0.095	0.099	2933	2415	1.632
	(5678)	(0.053)	(0.096)	(898)	(873)	(0.449)
Age 35-44	4450	0.066	0.074	3561	2851	1.668
	(7901)	(0.047)	(0.098)	(1260)	(1295)	(0.469)
Age 45-54	4113	0.052	0.063	3846	3031	1.801
	(7014)	(0.048)	(0.120)	(1580)	(1560)	(0.515)
Age 55-64	2040	0.043	0.070	3867	2988	2.348
	(3400)	(0.048)	(0.110)	(4417)	(2326)	(0.608)
Age 65-99	396	0.058	0.110	3081	1929	2.903
	(681)	(0.058)	(0.100)	(2346)	(3868)	(0.647)

Note: Observations in the QWI data are at the State – 3-digit NAICS level. Sample means report averages for the different age-groupings provided by the QWI. Standard deviations are reported in parentheses below the means. Hiring and separation rates are quarterly and the average monthly earnings variables are in nominal dollars. Sample period is from 1998Q1 to 2009Q4. See Section 3.2 and the data appendix for more information on the QWI.

Table A3: Employment Results: Dropping California

	(1)	(2)	(3)	(4)	(5)	(6)
	ln(Emp)	ln(Emp)	ln(Emp)	ln(Emp)	ln(Emp)	ln(Emp)
PostxNBPxEnInt	-3.396	-2.202**	-2.230*	-1.722*	-1.687**	-1.627*
	(2.997)	(1.037)	(1.161)	(0.863)	(0.697)	(0.830)
PostxNBP	-0.0169	0.00940		0.00858	0.00282	
	(0.0822)	(0.0208)		(0.0172)	(0.0182)	
State-Ind FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes			Yes		
State Linear Trend		Yes			Yes	
Ind-Year FE		Yes	Yes		Yes	Yes
State-Year FE			Yes			Yes
Ind-Region Trends				Yes	Yes	Yes
Observations	12,096	12,096	12,096	12,096	12,096	12,096
R-squared	0.979	0.992	0.993	0.991	0.993	0.993

Standard errors in parentheses

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

Note: This table reports results for the same set of regression models found in table 2 but excludes California from the data from the data. As with table 2, the results are from versions of equations (1) and (2). Robust standard errors are reported in parentheses and are clustered at the NBP Region-industry level.

Table A4: Diff-in-Diff Results

	(1) ln(Emp)	(2) ln(Emp)	(3) ln(Emp)	(4) ln(Emp)	(5) ln(Emp)
PostxEnInt	-0.703	-1.036**			
	(0.452)	(0.410)			
PostxNBP			-0.0215	-0.0220*	
			(0.0140)	(0.0129)	
PostxNBPxEnInt					-1.226*
					(0.662)
Observations	6,552	6,552	3,528	3,528	12,348
R-squared	0.991	0.992	0.989	0.992	0.993
State-Ind FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes		Yes	Yes	
Ind Linear Trend	Yes	Yes	Yes	Yes	
Ind-Year FE				Yes	Yes
State-Year FE		Yes			Yes

Standard errors in parentheses

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

Note: The first two columns of this table report differences-in-differences coefficients using a simpified regression model which drops all non-NBP region data and identifies the impact of the program based off of only the industry variation. Columns 3 and 4 use data from both the NBP and non-NBP region but keep only industries in the top quartile of the energy intensity index. Columns 3 and 4 are therefore identified off of only the geographic variation of the program. Column 5, listed simply for comparison purposes, presents the triple-diff result from the fully specified model. Note that the interpretation of the coefficients in columns 3 and 4 is different than the interpretation of columns 1, 2 and 5.

Table A5: Electricity Price Regressions

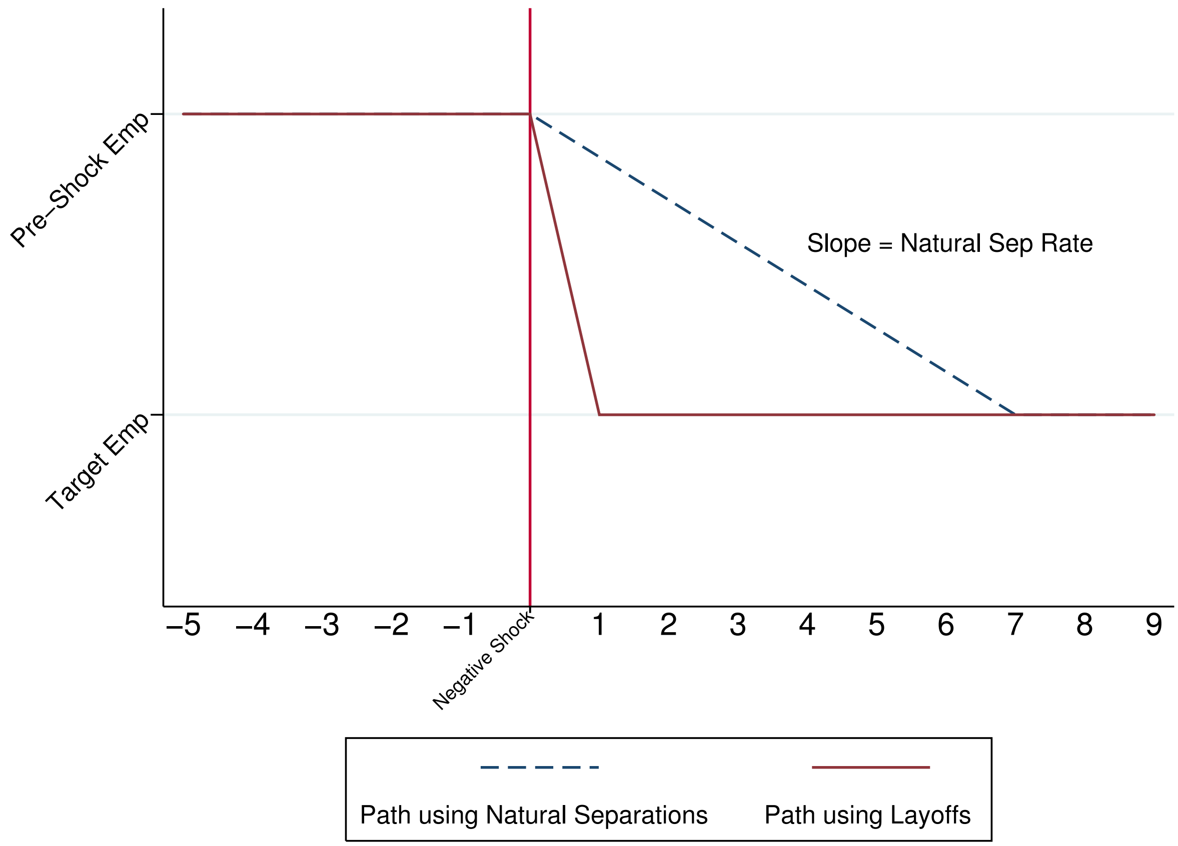
	(1)	(2)	(3)
	ln(Elec Price)	ln(Elec Price)	ln(Elec Price)
PostxNBP	0.0456	0.0587*	0.0350
	(0.0321)	(0.0322)	(0.0319)
PerCoalxCoalPrice	0.0228***	0.0121	0.0122
	(0.0054)	(0.0122)	(0.0121)
PerOilxOilPrice	0.0091***	0.0088***	0.0089***
	(0.0008)	(0.0010)	(0.0009)
PerNatGasxNatGasPrice	0.0663***	0.0811***	0.0819***
	(0.0126)	(0.0231)	(0.0229)
State FE	Yes	Yes	Yes
Year FE		Yes	Yes
E / W Trends			Yes
Observations	539	539	539
R-Squared	0.918	0.924	0.925

Standard errors in parentheses

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

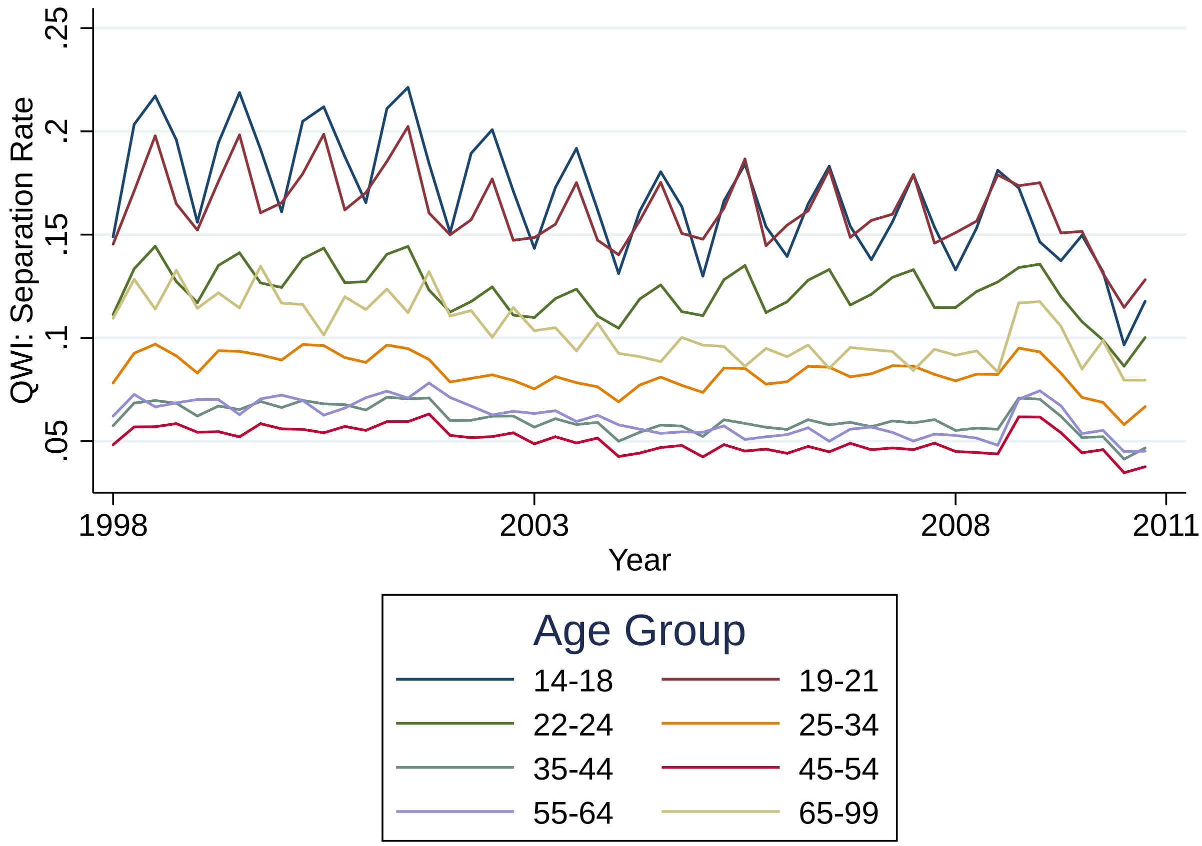
Note: See text for details on fuel prices. Standard errors are robust and clustered at the state level. The dependent variable is log of average industrial electricity prices in a state-year. controlling for the price of fuels used in electricity production is important if certain regions in the country have a relatively high reliance on certain fuels to produce electricity. Using the same technique as in the robustness check, I interact the average annual fuel price with the percent of electricity that is derived from that fuel in the NERC region and include these terms in the regression. This allows for the electricity price in regions with high reliance on certain fuels to vary with the price of those fuels. Column 2 contains the results with a full set of state and year fixed effects. The coefficient on the PostxNBP variable indicates that the NBP increased industrial electricity prices by approximately 5.8% in states impacted by the NBP. Column 3 includes separate NBP and Non-NBP region trends and while the coefficient is less precisely estimated it remains positive and of an economically significant magnitude.

Figure A1: Employment Adjustment Paths: Natural Separations vs. Layoffs



Note: This chart plots two employment adjustment paths a firm may take to arrive at a new employment level following a permanent negative shock. The dashed line represents the path taken when the firm adjusts through natural separations (quits and retirements) and the solid line represents the path when the firm adjusts immediately through layoffs.

Figure A2: Separation Rate by Age Group



Note: The above figure plots out the separation rate for workers in each age group found in the QWI. Younger workers are shown to have separation rates far above older workers.