THE TRANSITIONAL COSTS OF SECTORAL REALLOCATION: EVIDENCE FROM THE CLEAN AIR ACT AND THE WORKFORCE*

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This article uses linked worker-firm data in the United States to estimate the transitional costs associated with reallocating workers from newly regulated industries to other sectors of the economy in the context of new environmental regulations. The focus on workers rather than industries as the unit of analysis allows me to examine previously unobserved economic outcomes such as nonemployment and long-run earnings losses from job transitions, both of which are critical to understanding the reallocative costs associated with these policies. Using plant-level panel variation induced by the 1990 Clean Air Act Amendments (CAAA), I find that the reallocative costs of environmental policy are significant. Workers in newly regulated plants experienced, in aggregate, more than $5.4 billion in forgone earnings for the years after the change in policy. Most of these costs are driven by nonemployment and lower earnings in future employment, highlighting the importance of longitudinal data for characterizing the costs and consequences of labor market adjustment. Relative to the estimated benefits of the 1990 CAAA, these one-time transitional costs are small. *JEL* Codes: Q50, H41, R11.

I. INTRODUCTION

Environmental policy pertaining to air pollution has been estimated to have large health benefits (Chay and Greenstone 2003b; Currie and Neidell 2005; Schlenker and Walker 2011).

*I thank Janet Currie, Bernard Salanié, Wolfram Schlenker, and Till von Wachter for invaluable advice and discussions. I also thank David Autor, Alex Chinco, Lucas Davis, Olivier Deschenes, Walker Hanlon, Michael Greenstone, Jeff Grogger, Sue Helper, Solomon Hsiang, Wojciech Kopczuk, Matt Kotchen, Todd Kumler, Erin Mansur, Ben Marx, Matt Notowidigdo, Johannes Schmieder, Eric Verhoogen, Jonathan Vogel, and seminar participants at the Census Bureau, the NBER Environmental and Energy Economics meetings, Arizona, Arizona State, Berkeley-Haas, Case Western, Chicago-Harris, Columbia, Cornell, George Washington, Harvard-Kennedy, MIT, Oregon, Stanford-SIEPR, UC-Irvine, UC-Santa Barbara, UBC, Wharton, and Yale-FES for useful comments. Any opinions and conclusions expressed herein are those of the author and do not necessarily represent the views of the U.S. Census Bureau. All results have been reviewed to ensure that no confidential information is disclosed. This research uses data from the Census Bureau's Longitudinal Employer Household Dynamics Program, which was partially supported by the following NSF Grants SES-9978093, SES-0339191 and ITR-0427889; NIA Grant AG018854; and grants from the Alfred P. Sloan Foundation. Support for this research from the EPA Grant 834259010 is gratefully acknowledged.

The Quarterly Journal of Economics (2013), 1787–1835. doi:10.1093/qje/qjt022.
Advance Access publication on August 14, 2013.

However, these policies also come with costs. Production is typically reallocated away from newly regulated industries to other sectors and locations (Henderson 1996; Greenstone 2002; Walker 2011), and this creates a broad set of private and social costs. In terms of labor inputs, this reallocation is often framed in terms of "jobs lost," and the distinction between "jobs versus the environment" is one of the more politically salient aspects of these regulations.[1] However, workers often find new jobs elsewhere, perhaps in different locations and/or industries. If workers simply transition from one employer to the next without significant earnings loss, then job loss should not be considered a cost when evaluating policy. If workers lose job- or industry-specific skills and/or experience long periods of unemployment following job transitions, the cost of reallocating the workforce could be quite large. There also may be costs to workers who remain in these potentially less productive industries.

This article uses newly available longitudinal data on workers and firms to estimate the incidence of regulation-induced worker reallocation stemming from the 1990 Clean Air Act Amendments (CAAA). In doing so, this article offers an approach to characterizing the costs and consequences of external labor market innovations when production and workers are not instantly reallocated elsewhere within the economy. Using the confidential Longitudinal Employer Household Dynamics (LEHD) data set from the U.S. Census Bureau, I am able to follow workers across their jobs over time to explicitly incorporate two substantive features of labor market adjustment that are typically studied in isolation: the wage costs borne by workers who remain in the newly regulated, now less productive sector and the long-run earnings losses for those who leave the sector.

The 1990 CAAA created a new class of pollution standards and strengthened existing standards so that many areas of the United States fell under a new regulatory regime. Polluting firms in these areas were forced to reduce emissions and install new pollution-abatement technologies, increasing the cost of production and lowering productivity (Greenstone, List, and Syverson 2012).

1. For representative examples from the popular press see the *Wall Street Journal* (July 26, 2011) op-ed titled "The Latest Job Killer from the EPA", "Smoke Signals" from the *New Republic* (April 7, 2011), or "A Debate Arises on Job Creation and Environment" from the *New York Times* (September 4, 2011). Recent interest from the broader policy and law community may be found in work by Livermore, Piennar, and Schwartz (2012) and Masur and Posner (2012).

These regulations led to a sectoral shift in production and employment away from newly regulated, polluting sectors (Walker 2011). The empirical framework estimates the effect of this regulatory shock on the evolution of earnings for the workers in newly regulated plants.

Although the empirical setting pertains to environmental regulations, the analysis relates to a large literature on the costs and incidence of labor market adjustment to external factors, such as trade, immigration, or innovations in labor demand.[2] Traditionally, work in this area examines how prices in industries or regional labor markets respond to external labor market shocks, and then the estimates are used to back out a measure of welfare or incidence. In contrast, this study is able to observe and estimate both worker-specific nonemployment durations and any long-run earnings changes associated with the reallocation of production and workers. In doing so, this article provides an empirical framework for better understanding the distributional implications associated with the short- and medium-run reallocation of the labor force in the context of external labor market innovations.

This article departs from the existing literature in four important ways. First, prior research on the labor market impacts of environmental regulation has primarily focused on employment in manufacturing industries (Greenstone 2002; Kahn and Mansur 2010; Walker 2011). For example, Greenstone (2002) finds that the CAAA of the 1970s led to a loss of more than 500,000 jobs in regulated sectors relative to unregulated sectors. However, it is difficult to interpret the effects of job loss without knowing the long-run earnings losses associated with these job transitions. Numerous publications have highlighted the lack of credible estimates pertaining to the costs and economic incidence of environmental regulations for workers in these industries (see, e.g., Jaffe et al. 1995; Arrow et al. 1996; Greenstone 2002; Congressional Budget Office 2009).

2. The empirical literature on labor market adjustment to external factors is vast. For related work studying the effect of international trade on labor markets see Borjas and Ramey (1995), Topalova (2010), and Autor, Dorn, and Hanson (forthcoming); related work on immigration and labor markets includes Borjas, Freeman, and Katz (1997) and Ottaviano and Peri (2007); last, work looking more generally at labor market adjustment to innovations in labor demand includes Blanchard and Katz (1992), Bound and Holzer (2000), Notowidigdo (2012).

Second, it is widely acknowledged that industry-level wage and employment data are likely to be misleading in terms of labor market incidence. A large literature has documented barriers to the short-run adjustment of wages to productivity or labor market conditions, and these barriers may constrain wages to remain above market-clearing levels. In such a case, industry wages may respond minimally to external shocks, while a large fraction of workers in these industries may now be without jobs. Moreover, industry wages do not reflect the long-run costs of job loss for affected workers, because workers are often unemployed between jobs and/or transition to other sectors of the economy (see, e.g., Jacobson, LaLonde, and Sullivan 1993; von Wachter, Song, and Manchester 2009). Last, if firms and industries respond to shocks by laying off their least able or least senior workers (Abraham and Medoff 1984; Gibbons and Katz 1991), then industry earnings measures will be biased by these compositional changes in the workforce (e.g., more productive workers remain ex post).

I exploit detailed longitudinal data to follow workers over time and across jobs. The use of longitudinal data on workers overcomes many of the existing limitations. The primary estimation framework follows cohorts of workers in newly regulated counties and sectors over time and before and after plant-specific regulatory changes. The cohort-style analysis is meant to address concerns pertaining to compositional biases (i.e., the composition of workers is constant by construction) while also incorporating potentially costly job transitions into the average earnings estimates. By following cohorts, the baseline earnings estimates consist of both the long-run earnings changes for workers who remain in the newly regulated sector as well as the long-run earnings changes of the workers who leave the sector. Both of these earnings components are crucial for understanding the wage costs of labor reallocation, but due to data limitations they are most often studied in isolation.

The third departure from previous work comes from the use of a new, plant-level data set from the Environmental Protection Agency (EPA) that details exactly which plants are regulated under the various environmental programs in the United States. The Clean Air Act regulations apply heterogeneously within certain industries. Prior literature in this area has had to rely on more aggregate, industry-level proxies for environmental regulation because this plant-level data was not yet available.

I match this database to administrative, plant-level data from the U.S. Census Bureau, allowing me to observe *plant-level* regulatory status over time.

Fourth, this article is also able to lend insight as to how workers and labor markets adjust to sector specific shocks. Existing evidence suggests that local labor markets adjust to innovations in labor demand primarily through worker migration across labor markets (Blanchard and Katz 1992; Bound and Holzer 2000). However, this evidence is somewhat indirect because existing data do not permit fine-grained analysis of worker transitions.[3] This project combines both geographic and temporal detail to observe and estimate transitional dynamics surrounding wage and mobility responses to regulatory shocks.

The results in this study suggest that the reallocative costs to the workforce from the 1990 CAAA are significant. For those workers in the regulated sector prior to the change in regulation, the average earnings declined by more than 5% in the three years after the regulation. These earnings declines are persistent and only begin to recover some five years after the policy. The average worker in a regulated sector experienced a total earnings loss equivalent to 20% of their preregulatory earnings. Almost all of the estimated earnings losses are driven by workers who separate from their firm, highlighting the importance of longitudinal data when characterizing the costs and incidence of labor market adjustment. There is also substantial cross-sectional heterogeneity in the regulatory impact. For example, earnings losses depend on the strength of the local labor market, which suggests that policy-induced labor market reallocation may be more costly in periods of high unemployment. In aggregate, the total forgone wage bill associated with this regulation-induced sectoral shift in production is estimated to be more than \$5.4 billion (in 1990 dollars). These forgone earnings estimates are two orders of magnitude below most estimates of the health benefits of the 1990 CAAA.

The rest of the article is organized as follows: The following section discusses the details of the Clean Air Act more fully with a brief discussion of the previous literature. Sections III and IV discuss the research design and and data; Sections V and VI

3. Previous research examining labor market responses to innovations in labor demand either relies on aggregate state-level data (see, e.g., Topel 1986; Blanchard and Katz 1992) or long panels incapable of identifying detailed dynamic responses to shocks (see, e.g., Bound and Holzer 2000; Notowidigdo 2012).

discuss the econometric framework and estimation results. Section VII concludes.

II. The Clean Air Act and Environmental Regulation

Air pollution regulation in the United States is coordinated under the Clean Air Act (CAA), the largest environmental program in the nation. The CAA requires the EPA to develop and enforce regulations to protect the general public from exposure to airborne contaminants that are known to be hazardous to human health. The act was passed in 1963 and significantly amended in 1970, 1977, and 1990. The enactment of the CAA of 1970 resulted in a major shift in the federal government's role in air pollution control, authorizing federal and state regulations to limit emissions. In doing so, the EPA established national ambient air quality standards (NAAQS), which specify the minimum level of air quality acceptable for six criteria air pollutants.[4]

The 1977 Clean Air Act Amendments stipulated that every county in the United States must be designated annually as being in attainment or out of attainment (nonattainment) of NAAQS. When a county is out of attainment for one of the regulated pollutants, the EPA requires states to adopt regulatory plans, known as state implementation plans (SIPs), to bring their county into compliance. The EPA can impose sanctions in areas that fail to comply with these requirements, and sanctions include the withholding of federal grant monies (e.g., highway construction funds), direct EPA enforcement and control (through federal implementation plans), and bans on the construction of new establishments with the potential to pollute.

The regulatory plans require polluting plants locating in a county labeled as out of attainment or substantially expanding operations at an existing plant to adopt "lowest achievable emission rates" (LAER) technologies without regard to costs. Moreover, any new emissions from plant entry or investment/expansion must be offset from an existing source within the same county. In contrast, in areas designated as attainment, large polluting plants must use "best available control technology" (BACT). In cases concerning BACT, the economic burden on the

4. These pollutants consist of sulfur dioxide (SO_2), particulates (TSP, PM2.5, and PM10), nitrogen dioxide (NO_2), carbon monoxide (CO), ozone, and lead.

plant is considered in arriving at a final solution. Using plant-level survey evidence, Becker (2005) finds that BACT is significantly less costly to plants than LAER technology. Since SIPs require states to develop plant-specific regulations for every major source of air pollution, existing plants in nonattainment areas also face greater regulatory scrutiny than plants in attainment areas. These plant-specific regulations typically have come in the form of emissions limits. Regulatory compliance may also necessitate redesigns in production processes, introducing additional costs if output must be suspended in the interim (Becker and Henderson 2000). Becker and Henderson (2001) attempt to quantify these costs using plant-level data from the Census of Manufactures. They estimate that total operating costs were 17% higher in polluting plants from nonattainment areas relative to similar plants in attainment areas. In addition to the increased abatement expenditures, inspections and oversight are more persistent in nonattainment areas.

The CAAA of 1990 officially designated a set of counties as nonattainment for a new particulate matter standard (PM10) while also formally requiring all polluters of criteria air pollutants to obtain an operating permit.[5] The requirement that polluting sources obtain operating permits is crucial for this study, as it allows one to observe regulatory status at the plant-level, something that has not been possible in previous work in this literature. The EPA also formally evaluated their existing nonattainment designations for each air region so that 137 counties found themselves in nonattainment for at least one new pollutant, a 34% increase. Online Appendix Figure C.1 shows the 1990 CAAA led to by far the largest documented increase in county nonattainment designations since 1978.

In no small part due to these regulations, pollution levels have declined considerably from 1970 levels (Henderson 1996; Chay and Greenstone 2003a). More recently, pollution levels have declined even further despite GDP rising, vehicle miles traveled increasing, population growing, and energy consumption

5. Title IV of the 1990 Clean Air Act Amendments established a tradable market for sulfur dioxide emissions. These markets apply nationally, are primarily for electric utilities, and do not correspond to the variation in nonattainment designation used in this study.

rising by more than 20% since 1990 (Environmental Protection Agency 2008; Auffhammer, Bento, and Lowe 2009). The combined evidence suggests that nonattainment designations are effective at reducing pollution levels, and much of this reduction comes through increased firm compliance.

III. The Clean Air Act as a Research Design

Credible estimates of the effects of new environmental regulations requires the identification of a group of firms and workers that are similar to those affected by the regulations in ways observable and unobservable to the econometrician. Wages vary tremendously across both firms and locations for reasons observed and unobserved to the econometrician. Moreover, this variation in the wage structure is likely correlated with a firm's status as a polluter. For example, polluting firms tend to be both older and larger (Becker and Henderson 2000; Walker 2011), and larger plants tend to pay significant wage premia (Brown and Medoff 1989). Workers may also demand some form of compensating wage differential associated with potentially harmful working conditions. Therefore, a naive comparison of earnings in polluting and nonpolluting firms of nonattainment counties is likely to yield biased estimates. Fortunately, the regulatory variation inherent in the design of the CAA provides a possible solution to this identification problem.

Due to the way the CAA is implemented, regulations vary over both time and space. Figure Ia shows counties in the United States that were in nonattainment for any pollutant in 1991. Because only some counties are regulated in a given year, it is possible to estimate models that include flexible controls for nationwide or industry-specific shocks to employment and/or earnings such as the recession in the early 1990s or the phase-in of the North American Free Trade Agreement throughout the 1990s. Temporal variation in regulations exists from counties that go in and out of nonattainment based on annual pollution levels, allowing for pre/post comparisons within counties, industries, plants, or workers. This means that any time-invariant unobservables unique to these groups may be controlled for by including a set of group-specific fixed effects while still controlling for nationwide trends as detailed before. The inclusion of group-specific fixed effects ensures that estimates are derived only from those

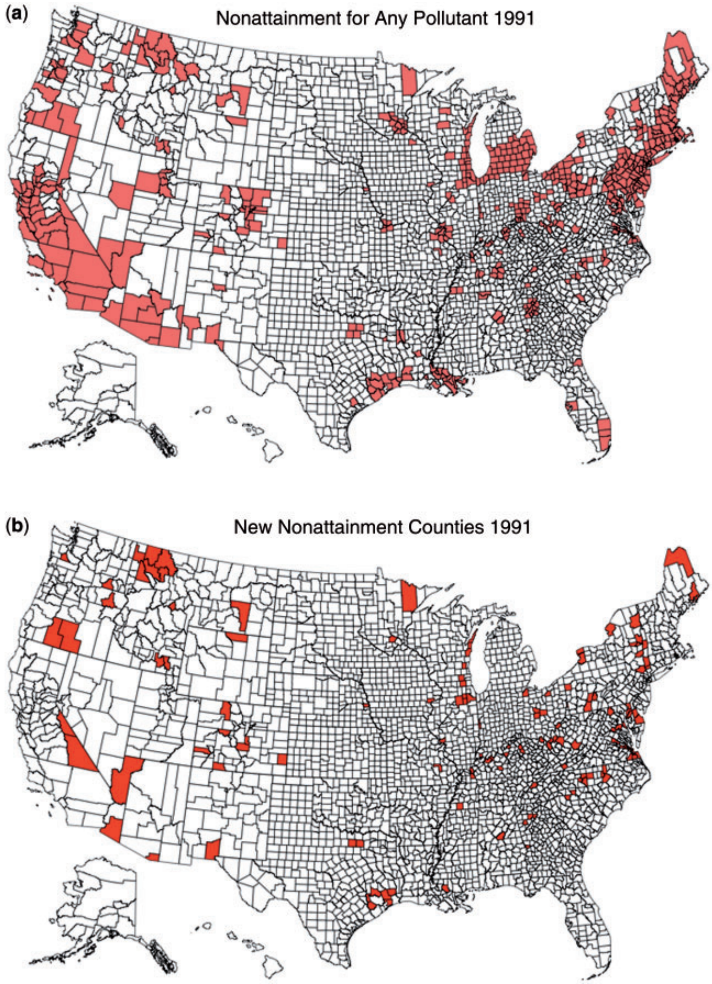


FIGURE I

Nonattainment Counties across All Pollutants in 1991. (a) Nonattainment for Any Pollutant 1991, (b) New Nonattainment Counties 1991.

The top panel shows all counties in nonattainment for any pollutant in 1991. The bottom panel shows those counties that switched into nonattainment in 1991 (i.e., from zero nonattainment designations to one or more designations). The top panel shows that although there is a considerable amount of variation in the cross-section, the nonattainment designation is primarily concentrated in large metropolitan areas and likely to be correlated with other unobservable features of urban labor markets. The bottom panel shows only those counties that switched into nonattainment status in 1991, the first year that the 1990 CAAA were adopted, showing a considerable number of counties that were newly regulated with the passage of the 1990 CAAA. These counties serve as the primary source of identification in the article.

sources that experience a *change* in the regulation—comparing outcomes before and after the change. In practice, nonattainment designations are fairly persistent; the mean duration of nonattainment for the sample of counties that were newly regulated in 1991 was around 14 years.[6]

A potential issue with time series variation in light of nonattainment designation is that pollution is correlated with economic activity (Chay and Greenstone 2003b). Therefore, counties that switch into nonattainment in a given year may also be more economically vibrant. To address this issue, this study relies on the discrete policy change induced by the 1990 CAAA. As mentioned, the 1990 CAAA were implemented in a way such that a handful of counties suddenly found themselves in nonattainment relative to the year prior. Figure Ib plots only those counties that switched into nonattainment status in 1991, the first year the 1990 CAAA were adopted.

Last, within any nonattainment county, only polluting plants are regulated and only if they emit the specific pollutant for which the county is in violation. Even within two-digit industry SIC codes, there is a considerable amount of variation in the fraction of plants that are classified as polluters. Online Appendix Figure C.2 plots the fraction of establishments in nonattainment counties that are classified as a polluter by the EPA, split by two-digit manufacturing SIC codes. Because only the polluting firms within a given county-industry are regulated, it is possible to control for unobservable, county-level (or county-by-industry level) changes in local economic conditions.

All of these sources of variation amount to a research design that examines the earnings outcomes of workers in polluting plants of newly regulated counties, before and after the introduction of the plant-level regulations—all while controlling for fixed, unobservable attributes of these workers, national variation in the polluting/nonpolluting sectors, and any common, county-level shocks experienced by all workers in a county. This research design amounts to a standard difference-in-difference-in-differences (DDD) regression estimator, which will be formally described after first providing an overview of the data.

6. This figure understates the actual duration of nonattainment counties because the nonattainment designation data from this study are right censored in 2008.

IV. Data Overview and Description

To understand the wage costs of the regulations for workers who remain in an industry and those who leave, one needs longitudinal data on workers and firms, as well as information detailing which set of firms were subject to these regulations. I have obtained access to two new data sources, heretofore not used to study the effects of regulation on local labor market shocks, that will be briefly described here. Online Appendix A provides additional details.

IV.A. The Longitudinal Employer Household Dynamics Files

The primary source of data used in this project comes from the Census Bureau's LEHD file, which provides administrative, quarterly earnings records for a large percentage of the U.S. workforce. I observe the complete employment history and earnings profile for each worker in the LEHD, conditional on the worker remaining within the reporting states over the course of the sample. Since the administrative earnings records are based on firms' reports that are used to calculate tax liabilities, they are presumably free of measurement error compared to existing survey data (Duncan and Hill 1985; Bound and Krueger 1991). The LEHD also provides important demographic information on workers such as age, race, and education as well as time-varying information from the firms at which they work. This provides a detailed snapshot of local labor markets at any given point in time.

The entire LEHD file consists of over 2.8 billion quarterly earnings records. To limit the computational burden of working with the complete file, I make some important sample restrictions. I begin by restricting the analysis to states in the LEHD that have data prior to the 1991 implementation of the CAAA. This limits the analysis considerably, as the only states that contain 1990 quarterly earnings are Illinois, Maryland, Washington, and Wisconsin. Online Appendix Table C.1 compares aggregate characteristics of this sample to those from a national sample. The counties in this study make up 20% of the total polluting sector employment share of counties newly regulated by the 1990 CAAA. When comparing newly regulated counties that are both in and out of the sample (i.e., columns (5) and (6) of Online Appendix Table C.1) we see that the newly regulated counties from these four states are slightly wealthier and have a slightly larger manufacturing base than the rest of the newly regulated counties. These will be important distinctions to consider when extrapolating the earnings estimates from this article to the rest of counties affected by the 1990 CAAA.

I limit the sample to workers who worked in the manufacturing and electric/gas industries (i.e., two-digit SIC 20–39, 49) in 1990.[7] This leaves me with a balanced panel of approximately 3 million workers in 1990 that I track over the course of their next 10 years, irrespective of whether they remain with their employer, transition outside of the manufacturing sector, or move across state lines. Note that as states enter into the LEHD throughout the 1990s, any workers who move from one of the four base states to "new" LEHD states show up when the new state enters the sample.[8] Earnings are deflated by the national-level CPI with the base year index as 1990.

Although the LEHD data are incredibly detailed in some regards, they have several important limitations that bear mention. As is true with most linked worker-firm administrative data sets, it is not possible to distinguish between unemployment and nonparticipation.[9] Moreover, the data do not allow the researcher to assess whether a missing earnings record is due to unemployment/nonparticipation or whether the worker moved outside of the states covered in the data. I explore the sensitivity of the results to reclassifying missing data in various ways designed to bound the true value. See Online Appendix A.1 for further details.

IV.B. Longitudinal Business Database

Since the LEHD contains earnings records only as far back as 1990, and the CAAA went into place in 1991, I draw on another

7. These industries constitute over 90% of the total number of plants regulated by the EPA. Although the electric/gas industry is one of the largest polluting sectors in the United States, electric utilities are also undergoing rapid restructuring over the sample period. I include this sector in the baseline sample while noting that results are not sensitive to this choice.

8. For example, California comes into the LEHD in 1991, and thus any set of workers who move from the four base states to California in between 1990 and 1991 will contribute an earnings observation for 1991. Online Appendix Table A.1 details state coverage in the LEHD.

9. Using administrative data linked to survey data, Frijters and van der Klaauw (2006) find this distinction not very important for men, but more so for women. Accordingly, I estimate robustness specifications using a sample of only prime-age males 25–55.

administrative data set from the Census Bureau to assess whether pre-CAAA trends differ significantly across the main sources of identifying variation. The Longitudinal Business Database (LBD) is a plant-level database that covers the universe of establishments in the United States from 1975 to 2005. Included in the database is annual information on employment, payroll, and firm age. The database also includes information on detailed industry, location, and entry/exit years for the respective establishments. Thus, I can observe trends in employment and earnings per worker before and after these changes for newly affected sectors relative to unaffected sectors. The LBD also allows me to revisit the previous literature pertaining to CAA regulation and sectoral employment (Greenstone 2002; Walker 2011) to see how sector size (as measured by employment) is related to variation in the regulations. I limit the LBD sample to plants in the manufacturing industry (SIC 20–39), the electric/gas industries (SIC 49), and plants residing in states covered by the LEHD sample.

IV.C. EPA Air Facility Subsystem

I match both the LEHD and LBD to plant-level, regulatory micro-data from the EPA. The EPA's AFS database is a plant-level database that provides information detailing the regulatory programs for which the plant is permitted (and regulated) as well as the specific pollutants for which the permit is issued.[10] An important aspect of this project is the ability to observe *plant*-level regulatory status over time. In doing so, this project is the first to use the AFS data to examine the effects of plant-level regulatory status on firm and worker outcomes.[11] Using the Census Bureau's Standard Statistical Establishment List (SSEL), I am able to link the LEHD and LBD to plant-level

10. Note that these permits are simply operating permits that the EPA requires polluting plants obtain to legally emit pollutants. These permits are unrelated to "cap and trade" permits from other pollution abatement programs, in that they arenontradable and are fixed for a particular source. There are additional air permit programs such as the Federally Enforceable State Operating Permit (FESOP) program, which are generally not included in the AFS database. However, the earliest of the FESOP programs begins in 1993 and hence should not affect the overall list of polluters in the AFS database (Environmental Protection Agency 2012).

11. In a companion paper, Walker (2011) uses the AFS linked to the LBD to examine how CAA regulatory status affects plant-level gross job flows.

regulatory and permit data from the EPA's Air Facility Subsystem (AFS) using a name and address matching algorithm. The exact details of the matching algorithm as well as the match rates are described in Online Appendix A.4.

One limitation of the AFS data is that it does not provide any information as to when these operating permits were issued. Fortunately, the regulatory structure of the CAA allows one to infer the timing of the regulations based on the county nonattainment status. Specifically, I define a plant as regulated if the plant has an operating permit in the AFS database and resides in a county that is in nonattainment for the specific pollutant on the permit.[12] I focus on PM10 and ozone designations, as they are the primary source of regulatory variation in the data.[13] I label a plant as a polluter of PM10 if it emits PM10 as identified in the pollutant-code field of the AFS database; I label a plant as a polluter of ozone if it emits VOCs or NOx, both precursors to ozone formation.[14] This plant-level definition of regulation is in contrast to previous research for which regulation was proxied by two- and four-digit SIC-level national pollution estimates (Greenstone 2002; List et al. 2003; Becker 2005; Kahn and Mansur 2010; Greenstone, List, and Syverson 2012).

Last, I match the EPA's annual county-level nonattainment designations to each data set.

IV.D. Aggregation

The linked worker-firm sample from the LEHD consists of a balanced panel of approximately 3 million workers for 10 years in the data so that the baseline sample consists of approximately 30 million annual earnings observations. For the baseline empirical analysis, I aggregate the data in two separate ways. First, I use the LBD to calculate mean earnings per worker and total employment in a county $\times$ sector $\times$ year, where a sector is based on the

12. I label a plant as regulated if it has one of the following permits within the "Air Program Code" field of the AFS database: Title V Permit, State Implementation Plant (SIP) Source, SIP Source under federal jurisdiction, Prevention of Significant Deterioration (PSD) permit, New Source Review (NSR) permit, or New Source Performance Standards (NSPS) permit.

13. There were a small number of counties newly designated as nonattainment for CO, but none are located in the states studied in this article.

14. Ozone is not directly emitted by plants and is formed through complex chemical reactions in the atmosphere. As a result, regulatory agencies focus on ozone precursors when trying to address ambient ozone levels.

polluting status of a plant.[15] Polluting status consists of four categories, defined by whether a plant emits (1) PM10, (2) ozone precursors, (3) both PM10 and ozone precursors, or (4) none of the above. Thus, each county has four possible observations in a given year. The second form of aggregation uses the LEHD to calculate the mean "cohort" earnings by collapsing the micro-data to the cohort $\times$ year. Cohorts are defined by the county, two-digit SIC industry, and sector of work in 1990, and each cohort-county has four observations per year to reflect earnings of workers in each of the polluting/nonpolluting sectors. In either model, collapsing the data eases the computational burden while also accounting for issues pertaining to inference when the identifying variation occurs at a more aggregate level (Bertrand, Duflo, and Mullainathan 2004).

IV.E. Baseline Industry and Worker Characteristics

Table I presents characteristics of the data in terms of plant, worker, and sectoral characteristics. The columns are indexed first by county (i.e., attainment, nonattainment, and those that switch into nonattainment following the CAAA of 1990) and then by the polluting/nonpolluting sector. Because this table presents statistics only from 1990, the sample only consists of manufacturing industries. As workers move from their initial 1990 employer, gradually other industries work their way into the analysis. When comparing polluting to nonpolluting sectors in Panel A, it becomes immediately clear that polluting plants tend to be older and larger than their nonpolluting counterparts. This distinction becomes important when comparing worker outcomes across these groups, as younger and smaller firms experience both faster growth (Dunne, Roberts, and Samuelson 1989) and tend to pay higher earnings conditional on size (Schmieder 2010).

Turning to Panel B of Table I, we see that workers in the polluting sector are paid considerably more than their nonpolluting counterparts. There are several reasons workers in the polluting sector may be paid more, such as higher rates of unionization, compensating wage differentials pertaining to worker health, and/or any skill differentials in the production process. This last point can be see when comparing education

15. Sectoral earnings measures are calculated using payroll/worker from the LBD, where the average sectoral earnings are calculated using a weighted average earnings measure, weighting by plant-level employment.

TABLE I

Sample Statistics: Preregulation (1990)

	(1) Attainment	(2)	(3) Nonattainment	(4)	(5) Switch into nonattainment	(6)
	Nonpolluting	Polluting	Nonpolluting	Polluting	Nonpolluting	Polluting
Panel A: Plant characteristics (LBD)						
No. of plants	6,806	1,707	5,910	1,549	7,622	1,452
Plant employment	146.82	244.6	133.15	266.42	144.24	234.58
Plant age	8.64	10.07	8.4	10.89	8.84	10.71
Panel B: Workforce characteristics (LEHD)						
Age	36.88	38.61	36.67	38.3	37.04	38.1
Education	13.34	13.56	13.72	14.08	13.67	13.89
Earnings	26,996.56	36,950.09	31,159.89	38,030.07	31,692.56	39,307.95
Separation rate	0.22	0.08	0.24	0.11	0.23	0.08
Same ind, same county	0.04	0.01	0.05	0.02	0.04	0.01
Same ind, diff county	0.02	0.01	0.05	0.02	0.01	0.00
Diff ind, same county	0.06	0.02	0.05	0.02	0.1	0.03
Diff ind, diff county	0.09	0.03	0.08	0.03	0.06	0.02
Leave sample	0.01	0.01	0.02	0.02	0.02	0.02
Sep: job to job	0.19	0.06	0.2	0.08	0.18	0.05
Panel C: Sectoral characteristics (LBD and LEHD)						
Total employment	233,994	210,386	179,590	146,123	379,553	209,364
Total earnings (in $millions)	6,317.03	7,773.78	5,596	5,557.07	12,029.01	8,229.67

Notes. Sample statistics for the "base" states from which the LEHD workforce sample is drawn (IL, MD, WA, WI). Since these are statistics for the "base" year, they only include the manufacturing industry. All dollar amounts are reported in 1990 dollars. Separation is defined as working in 1990 and working either at a different plant in 1991 or having missing earnings in 1991. The separation rate is further decomposed into five different categories listed in the table. LBD denotes characteristics derived from the LBD sample. LEHD denotes characteristics from the LEHD worker cohort sample.

Source. LEHD, LBD.

across polluting/nonpolluting sectors. Workers in the polluting sector are, on average, older and more educated than their nonpolluting counterparts.

Table I highlights two of the major forms of selection that need to be accounted for in any research design evaluating the differences in earnings profile across these groups. Younger firms tend to pay higher wages, and they experience higher separation rates relative to their older counterparts. In contrast, workers in the polluting sector tend to be older and more educated, leading them to have higher earnings. The primary research design relies on within-sector comparisons before and after the changes in regulatory status. Accordingly, the main source of identifying variation comes from within column (6) of Table I, which constitutes the polluting sector in counties that switched into nonattainment following the CAAA of 1990.

V. Measurement Framework

There are three margins of variation inherent in the basic empirical framework: county nonattainment status ($c \in$ Attain, Nonattain), sectoral polluter status ($s \in$ PM10, ozone, both PM10 and ozone, neither PM10 nor ozone), and two time periods ($\tau \in$ Pre, Post). County nonattainment designations are pollutant-specific, and they apply only to plants that emit the regulated pollutant. For example, a plant will be subject to PM10 nonattainment designation if it emits PM10 in a county newly designated as nonattainment for PM10; if the plant only emits CO, that plant will not be subject to the regulations contained within the PM10 nonattainment designation.

To effectively model this regulatory pattern, let N_c^{ρ} be an indicator equal to 1 for counties that were newly designated as nonattainment for pollutant ρ. Let P_s^{ρ} be an indicator for the sector of plants that emit pollutant ρ, and let $1(\tau_t > 0)$ be an indicator for the years after the introduction of the new regulations. Then $N_c^{\rho} \times P_s^{\rho} \times 1(\tau_t > 0)$ is an indicator equal to 1 for those sectors that change regulatory status with the introduction of the 1990 CAAA (i.e., plants that emit pollutant ρ in counties designated as nonattainment for pollutant ρ in the years after nonattainment went into place). Given the structure of the data, there are two possible ways for $N_c^{\rho} \times P_s^{\rho} \times 1(\tau_t > 0)$ to equal 1 in the years after the 1990 CAAA: (1) sector s emits PM10 and is in a county newly designated as nonattainment for PM10, or (2) sector s emits precursors to ozone and is in a county newly designated as nonattainment for ozone.

The interaction $N_c^{\rho} \times P_s^{\rho} \times 1(\tau_t > 0)$ is designed to estimate the average effect of nonattainment designation on the sectors directly affected by the designation, forming the basis for the following DDD estimator:

$$(1) \quad Y_{jcst} = \eta_1[N_c^{\rho} \times P_s^{\rho} \times 1(\tau_t > 0)] + \chi_{jcs} + n_{ct} + p_{st} + \Phi_{jt} + \epsilon_{jcst},$$

where an outcome Y_{jcst} (i.e., earnings and employment) in the polluting sector s of industry j in county c in year t is regressed on the explanatory variable of interest $[N_c^{\rho} \times P_s^{\rho} \times 1(\tau_t > 0)]$ and a series of control variables. The control variables include county $\times$ industry $\times$ sector indicators χ_{jcs} meant to model time-invariant observed or unobserved characteristics that govern outcome Y_{jcst}; a vector of nonattainment $\times$ year effects n_{ct} to model aggregate shocks common to nonattainment counties in a given year; a vector of polluting sector $\times$ year fixed effects p_{st} to control for unobserved shocks common to all polluting plants in a given year; and a vector of industry $\times$ year fixed effects Φ_{jt} to control for time-series shocks to specific industries in a given year.[16] The error, ϵ_{jcst}, represents unobserved county $\times$ industry $\times$ sector shocks to outcomes that are assumed to be uncorrelated with the regressor of interest

$$E[\epsilon_{jcst} \times (N_c^{\rho} \times P_s^{\rho} \times 1(\tau_t > 0)) | \chi_{jcs}, n_{ct}, p_{st}, \Phi_{jt}] = 0.$$

Equation (1) is simply a DDD estimator of the change in outcome Y_{jcst} attributable to changes in nonattainment designation for the polluting sectors affected by the designation. Notice that all first- and second-order interaction terms associated with a triple-difference estimator are implicitly included in equation (1), where time-invariant county c and sector s covariates are absorbed by the county $\times$ industry $\times$ sector indicators, χ_{jcs}, and the year effects are absorbed by the vector of industry $\times$ year fixed effects, Φ_{jt}. The coefficient η_1 in equation (1) delivers the reduced-form

16. The polluting sector in the polluting sector $\times$ year fixed effects p_{st} is defined as a plant that emits either ozone precursors or PM10. Similarly, nonattainment in the nonattainment $\times$ year fixed effects n_{ct} is defined by the set of counties that experienced a change in ozone or PM10 nonattainment status with the 1990 CAAA. In alternative specifications, I allow these indicators to be pollutant-specific, which has no impact on the baseline coefficient estimates.

effect of nonattainment designation on the sector directly affected by the designations. Because the impacts of nonattainment designation may be heterogeneous based on the pollutant being regulated, I also estimate models that allow for pollutant-specific regulatory heterogeneity, allowing η_1 to differ by pollutant.

In practice, I generalize equation (1) to allow the regulatory changes to evolve incrementally for the m years before and M years after the regulations go into place:

$$(2)\quad Y_{jcst} = \sum_{k=-m}^{M} \eta_1^k [N_c \times P_s \times 1(\tau_t = k)] + \chi_{jcs} + n_{ct} + p_{st} + \Phi_{jt} + \epsilon_{jcst},$$

where equation (2) is simply a generalization of a triple-difference estimator that allows any regulatory effects to evolve over time as opposed to assuming that the effect occurs immediately and lasts forever. The regression coefficients of interest $\hat{\eta}_1^k$ (for $k \in [-m, M]$) deliver event-study style regression estimates corresponding to the differential time path of employment and earnings in the newly regulated sectors in the years before and after the regulations go into place. Equation (2) serves as the principal econometric framework for the rest of the empirical analysis.

The identifying assumption in this class of models is that there are no other factors generating a difference in differential trends between production decisions in regulated and unregulated manufacturing firms. Although the identifying assumption of these models is untestable, data from other time periods and alternative specifications permit several indirect tests. First, data from years prior to the change in regulations permit the analysis of trends in covariates and outcomes prior to the change in policy. Second, the regulatory structure of the CAA provides different sources of identifying variation, allowing researchers to probe the robustness of their estimates to different identifying assumptions. For example, it is possible to isolate the source of identifying variation to come from within a county $\times$ industry $\times$ year, comparing the outcomes of polluters to that of nonpolluters within a county $\times$ industry $\times$ year cell; it is also possible to compare polluters across county $\times$ years, comparing polluters in newly regulated counties to polluters in counties that did not experience a change in regulatory status. Each specification lends some insight into potential threats to internal validity.

A few final estimation details bear mention. First, estimates using the LBD (employment and sectoral earnings) rely on samples for every year from 1985 to 2000. The LEHD samples (cohort earnings) are limited to work histories from 1990 to 2000. Because the LEHD begins in the year prior to the change in regulations (the regulations go into effect in 1991), this is the earliest period for which worker earnings records exist. Second, cluster-robust standard errors are used for inference to account for correlation between sectors and cohorts in the same labor market, both within periods and over time. Because there may be correlation between nonattainment status for counties within the same metropolitan area, I cluster standard errors by commuting zones (CZs) to account for this form of spatial dependence.[17] Third, the specifications are weighted by the sector or cohort employment size in the years before the change in regulations to account for heteroskedasticity-associated with differences in group sizes.[18]

VI. Results

The results are presented in the various subsections below. I begin by examining trends in aggregate sector employment. This is done to motivate the baseline empirical framework by first demonstrating that sectoral employment measures respond to changes in regulations. Section VI.B presents the central findings of the article, consisting of the earnings responses of workers in newly regulated sectors over time. Subsequent sections (VI.C–VI.E) offer various robustness checks and explore the mechanisms generating the observed earnings change as well as heterogeneity in the central estimates.

VI.A. Regulation Leads to a Reduction in Sectoral Employment

Previous literature has shown that the CAA regulations lead to industry downsizing (Henderson 1996; Greenstone 2002; Kahn and Mansur 2010; Walker 2011). However, these estimates reflect earlier time periods and/or a different sample of states, and it

17. The USDA Economic Research Service used county-level commuting data from the 1990 census data to create 741 clusters of counties that are characterized by strong commuting ties within CZs and weak commuting ties across CZs (Tolbert and Sizer 1996). Subsequent researchers have used similar levels of census geography for economic research on local labor markets (e.g. Autor and Dorn 2013).

18. For samples using the LBD, the weights correspond to sectoral employment in year $\tau = -5$. In samples using the LEHD, the weights correspond to the size of cohort in year $\tau = -1$.

is not clear how previous results generalize into this particular setting. I begin by estimating the degree to which sectoral employment responds to changes in environmental regulations. The focus on sectoral employment rather than plant employment is done so that regression estimates reflect employment flows on both the intensive and extensive plant operating margin. I draw on the LBD to construct measures of total sectoral employment for each county $\times$ sector from $\tau = -5$ to $\tau = 10$.

Figure II plots the event-time coefficients from a version of equation (2) using log(employment) in a county $\times$ sector $\times$ year as the dependent variable.[19] The plotted coefficients represent the time path of employment in the newly regulated sector relative to the year before the change in regulations conditional on county $\times$ sector fixed effects, polluting-sector $\times$ year, nonattainment $\times$ year, and year fixed effects. In the presence of county $\times$ sector fixed effects not all the event-time indicators are identified. For this reason, I normalize the coefficient on indicators in $\tau = -1$ to be equal to 0. There are two important features from Figure II. First, trends in employment in years prior to the change in regulations appear similar. The lack of significant differences in employment trends in the years prior to the change in policy provide an important test as to the validity of the identifying assumption; trends in outcomes across comparison groups evolve smoothly except through the change in policy. The second important feature of Figure II is that beginning with the year of the regulatory change, employment of polluting sectors in newly regulated counties begins to fall, reaching levels nearly 10% below 1990 employment levels in the five years after the regulatory change (or 15% below the counterfactual employment trends).[20] The magnitudes and dynamics of the estimates are similar to those of Walker (2011), which uses the same data set for the entire United States rather than this restricted subsample. See Walker (2011) for further discussion of these results as they pertain to plant and sectoral job destruction and job creation rates.

19. The focus is on total employment in a county $\times$ sector $\times$ year rather than more granular definitions of employment to incorporate changes on both the intensive and extensive plant-operating margin while also limiting attrition associated with log(employment) and the regulatory-induced "zeros" in more granular employment definitions. For a more detailed analysis of plant employment using plant-level micro-data see Walker (2011).

20. An auxiliary test of the slopes in Figure II, before and after regulation, rejects the null of equal slope with a p-value of .03.

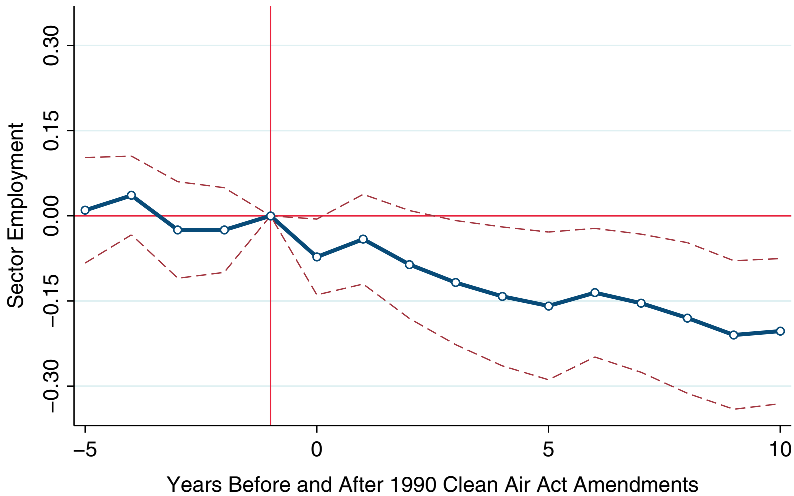


FIGURE II

Sector Level Employment before and after 1990 CAAA

Plotted are the event-time coefficient estimates from a version of equation (2), where the dependent variable consists of total sectoral employment in a county × sector × year. A sector is defined by one of four polluting categories. See text for details. The regression model controls for county × sector FE, nonattainment × year FE, polluting sector × year FE, and quadratic trends in county-level demographics. The first year of the nonattainment designation corresponds to year 0 in the graph. The regressions are weighted by the 1985 sectoral employment. The dashed lines represent 95% confidence intervals. An auxiliary test of the slopes before and after regulation rejects the null of equal slope with a *p*-value of .03.

Source. LBD.

VI.B. The Wage Costs of Sectoral Reallocation: Evidence from Cohorts

Table II presents the central findings of the article using cohort earnings as the dependent variable in various versions of equation (2). Each column in the table corresponds to a different regression, where the dependent variable is the mean log earnings of a cohort. Results are presented for different specifications that implicitly rely on different sources of identifying variation. All the columns report exponentiated coefficients from equation (2) using the translation $exp(\eta_1^k - 1)$. All columns control for the fraction of workers in various potential experience year bins (<5, 5–8, 8–13, 13–22, 22–32, 32–42, 42–47, and 47–55). I use a

TABLE II

EFFECT OF SECTOR-LEVEL REGULATION ON EARNINGS

	(1)	(2)	(3)	(4)	(5)	(6)
Regulation $(t+0)$	−0.033**	−0.031**	−0.034**	−0.036**	−0.036**	−0.033***
	(0.014)	(0.012)	(0.017)	(0.015)	(0.017)	(0.010)
Regulation $(t+1)$	−0.058***	−0.056***	−0.057***	−0.059***	−0.056***	−0.051***
	(0.012)	(0.014)	(0.019)	(0.011)	(0.014)	(0.012)
Regulation $(t+2)$	−0.046***	−0.045***	−0.062***	−0.040***	−0.051***	−0.030**
	(0.012)	(0.011)	(0.009)	(0.009)	(0.010)	(0.012)
Regulation $(t+3)$	−0.036**	−0.034**	−0.048*	−0.028**	−0.035**	−0.019**
	(0.017)	(0.016)	(0.026)	(0.012)	(0.016)	(0.009)
Regulation $(t+4)$	−0.041	−0.040	−0.054	−0.034**	−0.040**	−0.019**
	(0.026)	(0.025)	(0.033)	(0.016)	(0.019)	(0.008)
Regulation $(t+5)$	−0.011	−0.010	−0.020**	−0.013	−0.015	−0.011
	(0.014)	(0.015)	(0.009)	(0.014)	(0.009)	(0.014)
Regulation $(t+6)$	0.000	0.001	−0.002	−0.003	0.001	−0.011*
	(0.016)	(0.017)	(0.012)	(0.012)	(0.009)	(0.006)
Regulation $(t+7)$	0.003	0.004	0.008	−0.004	0.007	−0.010
	(0.012)	(0.012)	(0.013)	(0.011)	(0.012)	(0.009)
Regulation $(t+8)$	0.005	0.006	0.009	0.001	0.004	0.008
	(0.010)	(0.010)	(0.008)	(0.011)	(0.009)	(0.008)
9-year PDV	−0.202***	−0.191***	−0.241***	−0.199***	−0.204***	−0.162***
	(0.047)	(0.046)	(0.050)	(0.044)	(0.044)	(0.054)
N	153,249	153,249	153,249	153,249	153,249	153,249
2-digit SIC × year FE				X	X	
County trends		X		X		
County × year FE			X		X	
County × SIC × year FE						X

Note. This table reports regression coefficients from six separate regressions based on equation (2) where the dependent variable consists of mean log earnings in a cohort × year. Cohorts are defined by the county × industry × sector of work in 1990, and a sector is defined by one of four polluting categories. See text for details. All of the regressions implicitly or explicitly control for cohort specific FE, nonattainment × year FE, polluting sector × year FE, the fraction of workers in various potential experience bins (<5, 5–8, 8–13, 13–22, 22–32, 32–42, 42–47, and 47–55), quadratic trends in average cohort education, quadratic trends in county-level demographics, and year fixed effects. Standard errors are in parentheses and are clustered by commuting zone. Exponentiated coefficients are reported using the translation $(\exp(\eta_1^k)-1)$. The regressions are weighted by the 1990 cohort size. The final row of the table reports the discounted sum of the coefficients using a 4% annual discount rate (with discount factor β) and the translation $\sum_{k=0}^{8}\beta^k(\exp(\eta_1^k)-1)$. Standard errors for the final row are calculated using the delta method. ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

Source. LEHD.

measure of potential experience, defined as age – education – 6. Regressions also include interactions between time-invariant education categories (<12, 12–15, 16, 16+) and polynomial time trends in event time (up to a quadratic) to flexibly account for unobserved changes in returns to education over this time period; predetermined characteristics of the cohort county (population, income, and transfer spending), which I have similarly interacted with quadratic event-time polynomials (up to a quadratic); and controls for the first- and second-order interaction terms necessary for identification of a triple-difference estimator. These controls include switching-county $\times$ year FE, polluter $\times$ year FE, industry $\times$ year FE (or simply year FE), and cohort FE.[21]

Column (1) suggests that new environmental regulations lead to a reduction in earnings for the cohort affected by the regulations. The earnings decrease steadily over time, beginning to level off after three years, at which point they begin to recover to their preregulation level. The last row of Table II presents estimates of the total present discounted value of the earnings change over the nine reported years, discounting the future earnings changes using a 4% annual discount rate.[22] After nine years the average worker in the affected cohort experienced a present discounted earnings loss of around 20.2% of their preregulatory earnings. Note that these estimates do not condition on job separation and thus are a weighted average of earnings changes for those workers who leave the sector and those workers who stay.

Column (2) of Table II adds a common trend for earnings in each cohort county.[23] Here, estimates are identified by comparing earnings changes before and after the policy, after netting out any earnings trends common to both the polluting and nonpolluting sector workers from a given county. The results remain very similar to the estimates from column (1). Column (3) includes county $\times$ year fixed effects, so that the identifying variation comes from pairwise comparisons of cohort earnings for workers who were in either the polluting or nonpolluting sector of a newly regulated county in the years before the policy. The estimates remain similar to those presented in columns (1) and (2), albeit a bit larger in magnitude.

Columns (3)–(6) build on the specifications presented from columns (1)–(3) by controlling for unobserved, time-varying industry heterogeneity in earnings. Columns (4) and (5) add two-digit industry-year controls to the models controlling for county

21. Depending on the regression specification, some of these control variables will be absorbed by higher order fixed effects.

22. Specifically, this is calculated as $\sum_{k=0}^{8} \beta^k(\exp(\eta_1^k) - 1)$, where β^k is the discount factor corresponding to a 4% annual discount rate.

23. Recall that cohorts are defined by the 1990 county $\times$ sector. Thus, the "county trend" in Table II corresponds to a trend for each 1990 "county cohort."

trends (i.e., column (2)) and county × year fixed effects (i.e., column (3)), respectively. The results remain similar to before. Last, column (6) includes county × industry × year fixed effects to isolate the source of identifying variation to come from pairwise comparisons of workers in polluting versus nonpolluting plants of a regulated county × industry × year. The point estimates become smaller in magnitude, suggesting that the average worker in the affected cohort experienced a present discounted earnings loss of around 16% of their preregulatory earnings.

The identifying assumption in column (6) is the weakest among all columns of Table II; any unobserved shock that occurred only to workers in the polluting sector of a two-digit industry in a county that went into nonattainment in the years after nonattainment went into place would bias the regression estimates. However, the regression specification in column (6) comes with some important trade-offs worth highlighting. First, by restricting the source of variation to come from within a county × industry × year, the issue of control group "contamination" becomes more salient; externalities in the local-sectoral labor market may disproportionately impact workers in the control group (e.g., through labor supply effects and/or search externalities). Second, there are many counties for which an industry is either completely polluting or completely nonpolluting, and by including county × industry × year fixed effects the regression model effectively discards these potentially useful sources of variation.[24] Third, this specification also shuts off the seemingly useful source of variation contained in direct comparisons of polluters inside/outside newly designated nonattainment regions. Last, in this fully saturated regression model any measurement error in the treatment variable (e.g., through problems with matching external data sources or heterogeneity in regulatory enforcement), will lead to attenuation bias in the estimated coefficients, and this attenuation will be exacerbated in models with a large number of controls (i.e., fixed effects) as the signal to noise ratio of the measurement error relative to the treatment increases.

For the reasons just listed, the preferred specification throughout the rest of the article comes from column (4), a

24. The fraction of county × industry cells that have more than one observation per year (i.e., contain both polluters and nonpolluters) is only 30%. This means that models that include county × industry × year fixed effects effectively throw away 70% of the data.

model that exploits both within- and across-county variation, all while controlling for unobserved county-specific trends, industry $\times$ year fixed effects, nonattainment $\times$ year fixed effects, and polluting-sector $\times$ year fixed effects. Additional results based on the regression model in column (6) can be found in Online Appendix C.

Online Appendix Table C.2 presents estimates using alternative earnings measures. Column (1) of Table C.2 presents estimates where the dependent variable has been replaced with the average cohort earnings (as opposed to mean log(earnings)), and the results are similar to the baseline log estimates. The present discounted earnings loss amounts to $8,438, which is 21% of the average earnings for workers in the polluting sector of switching counties (i.e., column (6) of Table I). Column (2) presents estimates from models that replace any missing earnings observations with zeros. Recall that the baseline estimates ignore any missing *annual* earnings when calculating the average cohort earnings (i.e., up to three quarterly earnings can be missing per year). Because missing earnings may occur through either unemployment or sample attrition, it is not a priori clear how to address such an issue.[25] To understand the implications of missing earnings in my data, I replace any annual earnings data that is missing with a zero, conditional on that worker having positive earnings in the final year of the data. In theory, this should serve as a lower bound on earnings estimates as we are attributing all sample attrition to unemployment rather than other worker transitions (such as moving to non-LEHD states or switching into self-employment). As expected, the results are larger in magnitude.

Column (3) and (4) of Online Appendix Table C.2 presents results where I have replaced any missing earnings in a worker's earnings history with their last observed earnings. In contrast to filling missing earnings observations with zero, these results should serve as a useful upper bound on the absolute magnitude of the earnings losses. The estimates are indeed smaller in magnitude than those from Table II, but earnings losses still remain more than 19% of the preregulatory earnings. Columns (5)–(6) present results from a sample that consists only of workers that had nonmissing earnings observations in *every* year of the

25. This issue is not unique to this article, and other publications have dealt with these limitations in similar fashions. See, for example, Jacobson, LaLonde, and Sullivan (1993).

sample. This selection criteria mimics that employed by Jacobson, LaLonde, and Sullivan (1993) in their seminal publication on mass layoffs and the cost of job loss. This selection criteria results in a different sample of workers, but the results remain fairly similar. The main difference between this sample and the baseline sample is that it excludes cohorts for which all workers were unemployed for at least one year in the data. This selective attrition occurs primarily for small cohort sizes (i.e., rural areas) and is reflected in the heterogeneity of the estimates.

The average present discounted earnings loss estimate from Table II is around 20% of 1990 earnings. Multiplying this number by the average annual earnings in that sector (≈$39,000) and then by the total number of employees in the polluting sector of all "switching" counties in the United States (approximately 1 million workers),[26] the total forgone wage bill is approximately $7.8 billion. This thought exercise relies on the strong assumption of constant treatment effects across all newly designated nonattainment counties. Online Appendix B shows how treatment effect heterogeneity can sharpen out-of-sample predictions, suggesting that the total forgone wage bill is closer to $5.4 billion for the set of workers in newly regulated counties.

To facilitate interpretation, Figure III plots the event-time indicators from a version of equation (2) corresponding to column (4) of Table II. When comparing the transitional dynamics between Figure II and Figure III, one notices some important differences. Although employment trends in the newly regulated sector fall continuously in the years after the regulations go into place, the earnings dynamics initially fall and then begin to recover in subsequent years. There are a few possible explanations for these seeming discrepancies. First, workers are leaving the newly regulated sector both as a result of the regulations and due to the natural churn in the labor market. This means that in the years following the policy, the employment "effect" on cohort earnings will be attenuated by the fact that a smaller fraction of the workforce still works in the affected firm. For example, in the six years after the regulations, only about half of the workers in the newly regulated, polluting plants remain with their preregulation employer. Second, sectoral employment is measured relative to a counterfactual. It might be the case that employment is increasing in the counterfactual sector and remaining constant in

26. This last statistic comes from Table 1 of Walker (2011).

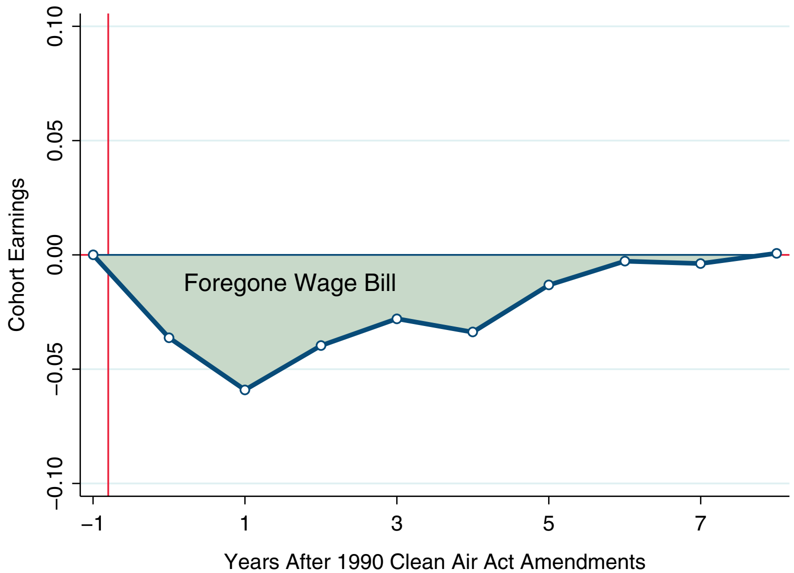


FIGURE III

Cohort Wage Trends after Nonattainment Designation

Plotted are the event-time coefficient estimates from a version of equation (2), where the dependent variable consists of the mean log earnings for a cohort × year, and cohorts are defined by the county × industry × sector of work in 1990. A sector is defined by one of four polluting categories. See text for details. The regression model controls for cohort specific FE, nonattainment × year FE, polluting sector × year FE, county-specific time trends, the fraction of workers in various potential experience bins (<5, 5–8, 8–13, 13–22, 22–32, 32–42, 42–47, and 47–55), quadratic trends in average cohort education, quadratic trends in county-level demographics, and industry-year fixed effects. The regressions are weighted by the 1990 cohort size. Exponentiated coefficients are reported using the translation $(\exp(\eta_1^k) - 1)$.

Source. LEHD.

the treated sector. To more fully understand the relationship between employment dynamics and earnings, one must look at job flows, which will be explored in subsequent sections. Last, another important factor underlying the magnitude and duration of the earnings losses is the underlying condition of the aggregate and local labor market in early versus later years of the sample. During periods of low unemployment, workers experience much lower earnings losses during job transitions (Davis and von Wachter 2011), and the latter half of Figure III is characterized by unusually low rates of unemployment. The extent to which the local unemployment rate influences the regulatory impact of the CAA regulations is explored more formally in subsequent sections.

VI.C. Effects of Regulations for "Stayers" and "Leavers"

The baseline earnings estimates consist of the average earnings loss for the set of workers in the newly regulated sectors. However, this obscures a tremendous amount of heterogeneity in the earnings process across different groups of workers. Here, I decompose the earnings changes into the component explained by workers that remain with their initial employer and the component explained by workers who separate from their initial employer. Inference is challenging in this setting because workers who stay with their firm and workers who leave the firm are selected; workers who remain with the firm are likely positively selected (i.e., higher than average ability) and workers who leave are likely negatively selected (i.e., lower than average ability). To address the issue of selection bias explicitly, I look at wage changes for only the "stayers" and only the "leavers" in separate models. This allows the inclusion of group-specific fixed effects to isolate the source of identifying variation to come from within each of these groups, before and after the change in policy. By including group-specific fixed effects, the regression model implicitly controls for any time-invariant unobservable characteristics of these stayers or leavers.[27]

I stratify the treatment group based on whether the worker stayed with their firm or separated at some point within the first four years after the regulations. In each specification, I decompose the earnings for the affected cohort *only*. That is, the earnings of the various control group cohorts remain the same as in the previous section, whereas the newly regulated sector's earnings consists only of stayers or only of leavers, depending on the specification. Column (1) of Table III presents results from a version of equation (2) where I compare trends in earnings of the affected cohort only if they remain at their "base-year" firm for more than four years after the change in regulations (i.e., they remain at their firm for at least the years $\tau \in [0, 4]$). The results from column (1) suggest that the earnings of stayers are

27. This is equivalent to the identifying assumption in the large literature on the cost of job loss stemming from mass layoffs (Jacobson, LaLonde, and Sullivan 1993; von Wachter, Song, and Manchester 2009).

TABLE III

EFFECT OF SECTOR-LEVEL REGULATION ON EARNINGS: DECOMPOSITION BASED ON SECTORAL TRANSITIONS

	(1) Stayer	(2) Separator	(3) Separator: same industry same county	(4) Separator: diff. industry same county	(5) Separator same industry diff. county	(6) Separator diff. industry diff. county
Regulation ($t+0$)	−0.011	−0.087***	−0.033	−0.084***	−0.125***	−0.084***
	(0.019)	(0.007)	(0.021)	(0.011)	(0.015)	(0.008)
Regulation ($t+1$)	−0.027**	−0.184***	−0.123***	−0.171***	−0.124***	−0.178***
	(0.012)	(0.011)	(0.012)	(0.011)	(0.022)	(0.012)
Regulation ($t+2$)	0.004	−0.265***	−0.195***	−0.235***	−0.174***	−0.258***
	(0.009)	(0.026)	(0.026)	(0.029)	(0.012)	(0.022)
Regulation ($t+3$)	0.004	−0.267***	−0.220***	−0.257***	−0.179***	−0.272***
	(0.012)	(0.039)	(0.064)	(0.046)	(0.012)	(0.029)
Regulation ($t+4$)	−0.008	−0.208***	−0.153***	−0.190***	−0.109***	−0.225***
	(0.018)	(0.036)	(0.054)	(0.045)	(0.020)	(0.022)
Regulation ($t+5$)	0.014	−0.169***	−0.136***	−0.169***	−0.098***	−0.174***
	(0.015)	(0.021)	(0.046)	(0.028)	(0.016)	(0.013)
Regulation ($t+6$)	0.019*	−0.113***	−0.023	−0.107***	−0.032	−0.130***
	(0.011)	(0.011)	(0.016)	(0.012)	(0.021)	(0.013)
Regulation ($t+7$)	0.006	−0.063***	−0.026	−0.069***	0.004	−0.056***
	(0.012)	(0.010)	(0.017)	(0.011)	(0.014)	(0.010)
Regulation ($t+8$)	0.007	−0.034**	−0.002	−0.030	−0.014*	−0.040***
	(0.016)	(0.014)	(0.010)	(0.020)	(0.008)	(0.007)
9-year PDV	−0.000	−1.225***	−0.810***	−1.155***	−0.770***	−1.244***
	(0.053)	(0.098)	(0.141)	(0.120)	(0.067)	(0.082)
N	152,988	153,160	151,523	152,715	151,929	153,025

Note. This table reports regression coefficients from equation (2). Each column corresponds to a different regression, where the results are presented for six different samples as indicated in the table headings. An observation is a cohort × year where cohorts are defined by the county × industry × sector of work in 1990, and a sector is defined by one of four polluting categories. See text for details. All of the regressions control for cohort specific FE, nonattainment × year FE, polluting sector × year FE, county-specific time trends, the fraction of workers in various potential experience bins (<5, 5–8, 8–13, 13–22, 22–32, 32–42, 42–47, and 47–55), quadratic trends in average cohort education, quadratic trends in county-level demographics, and industry-year fixed effects. Standard errors are in parentheses and are clustered by commuting zone. Exponentiated coefficients are reported using the translation $(\exp(\eta_1^k) - 1)$. The regressions are weighted by the 1990 cohort size. The final row of the table reports the discounted sum of the coefficients using a 4% annual discount rate (with discount factor β) and the translation $\sum_{k=0}^{8} \beta^k(\exp(\eta_1^k) - 1)$. Standard errors for the final row are calculated using the delta method. ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

Source. LEHD.

essentially unaffected by the regulatory change. This finding of zero wage response for those who remain at their firm is consistent with previous literature examining wage responses to labor market shocks in the manufacturing industry (Blanchard and Katz 1992; Autor, Dorn, and Hanson forthcoming; Ebenstein et al. forthcoming). This result is also consistent with earnings estimates derived from a sectoral earnings regression. Online Appendix Figure C.3 plots the average earnings per worker in the newly regulated, polluting sector before and after the change in regulations using data from the LBD. The figure shows no significant industry earnings response to regulatory changes. Whereas previous research often cites composition bias as a potential reason behind this zero result, the results in Table III suggest that composition bias is not the answer (because the composition is constant by construction). One explanation for these results is that union wages are often set in multiyear contracts, making manufacturing wages less sensitive to external shocks in the short/medium run (Lewis 1963).

Column (2) of Table III presents the same model as before, except that I examine earnings changes for those who separate from their "base-year" firm in the four years after the change in regulations. Here we see much larger earnings changes that are all statistically significant. The pattern of the estimates suggests that the average earnings declines rapidly in the years following the change in regulations, and they begin to recover only in the later years. The present discounted earnings change for separators is more than 120% of their preregulatory annual earnings.[28]

In general, the earnings loss attributable to job separations in this context is smaller than estimates found in the literature on displacement induced by mass layoffs (Jacobson, LaLonde,and Sullivan 1993; von Wachter, Song, and Manchester 2009). For example, the average earnings losses attributable to mass layoffs from Davis and von Wachter (2011) is equivalent to 1.82 of the

28. Another estimate of the average cost of job transitions comes from "scaling" the baseline cohort earnings regressions (i.e., column (3) in Table II) by the number of workers we see transitioning out of that sector (i.e., Figure II). The average sectoral earnings loss for a cohort from Table II is around 23%. Estimates from Figure II suggest a more than 15% reduction in the sectoral workforce in the five years after the policy. This suggests that the average earnings loss for a worker who loses his or her job because of the policy shock is around 150% of their 1990 earnings. This is consistent with what we see for those who separate from their job in column (2) of Table III.

worker's preperiod earnings. Notably, the earnings recovery of the average worker in this setting is more rapid than that found in the displacement literature.[29] There are a few possible explanations for these discrepancies. First, it seems possible that the rapid earnings recovery comes from the fact that most of these regulations occur in dense, urban labor markets. Workers are likely able to reintegrate themselves into the workforce more quickly in these "thicker" labor markets than elsewhere where alternative job options are limited (Marshall 1920). Second, it is possible that some of the job transitions I observe are voluntary, job-to-job transitions for which workers often experience a *rise* in earnings (Bjelland et al. 2011). In contrast, the job transitions from mass layoffs are often characterized by involuntary job loss and prolonged unemployment durations (Jacobson, LaLonde, and Sullivan 1993; Davis, Faberman, and Haltiwanger 2006; von Wachter, Song, and Manchester 2009). Third, the rapid earnings recovery may be unique to this setting, stemming from the high-pressure labor market in the mid- to late 1990s (Katz and Krueger 1999). Last, an alternative explanation for this relatively quick recovery comes from the difference in research designs and research questions. Namely, the long-run implications from job loss for those who were affected by mass layoffs may be fundamentally different from those affected by the more gradual sectoral changes that we see in this setting.

Next, I decompose the earnings effects of separators (i.e., column (2) of Table III) based on the location and industry where workers in the newly regulated sector find their subsequent job: the same industry of the same county (column (3) of Table III), a different industry in the same county (column (4)), the same industry in a different county (column (5)), and a different industry in a different county (column (6)). The estimates suggest that the earnings changes for workers who stay within the same industry are significantly less than for those workers that change industries (even within the same county). This is consistent with previous literature suggesting a role for industry-specific human capital as a barrier to job mobility (Topel 1991; Neal 1995). Of course, these earnings losses also reflect losses due tononemployment between jobs that may also be

29. An important exception is von Wachter, Handwerker, and Hildreth (2008), who find similar earnings dynamics for displaced workers in California between 1991 and 2000, the same period of time covered in this study.

higher for workers who switch industries (Murphy and Topel 1987). Figure IV displays the estimates from columns (3)–(6), summarizing the dynamics and incidence by destination sectors.

VI.D. Heterogeneity and Robustness of Cohort Wage Estimates

The previous sections showed that most, if not all, of the long-run earnings losses accrue to the set of workers displaced from their preregulatory firm; the earnings of workers who stay are essentially unaffected. The subsequent empirical sections explore additional sources of regulatory heterogeneity. Table IV presents cohort earnings regression estimates for different subgroups of workers and firms. Each cell in the table consists of a different regression from a different sample, where the regression estimates represent the present discounted value earnings change over the nine-year horizon. Panel A begins by showing heterogeneity in earnings losses for different age groups of workers, defined by their preregulatory, 1990 ages. Column (1) explores earnings losses for prime-age male workers, defined as male-workers aged 25–55 in 1990. Results remain very similar in magnitude to the baseline estimates. Columns (2), (3), and (4) show that younger workers generally fare much better than older workers, which is consistent with the earnings loss findings in the job-displacement literature (Jacobson, LaLonde, and Sullivan 1993; von Wachter, Song,and Manchester 2009). This is likely driven by the fact that older workers tend to have longer job tenure in preregulatory jobs, which can lead to larger long-term earnings losses (Kletzer 1989; Neal 1995).

Panel B explores heterogeneity by sex, showing that female workers experience much larger earnings losses as a percentage of preregulatory income, although the 95% confidence intervals overlap between specifications. The larger earnings losses for women are consistent with recent estimates from the job displacement literature (von Wachter, Song, and Manchester 2009). Panel C of Table IV shows that workers who have earnings above the average earnings of their preregulatory firm experience larger earnings losses. This is likely driven by the same mechanism in Panel A; workers with above average earnings tend to be those who are most senior and have longer job tenure prior to the regulation.

Panels D and E of Table IV look at heterogeneity in earnings losses based on the preregulatory plant at which the worker

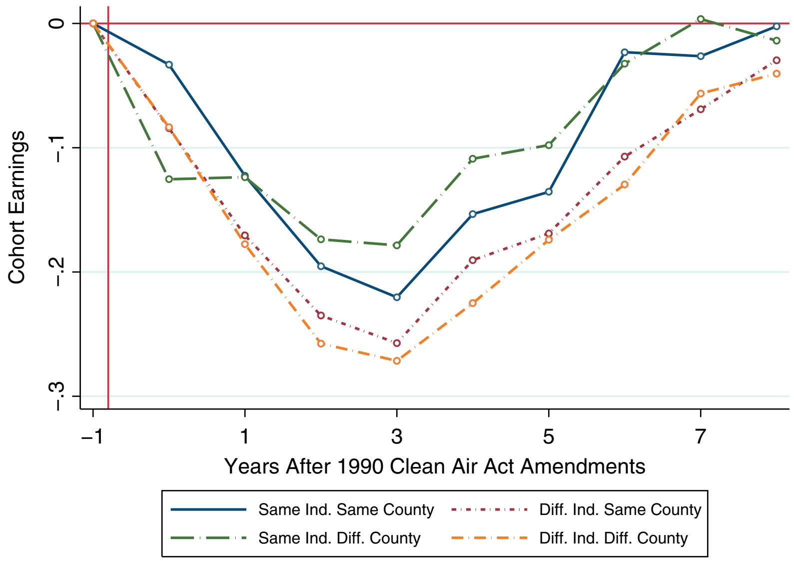


FIGURE IV

Effect of Sector-Level Regulation on Earnings: Decomposition Based on Sectoral Transitions

Plotted are the event-time coefficient estimates from regression coefficients stemming from equation (2). The lines correspond to the estimates reported in columns (3)–(6) of Table III. Each line corresponds to a different regression, where the results are presented for four different outcomes as indicated in the figure legend. An observation is a cohort × year where cohorts are defined by the county × industry × sector of work in 1990. A sector is defined by one of four polluting categories. See text for details. The regressions control for cohort specific FE, nonattainment × year FE, polluting sector × year FE, county-specific time trends, the fraction of workers in various potential experience bins (<5, 5–8, 8–13, 13–22, 22–32, 32–42, 42–47, and 47–55), quadratic trends in average cohort education, quadratic trends in county-level demographics, and industry-year fixed effects. Exponentiated coefficients are reported using the translation $(\exp(\eta_1^k) - 1)$. The regressions are weighted by the 1990 cohort size.

Source. LEHD.

worked. Panel D shows that workers in low-wage plants experienced larger earnings losses relative to workers in high-wage plants. Panel E splits estimates by whether a worker was in a plant with above or below median employment. The results suggest that the largest earnings losses come from workers in larger plants relative to smaller plants. However, because the 95% confidence intervals of each of the pairs of estimates in Panels D and

TABLE IV

HETEROGENEOUS EFFECTS OF SECTOR-LEVEL REGULATION ON COHORT EARNINGS

	Panel A: Age			
	(1) Prime-age male workers	(2) $25 \le$ age < 35	(3) $35 \le$ age < 45	(4) $45 \le$ age < 55
9-year PDV	−0.212***	−0.113**	−0.216***	−0.430***
	(0.060)	(0.051)	(0.055)	(0.108)
N	124,671	125,959	112,792	90,280

	Panel B: Worker sex		Panel C: Initial worker earnings	
	(1) Male	(2) Female	(1) Initial earnings < firm average	(2) Initial earnings ≥ firm average
9-year PDV	−0.154***	−0.271***	−0.080*	−0.270***
	(0.047)	(0.048)	(0.046)	(0.045)
N	138,829	124,266	122,573	150,211

	Panel D: Initial plant earnings		Panel E: Initial plant size	
	(1) Avg. plant earnings > avg. sector earnings	(2) Avg. plant earnings ≤ avg. sector earnings	(1) Plant emp ≤ 150	(2) Plant emp > 150
9-year PDV	−0.185	−0.188**	−0.039	−0.321***
	(0.255)	(0.081)	(0.184)	(0.108)
N	118,889	119,681	125,991	88,083

	Panel F: Unemployment rate		Panel G: Pollutant-specific	
	(1) Below median	(2) Above median	(1) PM10	(2) Ozone
9-year PDV	−0.062	−0.258***	−0.198***	−0.138
	(0.119)	(0.086)	(0.061)	(0.107)
N	87,636	65,613	153,249	

Note. This table reports regression coefficients from 15 separate regressions based on equation (2) where the dependent variable consists of mean log earnings in a cohort × year. Each panel presents two separate regressions, with the exception of Panel G, which reports the results from a single regression. Cohorts are defined by the county × industry × sector of work in 1990, and a sector is defined by one of four polluting categories. See text for details. All of the regressions control for cohort specific FE, nonattainment × year FE, polluting sector × year FE, county-specific time trends, the fraction of workers in various potential experience bins (<5, 5–8, 8–13, 13–22, 22–32, 32–42, 42–47, and 47–55), quadratic trends in average cohort education, quadratic trends in county-level demographics, and industry-year fixed effects. The table reports the discounted sum of event-time coefficients using a 4% annual discount rate (with discount factor β) and the translation $\sum_{k=0}^{8} \beta^k(\exp(\eta_1^k) - 1)$. The regressions are weighted by the 1990 cohort size. Standard errors are calculated using the delta method and are clustered by commuting zone. ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

Source. LEHD.

E overlap, the estimates are not precise enough for definitive conclusions.

Panel F explores heterogeneity in the underlying earnings losses based on the preregulation state of the local labor market. I stratify workers into counties with above and below median unemployment rates in 1990. The conditional mean unemployment rate in 1990 for below-median counties is 4%, and the conditional mean unemployment rate for above-median counties is 8%. Results suggest that workers in high-unemployment counties experience much larger earnings losses relative to their counterparts in “tighter” labor markets. These findings, that the economic conditions matter for reallocative incidence, echo recent findings in the displacement literature (Davis and von Wachter 2011).

The last panel of Table IV, Panel G, explores heterogeneity in the type of nonattainment designation. Recall that nonattainment designation is pollutant specific, and some counties were newly regulated for ozone whereas others were newly regulated for PM10. Panel G estimates the pollutant-specific effects jointly, delivering the average effect of ozone or PM10 related nonattainment designations while holding the other designation constant. Whereas the previous panels stratify the sample based on the underlying heterogeneity of interest, Panel G reports estimates from a single regression.[30] Of the two regulated pollutants, PM has a larger and more significant effect. However, a test of equality of the coefficients is not able to reject the null that the two regulations are of equal magnitude and sign. These results are consistent with those found in Walker (2011), who found that PM regulations have larger effects on employment growth, in particular plant-level job destruction rates, relative to that of ozone.

Online Appendix Table C.3 presents the results from Table IV controlling for county × industry × year fixed effects (i.e., the regression model from column (6) of Table II). The results are qualitatively similar, although the actual magnitudes differ in some models. Online Appendix Table C.4 explores the sensitivity of the regression estimates to removing potential

30. Specifically, I estimate the following model:

$$\tilde{Y}_{jcst} = \sum_{\rho \in \mathrm{PM},\, \mathrm{O}_3} \left(\sum_{k=0}^{10} \eta_1^{k\rho} [\tilde{N}_c^{\rho} \times \tilde{P}_s^{\rho} \times 1(\tau_t = k)] + \tilde{n}_{ct}^{\rho} + \tilde{p}_{st}^{\rho} \right) + \tilde{\chi}_{jcs} + \tilde{\Phi}_{jt} + \tilde{\pi}_{jcst} + \tilde{\epsilon}_{jcst} \tag{3}$$

where ρ is the pollutant-specific superscript.

outlier counties by estimating a series of regression models that leave out a single treated county in each iteration. I then take a sample average over the full set of treatment effects. The standard errors, in parentheses, reflect the standard deviation across all treatment effects. The mean effect over all samples is centered on the preferred baseline estimate, and the standard error around this mean is small in magnitude.[31]

1. The Heterogeneous Impacts of Nonattainment within the Earnings Distribution. Most of the previous results in this article have focused on the conditional mean with respect to earnings outcomes, but this obscures a considerably amount of heterogeneity across different parts of the earnings distribution. I explore some of this heterogeneity by estimating how regulations influence the fraction of individuals in various percentiles of the earnings distribution. I begin by calculating the 1st, 5th, 10th, 25th, 50th, 75th, 90th, 95th, and 99th percentiles of the within-cohort, pretreatment earnings distribution.[32] For each subsequent, post-CAAA year, I classify individuals into bins based on their place in the "pretreatment" earnings distribution. Collapsing the data to the cohort-year yields the fraction of individuals in a given cohort that are in the binned percentile of the 1990 earnings distribution. An increase in the binned percentile variable would mean that the regulations led to a relative increase in the fraction of individuals in this part of earnings distribution.

Table V presents results from versions of equation (2) that use the fraction of workers in various quantiles of the earnings distribution as the dependent variable. The results provide suggestive evidence that most of the mean effect is being driven by a reduction in earnings at the very top of the distribution and a subsequent increase in wage earners at the very bottom of the earnings distribution. There seems to be relatively little effect of the regulations within the bulk of the earnings distribution. These results, and those presented in Table IV, suggest that much of the earnings effects are concentrated by high-wage workers (e.g., older individuals), and these earnings losses are driven by changes in the extensive margin of work (i.e., workers

31. Alternatively, across the full distribution of "leave-one-out" style treatment effects, each of the estimates is negative and statistically different from zero.

32. The percentiles of the earnings distribution were calculated for workers with nonzero earnings.

TABLE V

EFFECT OF SECTOR LEVEL REGULATION ON THE EARNINGS DISTRIBUTION

	(1) $0 \leq p \leq 1$	(2) $1 < p \leq 5$	(3) $5 < p \leq 10$	(4) $10 < p \leq 25$	(5) $25 < p \leq 50$	(6) $50 \leq p \leq 75$	(7) $75 < p \leq 90$	(8) $90 < p \leq 95$	(9) $95 < p \leq 99$	(10) $99 < p \leq 100$
9-year total	0.045*	−0.005	−0.009	0.013	0.061	0.016	−0.005	−0.024	−0.064**	−0.028***
	(0.025)	(0.017)	(0.006)	(0.030)	(0.063)	(0.033)	(0.051)	(0.028)	(0.026)	(0.009)
N	156,324	156,324	156,324	156,324	156,324	156,324	156,324	156,324	156,324	156,324

Note. This table reports regression coefficients from nine separate regressions based on equation (2) where the dependent variable consists of the fraction of workers in the pretreatment, cohort-specific earnings percentile in the years after the regulations. The earnings percentiles are indicated in the column headings. Cohorts are defined by the county × industry × sector of work in 1990, and a sector is defined by one of four polluting categories. See text for details. All of the regressions control for cohort specific FE, nonattainment × year FE, polluting sector × year FE, county-specific time trends, the fraction of workers in various potential experience bins (<5, 5–8, 8–13, 13–22, 22–32, 32–42, 42–47, and 47–55), quadratic trends in average cohort education, quadratic trends in county-level demographics, and industry-year fixed effects. The table reports the nine-year sum of event-time coefficients. The regressions are weighted by the 1990 cohort size. Standard errors are calculated using the delta method and are clustered by commuting zone. ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

Source. LEHD.

transitioning between the 99th and 1st percentile of the earnings distribution).

2. Local Labor Market Spillovers and Additional Specifications. The results in previous sections showed that when we isolate the source of identifying variation to come from *within* a county $\times$ industry $\times$ year, the regression estimates are somewhat attenuated relative to estimates that use both within- and across-county variation for identification. Part of this result may have to do with spillovers in the local labor market and "contamination" of the control group; if workers disproportionately find new jobs within the same county (for which I show in subsequent sections), this could put pressure on earnings in "counterfactual" sectors (i.e., by pushing out the labor supply curve).[33] These type of labor market spillovers would violate the identifying assumption of the within-county $\times$ year (or within-county $\times$ industry $\times$ year) regression models. Online Appendix B explores two additional tests to better understand the magnitude of these possible biases. The results suggest that earnings fell by 1% in the nonpolluting sector of the manufacturing industry for newly regulated counties, and hence the labor market contamination effects in the "within-county" control group are small. Online Appendix B also explores the sensitivity of the estimates to limiting the sample to the set of counties within 1 standard deviation of the EPA's TSP standard; Online Appendix Figure C.4 summarizes the results, which remain similar to the baseline estimates both in magnitude and dynamics.

VI.E. Mechanisms: Regulation Increases the Separation Rate and Time between Jobs

Because most of the earnings losses seem to be driven by workers who separate from their firm, it is instructive to look at the dynamics of job separations to better understand how job flow dynamics interact with the underlying cohort earnings regressions. Because sectoral employment from the LBD only measures net changes in employment, I turn to the LEHD data to estimate the degree to which changes in environmental regulations lead to excess labor reallocation in the years following the policy. I begin

33. Here we are assuming that workers are substitutes across sectors and wages are set on a downward sloping demand curve.

by examining the rate of separation from a worker's "base-year" employer, and thus the following discussion parallels that of duration analysis, where a failure in this model is defined by a separation from the base-year firm.[34] Separation rates for a cohort are calculated at the county $\times$ industry $\times$ sector level as the number of base-year firm separations in a cohort-year over the total number of workers in that cohort that remain at their preregulatory establishment. The data are constructed so that workers contribute to the cohort-year observation for every year they remain with their 1990 plant. The risk set evolves over time as workers leave their 1990 plant.

The empirical analysis estimates how this failure rate changes as a function of the new environmental regulations. The regression framework is the same as used in equation (2), where the dependent variable is the failure rate for workers remaining at their 1990 "preregulation" firm. Given the definition of a job separation, there are a positive number of separations in the base year. The baseline estimates then measure the differences in the failure rate as a function of the change in regulatory status. Figure V plots estimates of the η_1^k's from equation (2), which corresponds to the difference in the failure rate for the newly regulated sector relative to the year prior to the nonattainment designation. Accompanying Figure V is a bar graph representing the fraction of workers in polluting plants of newly designated nonattainment counties that remain in their 1990 firm t years after the CAAA (i.e., the Kaplan-Meier survival probability). The figure shows that the rate of separation increases for the newly regulated cohort in the years following the regulations. After four years, the failure rate peaks and then begins to return to the baseline level. Similar to the sectoral employment regressions from before, the estimates here suggest that the changes in regulations lead to a relative reallocation of labor away from the newly regulated sector.

As before, we can decompose worker transitions based on destination sectors. This offers some degree of insight as to how workers respond to sector-specific shocks of this nature. In this case, a "failure" is defined as a job separation from the base-year firm *to* a specific location and/or industry. Figure VI presents these estimates for workers who transition to a different industry

34. A job separation is defined as equaling 1 in period t if earnings > 0 at plant j in period t and either earnings $= 0$ in period $t+1$ *or* $\text{plant}_{j_{1990}t} \neq \text{plant}_{j_{1990}t+1}$.

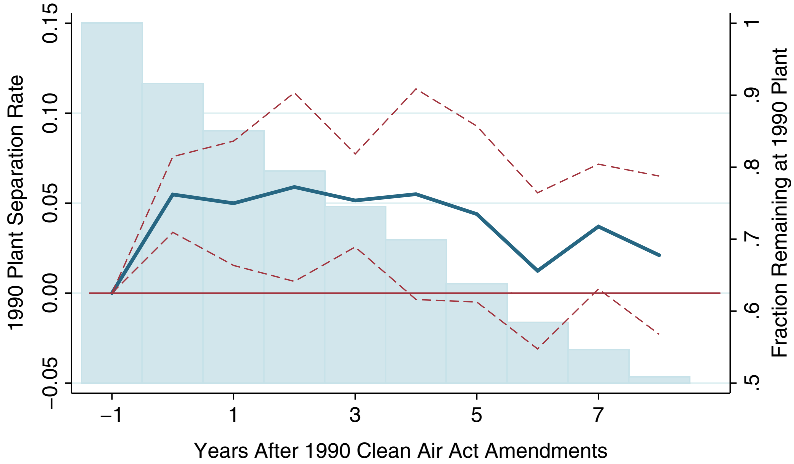


FIGURE V

Job Transition Rates from the Newly Regulated Sector

Plotted are the event-time coefficient estimates from a version of equation (2), which pertain to the difference in separation probabilities for the newly regulated sector after the change in regulations. An observation is a cohort × year where cohorts are defined by the county × industry × sector of work in 1990. A sector is defined by one of four polluting categories. See text for details. The regression controls for cohort specific FE, nonattainment × year FE, polluting sector × year FE, county-specific time trends, the fraction of workers in various potential experience bins (<5, 5–8, 8–13, 13–22, 22–32, 32–42, 42–47, and 47–55), quadratic trends in average cohort education, quadratic trends in county-level demographics, and industry-year fixed effects. The regressions are weighted by the 1990 cohort size. The dashed lines represent 95% confidence intervals. The bars represent the survivor probability for workers in polluting plants of newly designated nonattainment counties.

Source. LEHD.

in a different county (Panel A), the same industry in the same county (Panel B), a different industry in the same county (Panel C), and the same industry in a different county (Panel D). The figures suggest that workers in newly regulated sectors are disproportionately more likely to exit to a completely different industry after the regulations, relative to before. Furthermore, workers are more likely to transition to a different industry within the same county.

As an alternative to examining gross job flows, it is also possible to estimate the duration between jobs by looking at the average incidence of nonemployment for a cohort as a function of changes in regulatory stringency. Nonemployment is defined as

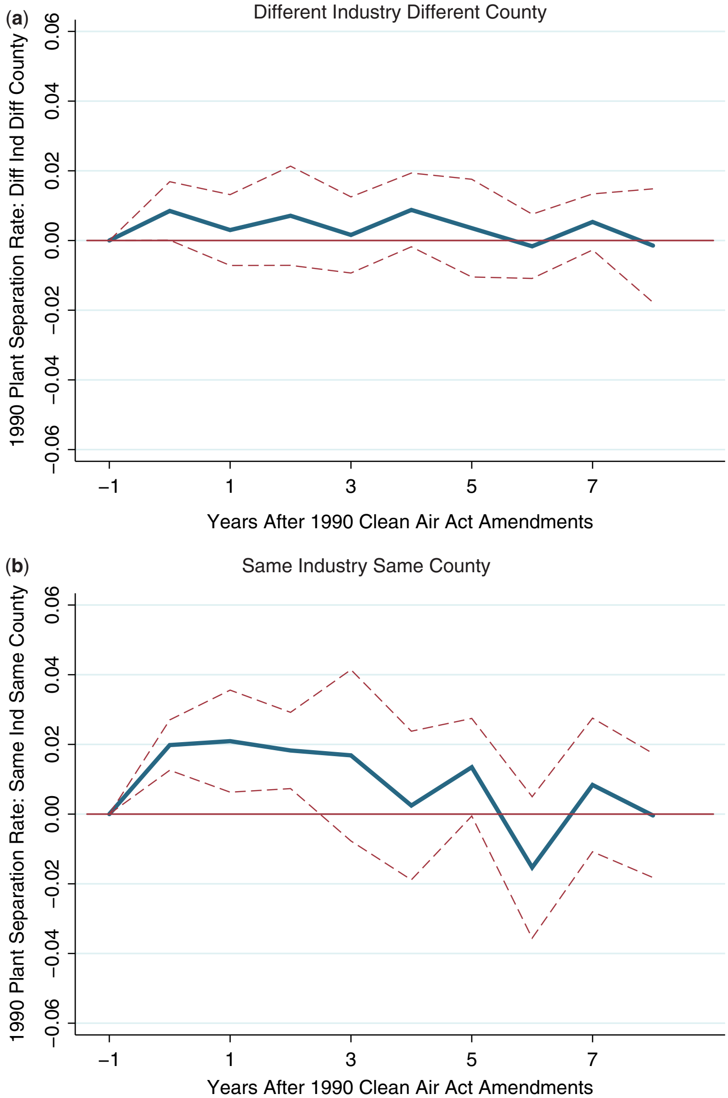
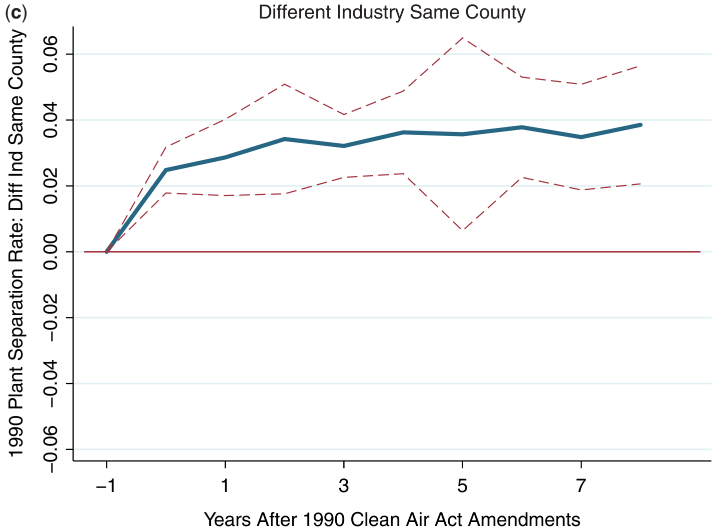


FIGURE VI

Decomposing Differences in Job Transition Rates. (A) Different Industry Different County, (B) Same Industry Same County, (C) Different Industry Same County, (D) Same Industry Different County.

Plotted are the event-time coefficient estimates from a version of equation (2), which decomposes the separation probabilities into destination sectors. The regression estimates pertain to the relative difference in separation probabilities by destination sector for the newly regulated sector after the change in regulations. An observation is a cohort $\times$ year where cohorts are defined by the county $\times$ industry $\times$ sector of work in 1990. A sector is defined by one of four



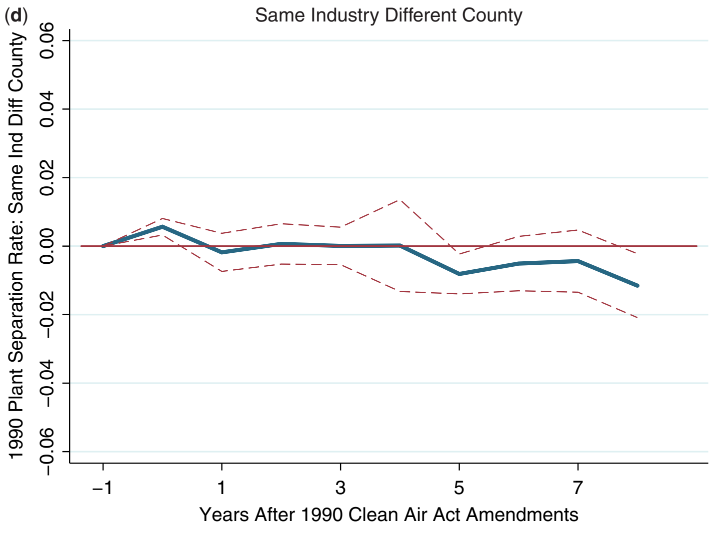


FIGURE VI

CONTINUED

polluting categories. See text for details. The regression controls for cohort specific FE, nonattainment × year FE, polluting sector × year FE, county-specific time trends, the fraction of workers in various potential experience bins (<5, 5–8, 8–13, 13–22, 22–32, 32–42, 42–47, and 47–55), quadratic trends in average cohort education, quadratic trends in county-level demographics, and industry-year fixed effects. The regressions are weighted by the 1990 cohort size. The dashed lines represent 95% confidence intervals.

Source. LEHD.

the number of quarters for which a worker has missing earnings in a given year, bounded below by 0 and above by 4. Online Appendix Table C.6 summarizes the results from a regression using the average quarters of nonemployment as a dependent variable in equation (2). The temporal dynamics match those from the earnings estimates and the separation hazards, but the results are somewhat sensitive to specification. The preferred estimate from column (2) suggests that the average duration of nonemployment increased by 0.25 quarters (i.e., approximately three weeks in a year) following the introduction of the regulations. Using the average annual earnings in the polluting sector of counties that switch nonattainment (i.e., column (6) of Table I), this amounts to $2,456 in forgone earnings. This is 31% of the estimated total earnings loss in column (4) of Table II. This exercise should be interpreted as more speculative because the earnings in periods before and after nonemployment may also be lower due to a reduction in the total amount of time spent working in the respective quarters.

VII. Conclusion

This article makes three primary contributions. First, the estimates document that the reallocative costs of environmental policy in the context of worker outcomes is significant. The average worker in a newly regulated plant experiences a present discounted earnings loss of around 20% of their preregulatory earnings. In aggregate, this amounts to almost $5.4 billion in forgone earnings. Prior research on the labor market effects of environmental regulation has primarily focused on employment losses in manufacturing industries, without regard for how quickly and at what costs workers are reallocated back into the labor market. The results presented here suggest that the predominant focus of the previous literature on employment misses important aspects of labor market adjustment to environmental regulations. Moreover, the results shed light on important heterogeneity in labor market adjustment, as earnings losses depend crucially on the underlying condition of the labor market in which they occur.

Second, the estimates shed light on how both firms and workers respond to gradual changes in regulatory stringency. Aggregate employment falls in affected sectors, but the wages for workers who remain at the firm do not. Instead, most of the costs of reallocation occur through costly job transitions associated with sectoral downsizing. Job transitions occur mostly from the regulated sector to other industries within the same labor market rather than across labor markets. These findings highlight the importance of longitudinal data for characterizing the costs and consequences of labor market adjustment.

Third, this article offers an approach to understanding the costs and consequences of labor market shocks in an economy where labor is not instantly reallocated and average industry wages may not fully reflect shifts in the labor demand curve. By exploiting detailed longitudinal data and following the same group of workers over time, regression estimates correspond to the average change in earnings for workers who stay at their firm and the cost of job transitions for those who leave. In addition, concerns pertaining to compositional biases are explicitly addressed.

The central estimates in this article reflect the earnings losses by workers who were working in a newly regulated sector in the years before the policy went into place. There are likely additional costs borne by these workers that are not captured by earnings alone. In addition, there are many labor market implications of environmental regulations for which these estimates shed little light. For example, regulations might affect the set of workers who had planned to work in the newly regulated sector by removing a once viable career tract. Similarly, regulations may also have some effect on the set of workers that enter the sector in the years after the policy. There may be additional labor market implications in sectors that benefit from environmental policy, such as industries that create and design pollution abatement equipment (Morgenstern, Pizer, and Shih 2002).

Broadly speaking, the estimates in this article are derived in a partial equilibrium framework, focusing on one particularly salient aspect of environmental policy: the reallocative costs born by workers in newly regulated firms. Therefore, given the research design and empirical framework, it is difficult to make precise inferences as to the overall economic effect of these regulations on the aggregate labor market or the economy more generally.[35]

35. Some researchers have attempted to estimate these macroeconomic or industry level effects of environmental policy (e.g., Morgenstern, Pizer, and Shih 2002) under a strong set of identifying assumptions, generally finding positive

Nevertheless, the EPA estimates the present discounted value (in terms of health benefits) of the 1990 CAAA from 1990 to 2010 to be between \$160 billion and \$1.6 trillion (1990\$) (Environmental Protection Agency 1999).[36] In light of these benefits, the earnings losses borne by workers in newly regulated industries are relatively small.

The arguments supporting environmental regulations point to increasing evidence that benefits from environmental policy far exceed the costs. The findings in this study do not contradict this logic; the wage costs borne by workers are a small fraction of the estimated benefits. These findings simply highlight the fact that regulations have distributional consequences—there are both winners and losers. The empirical approach here could be applied to any area concerned with the distributional implications of labor market adjustment, be it cost shocks to firms, local labor demand shocks to economies, trade shocks, or the incidence of natural disasters. The one limitation of this approach is the data necessary to implement it. However, with the growing availability of longitudinal micro-data, this should be a fruitful area of future research.

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SUPPLEMENTARY MATERIAL

An Online Appendix for this article can be found at QJE online (qje.oxfordjournals.org).

effects of environmental policy on employment. The results derived in this article are entirely consistent with the findings of Morgenstern, Pizer, and Shih (2002); the focus here has simply been on the reallocative incidence of sectoral reallocation and not the overall impact on employment per se.

36. Note that these estimates do not include the estimated benefits from the SO_2 trading markets, which are not analyzed in this study. The lower bound EPA estimate is similar to the estimated benefits from various hedonic studies which estimate marginal willingness to pay for improvements in air quality (see, e.g., Chay and Greenstone 2005; Bayer, Keohane, and Timmins 2009).

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