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THE POLLUTION HAVEN HYPOTHESIS

The Unintended Disincentive in the Clean Air Act

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Abstract

The Clean Air Act and its subsequent amendments have been lauded as the primary stimulant to the impressive improvement in local air quality in the US since 1970. A key component of these regulations is the New Source Review (NSR) requirement, which includes the contentious stipulation that when an existing plant seeks to modify its operations, the entire plant must comply with current standards for new sources. This requirement was included to improve air quality in dirty areas, and prevent a deterioration of air quality in clean areas. Yet, whether NSR provides the proper plant-level incentives is unclear: there are strong disincentives to undertake major plant modifications to avoid NSR. In our examination of more than 2500 and 2200 plant-level modification decisions and closures, respectively, we find empirical evidence suggesting that NSR retards modification rates, while doing little to hasten the closure of existing dirty plants.

KEYWORDS: environmental regulations, Clean Air Act, New Source Review, propensity score matching

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Recent reforms to the New Source Review (NSR) requirement in the Clean Air Act have emerged as a source of controversy.[1] These reforms, which relax some of the NSR requirements for the expansion of existing operations in certain industries, are, in part, a response to what has been a contentious issue surrounding NSR – the disparate treatment between existing facilities and new or expanding facilities. Existing facilities are "grandfathered" under much less stringent environmental standards, while new or expanding facilities are subjected to NSR and forced to install sufficient emissions control technologies. A provocative line of research has shown that the differential application of environmental regulations has led to unintended consequences among power plants: Nelson et al. (1993), Maloney and Brady (1988), Gollop and Roberts (1983), and Stewart (1981) present empirical evidence that suggests such uneven treatment has lengthened the lifetime of utility plants, admitting the possibility that air quality regulations have *increased* emissions of certain pollutants in the short run.

While these studies have provided invaluable insights, a related provision within the Clean Air Act (CAA) that has heretofore been ignored is the effect of NSR requirements on plant-level modification rates. Given that when an *existing* plant seeks to modify operations, the entire plant must comply with pollution control standards for new sources, it is quite possible that pollution-reducing modifications are deterred.[2] Accordingly, the CAA provides perspicuous incentives for existing plants: proceed with modification if the expected costs – potentially including installation of new capital that is less pollution-intensive if NSR is triggered – outweigh the expected benefits from more productive capital. Therefore, it is quite possible that this particular NSR requirement has contributed to *more*, rather than less, pollution in the short-run. This could be the case if NSR requirements deter modifications that would replace out-dated technology with new capital that, while potentially not as environmentally-friendly as technology mandated under NSR, is cleaner than the capital currently in use.

Our research goal in this study is not as ambitious as determining unequivocally whether NSR requirements governing modifications of existing plants have led to greater emissions. Rather, our main goal is to provide evidence on whether the NSR requirement for existing plants has retarded modification rates.[3] To our best

[1] Specifically, parts C and D of Title I of the Clean Air Act (CAA). This fact is highlighted when one considers the attention that the recent NSR reforms have received. For example, the New York Times reported on Nov. 22, 2002: "The Bush administration will ease clean air rules, allowing power plants and refineries to avoid new pollution controls when expanding operations… [T]he long-awaited rule changes will 'increase energy efficiency and encourage emissions reductions,' the Environmental Protection Agency said in a statement obtained by The Associated Press… Environmentalists and a group of Northeastern states said they plan to file suit immediately challenging the changes. The state officials contend that the easing of the clean air requirements 'will undermine efforts' to meet air quality standards."

[2] To be precise, only modifications of existing plants that are expected to lead to a net increase in emissions are subject to NSR. However, as explained more fully in Section I, the manner by which the expected change in net emissions is forecasted virtually guarantees that all modifications will fall within the bounds of NSR.

[3] More specifically, we examine whether attainment status influences modification and closure rates. We interchange NSR and attainment status throughout.

knowledge, current evidence is purely anecdotal. Yet in the absence of robust empirical evidence, these anecdotes have evidently taken on significant policy importance, as they have emerged in the most recent *Economic Report of the President* (2003; pp. 172; coauthored by List):

> The NSR permit process...might lead firms to delay or forgo plans to modernize their facilities in ways that would benefit the environment. To take just one example, a manufacturer that operates a process that includes a drying system determined that the system's energy efficiency could be improved if the existing drier nozzles were replaced with Teflon-coated nozzles. The firm found, however, that the replacement would be economical only if the expense of obtaining an NSR permit could be avoided. … Since the firm could not readily discern whether the installation of new nozzles would be considered routine maintenance, repair or replacement, it decided not to proceed with the project. In this way, NSR deters firms from conducting needed repairs and often results in unnecessary emissions of pollutants. In this case NSR requirements actually made the environment worse off.

Our study fills part of this chasm by making use of the spatial variation in NSR compliance costs across counties in the US. In counties labeled "out-of-attainment" of federally mandated air quality levels modifying plants are compelled to install technology that achieves the Lowest Achievable Emissions Rate (LAER), *regardless of cost*. Alternatively, plants located in counties with air quality that is "in-attainment" of federal standards face a NSR requirement that is less onerous; modifying plants only need to install the Best Available Control Technology (BACT), which in most cases is much less costly than LAER equipment.

In our empirical analysis we use a series of empirical methodologies, including a difference-in-differences semi-nonparametric propensity score matching estimator, to analyze panel data on more than 2500 plant-level modification decisions in New York State from 1980 – 1990 to determine the effects of nonattainment status on plant-level modification decisions, given the way that NSR rules differ by attainment status.[4] To provide an explicit link to the capital turnover studies discussed above, we supplement this exercise with an empirical examination of more than 2200 plant-level closure decisions in the same state and time period to determine the impact of county-level attainment status on plant-level closure decisions in industries other than electric utilities. By combining results from these two data sets, we can examine if the differential application of NSR provisions based on a county's attainment status have unintentionally altered the turnover of antiquated manufacturing capital. If modification and closure rates of pollution-intensive plants have been affected by county-level attainment status, then one cannot rule out the possibility that NSR has had deleterious effects on air quality.

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[4] See also Greenstone (2003) for another application of propensity score matching in the environmental literature.

Strikingly, the analysis is consonant with such a conclusion: our empirical estimates suggest that substantial modifications of existing pollution-intensive plants in nonattainment counties have been delayed, or foregone, without a concomitant increase in the turnover rate of dirty capital. In much the same manner that the Clean Air Act has altered new capital flows (e.g., Henderson, 1996; List et al., 2002), we find that the NSR requirement for existing plants has altered the equilibrium allocation of expenditures on capital improvement, conferring a competitive advantage to certain regions.

We view our work as fitting in broadly with the extant "pollution haven hypothesis" literature. While defined narrowly, the "pollution haven hypothesis" refers to the notion that multinational pollution-intensive firms will relocate to countries with weaker environmental standards, we take the hypothesis to represent a concept that is much more general: the tendency for pollution sources to locate, or relocate, in response to pollution policies. In this sense, our contribution is to examine one dimension of the pollution haven hypothesis that has received considerably less attention: the behavior of existing plants.

1. Data and Empirical Strategy

1.1. Data

The plant-level modification and closure data come from the Industrial Migration File (IMF), maintained until 1990 by the New York State (NYS) Department of Economic Development (DED). The goal of the IMF was to monitor all annual changes in manufacturing activity in NYS at the county-level. The IMF includes observations of individual plant openings, closings, and modifications and classifies plants by standard industrial classification (SIC) code.

Following previous studies, we focus on plants in pollution-intensive sectors most likely to be affected by county-level ozone attainment status.[5] Given that attainment status is determined by county-level air quality readings, we follow Greenstone (2002) and classify industrial sectors as "ozone pollution-intensive" if they emit at least 6% of the total industrial sector's emissions of nitrogen oxide or volatile organic compounds, the primary chemical precursors to ozone. Plants labeled pollution-intensive are in SIC

[5] The 1977 Clean Air Act Amendments set standards on five criteria air pollutants: sulfur dioxides (SO_2), carbon monoxides (CO), ozone (O_3), nitrogen oxides (NO_x), and total suspended particulates (TSP). Since ozone has attracted the most regulatory attention due to the limited progress that has been made to reduce concentration levels, we follow Henderson (1996), Becker and Henderson (2000), and List et al. (2002) and focus on county attainment status of ozone. Yet, we should note that if counties are in attainment for ozone and out-of-attainment for any (or all) other pollutants, then we are obtaining lower bound estimates of the treatment effect. Given that attainment status of the various individual pollutants affects abatement technology choice, however, we are comfortable with our modeling choice. We should note, in any event, that attainment status appears to be correlated across pollutants in our sample.

codes: 2611-31, 2711-89, 2812-19, 2861-69, 2911, 30, 32, 3312-3, 3321-5, 34, and 371. All remaining sectors are labelled non-pollution-intensive.

After classifying plant modifications by pollution intensity, the data are aggregated to the county-level. In total, we observe more than 2500 plant modifications: 742 (1788) pollution-intensive (non-pollution intensive) plant modifications across the 62 counties in NYS over the sample period. In addition, we observe 2269 plant closures: 561 (1708) pollution-intensive (non-pollution intensive) plant closures across the 62 counties in NYS over the sample period. We then combine these measures with each county's ozone attainment designation, and other county-level attributes (discussed below).[6] Over the sample period, slightly more than 25% of the observations are in nonattainment counties. Summary statistics are provided in Table 1.

Before proceeding, it is important to note in greater detail the costs associated with modifying operations in a nonattainment county. Before the most recent set of reforms mentioned in footnote 1, NSR required a source to obtain a permit prior to any major modification. The permit establishes various actions that the source must undertake to control its emissions of air pollution. For an existing source proposing a modification, NSR is only triggered if the modification will result in a net increase in emissions. However, the NSR program estimates post-modification emissions assuming the plant will operate 24 hours a day, year-round, even if the plant does not maintain non-stop production prior to the modification. As a result, even modest changes in a facility will most assuredly trigger NSR, even if those changes are unlikely to increase actual emissions. Once the NSR process is triggered, the time and monetary costs required prior to undertaking the modification may be extreme—permit haggling may take years, and the costs may reach millions of dollars.

We should note that we have used this data set for several other purposes. For example, List and McHone (2000), List et al. (2003a), and Millimet and List (2003) examine the influence of attainment status on plant births. Relatedly, List et al. (2003b) explore the effects of air quality regulation on the destination choice of relocating plants, while List et al. (2004) compare and contrast the effects of environmental regulations across foreign and domestic plant births. While there are certainly similarities across these studies—the methodologies, regressors, etc.—the outcomes of interest in these studies are new plant births or new plant relocations.[7] The current study analyzes a completely distinct issue: plant modifications (as well as closures). We link this issue to the current political debate concerning the NSR provision of the CAA.

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[6] Although attainment status can range from in-attainment of the primary standard to out-of-attainment, with partial standards in between, ozone designation has typically been polar in nature; that is, a county is either in- or out-of-attainment. For a county to be labeled out-of-attainment, according to the CAA its second highest daily air quality reading must exceed 0.12 parts per million. This is currently being revised by the EPA, however.

[7] Millimet and List (2003) also utilize the plant closure data examined herein, but not the data on plant modifications.

1.2. The Empirical Models

1.2.1. Propensity Score Matching Method

A method of assessing the impact of the differential application of NSR provisions based on a county's attainment status, on plant modification and closure rates is the method of propensity score matching developed in Rosenbaum and Rubin (1983). While extensively used by statisticians, economic applications have proliferated only recently. A few notable examples include Heckman et al. (1997), Dehejia and Wahba (1999, 2002), Smith and Todd (2000), and List et al. (2002). Blundell and Costa-Dias (2002) provide an excellent introduction to the matching method, concluding, "….matching methods have been extensively refined in the recent evaluation literature and are now a valuable part of the evaluation toolbox" (p. 4).

The fundamental problem in identifying the effect of a binary variable, such as attainment status, is one of incomplete information. While the econometrician observes whether the "treatment" (in this case, nonattainment of the federal ozone standard) occurs and the outcome conditional on treatment assignment, the counterfactual is not observed. Let y_{i1} denote the outcome of observation *i* with the treatment ($T_i = 1$); y_{i0} denotes the outcome without the treatment ($T_i = 0$). If both states of the world were observable, the average treatment effect, τ, would equal $\bar{y}_1 - \bar{y}_0$, where the former (latter) average represents the mean outcome for the treatment (control) group. However, given that only y_1 or y_0 is observed for each observation, unless assignment into the treatment group is random, generally $\tau \neq \bar{y}_1 - \bar{y}_0$. In the application, y represents a count of either plant modifications or closures.

The solution Rosenbaum and Rubin (1983) advocate is to find a vector of covariates, *Z*, such that

$$(1) \qquad y_1, y_0 \perp T \mid Z, \qquad\qquad pr(T = 1 \mid Z) \in (0,1)$$

where $\perp$ denotes independence. If one is interested in estimating only the average treatment effect, the weaker condition

$$(1') \qquad E[y_0 \mid T = 1, Z] = E[y_0 \mid T = 0, Z] = E[y_0 \mid Z], \quad pr(T = 1 \mid Z) \in (0,1)$$

is required. For condition (1') to hold, *Z* should be multi-dimensional. Consequently, finding observations with identical values for all covariates in *Z* may be untenable. However, Rosenbaum and Rubin (1983) prove that conditioning on $p(Z)$ is equivalent to conditioning on *Z*, where $p(Z) = pr(T = 1 \mid Z)$ is the propensity score. $p(Z)$ is estimated via logit.

After estimating the propensity score, a matching algorithm is required to estimate the missing counterfactual, y_{0i}, for each treated observation *i*. The simplest algorithm is (single) nearest-neighbor matching, whereby each treated observation is paired with the control observation whose propensity score is closest in absolute value (Dehejia and Wahba, 2002).[8] As unmatched controls are discarded, the matching method identifies a sub-sample of controls that more closely resembles the treatment group in terms of pre-treatment attributes. The treatment effect on the treated (TT) is given by

(2) $\tau_{TT} = E[y_1 \mid T = 1, p(Z)] - E[y_0 \mid T = 0, p(Z)] = E[y_1 - y_0 \mid p(Z)]$

During the actual estimation here, the nearest-neighbor algorithm is amended along three dimensions. First, while a match exists for each out-of-attainment county, the propensity scores may still differ considerably. Because the unbiasedness of the matching estimator relies on the propensity scores being identical, pairs with scores significantly different are excluded. We present estimates for several cut-off values, which is a procedure known as "caliper matching" in the literature (Cochran and Rubin, 1973).

Second, to utilize our panel data fully, we amend the caliper matching method by restricting the pool of potential controls to which a given treated observation may be paired. Specifically, we match each treated observation twice: first, restricting matched pairs to be from the same year and region of the state (see Figure 1); second, restricting matched pairs to be the same county from different years. By matching within-year/within-region, or within-county, we explicitly remove any time-, region-, or county-specific unobservables not already controlled for by the propensity score. This is the matching method's analogy to fixed effects, and is similar in spirit to Smith and Todd's (2000) claim that matches used to identify the effect of employment programs should be from the same local labor market. Thus, the estimator in (2) becomes

(3a) $\tau_{TT,tr} = E[y_1 \mid T = 1, p(Z), t, r] - E[y_0 \mid T = 0, p(Z), t, r] = E[y_1 - y_0 \mid p(Z), t, r]$

(3b) $\tau_{TT,i} = E[y_1 \mid T = 1, p(Z), i] - E[y_0 \mid T = 0, p(Z), i] = E[y_1 - y_0 \mid p(Z), i]$

where t indexes year, r indexes region, and i indexes county.

Finally, as the usual matching estimators are applicable only to problems of "selection on observables," we amend the matching estimators in (3a), (3b) to remove explicitly certain unobservables by employing a difference-in-differences (DID) matching estimator. Since we have a count of both pollution-intensive *and* non-pollution-intensive plant modifications (closures) for each county-year observation, and the behavior of "clean" plants should not be affected by attainment status, any difference in the modification (closure) rates of "clean" plants across the matched

[8] Typically, nearest-neighbor matching is performed *with replacement*, implying that a given control observation may be matched with multiple treatment observations. Dehejia and Wahba (2002) verify that matching with replacement fares at least as well as matching without replacement, and possibly better.

treatment and control groups is assumed to reflect unobservable county-specific qualities that serve to influence plant modification (closure). Thus, in the spirit of similar estimators used in Eichler and Lechner (2002), Heckman et al. (1997), and List et al. (2003a), we define the DID counterpart to (3a), (3b) as

(4a) $\tau_{DID,tr} = \tau_{TT,tr} - \tau'_{TT,tr}$

(4b) $\tau_{DID,i} = \tau_{TT,i} - \tau'_{TT,i}$

where the prime refers to clean plants and so $\tau'_{TT,tr}$ ($\tau'_{TT,i}$) is the mean difference in the count of modifications (closures) of "clean" plants across the matched treatment and control groups. As the DID estimator only requires

(1'') $\mathrm{E}[y_0 - y'_0 \mid T = 1, Z] = \mathrm{E}[y_0 - y'_0 \mid T = 0, Z] = \mathrm{E}[y_0 - y'_0 \mid Z],\ pr(T = 1 \mid Z) \in (0,1)$

for identification, where y'_0 is the count of "clean" plant modifications (closures), Smith and Todd (2000) conclude that DID matching estimators are more robust.

Upon completing the matching estimation, balancing and specification tests are conducted. Balancing refers to the fact that after conditioning on the propensity score, the distribution of the conditioning variables, Z, should not differ across the treatment and control group in the matched sub-sample. Thus, after matching, we also test for differences in the mean of the Z's. The specification test proposed in Ham et al. (2001) requires testing for mean differences in the lagged outcome across the matched treatment and control groups. In the present context, this test serves two purposes. First, if the lagged outcome differs across the treatment and control groups, then the presence of uncontrolled unobservables may bias the estimated treatment effect. Second, since lagged plant modifications (closures) may affect current plant births due, for example to agglomeration externalities, differences in lagged modifications may have a direct effect on the outcome as well, further biasing the estimator (Henderson, 1997; 2003).

1.2.2. Parametric Approach

For comparison, we also estimate several parametric models. The count of (pollution-intensive) plant modifications (closures) in county i at time t, y_{it}, is assumed to be given by

(5) $y_{it} = \exp(X_{it}\beta)\eta_{it} + \varepsilon_{it}$

where X_{it} is a vector of county attributes, including attainment status, η_{it} captures all unobservable, time-varying attributes of county i and is potentially correlated with some of the variables in X_{it}, and ε_{it} is an error term satisfying $\mathrm{E}[\varepsilon \mid X, \eta] = 0$. Given the inclusion of a constant in X_{it}, E[η] = 1 can be assumed without loss in generality (Mullahy, 1997).

Equation (5) is estimated via the fixed effects (FE) Poisson model of Hausman, Hall and Griliches (1984; hereafter HHG). Resulting estimates of β will be consistent if $E[\eta \mid X] = E[\eta]$; thus, regressors in X may not be correlated with time-specific, county-specific unobservables.[9] If $E[\eta \mid X] \neq E[\eta]$, then another estimator is needed. Mullahy (1997) and Windmeijer and Silva (1997) discuss various instrumental variables (IV) and two-step solutions, given a sufficient number of instruments, denoted by vector W_{it}.

If the source of endogeneity is a dichotomous treatment variable, as is the case with county-level attainment status, a straightforward two-step solution is available. If one envisions plant modification (closure) decisions as dependent upon the latent variable underlying attainment status, consistent estimates of the effect of that latent variable is obtained by replacing the treatment variable in (5) with its predicted linear index.[10] The estimating equation then becomes

$$(5') \qquad y_{it} = \exp(\delta T^*_{it} + X'_{it}\beta')\eta_{it} + \varepsilon_{it}$$

where T^*_{it} is a latent variable, such that if $T^*_{it} > 0$ then a county is out-of-attainment (T_{it} = 1); if $T^*_{it} \leq 0$, then a county is in attainment ($T_{it} = 0$); δ is the parameter of interest; X'_{it} contains the remaining variables in X_{it} excluding attainment status; and β' is the corresponding parameter vector. Consistent estimates can be obtained by replacing T^* with $W\gamma$, where $T^*_{it} = W_{it}\gamma + \upsilon_{it}$. Assuming γ is unknown, it can be estimated via logit. The second-stage standard errors are obtained via bootstrap.

Prior to continuing, it is important to note the major differences between the matching and parametric Poisson estimators. First, matching estimators entail fewer distributional assumptions. Second, matching estimators allow one to use additional endogenous variables that are difficult to incorporate into standard parametric count models: for example, lagged values of the dependent variable. In a parametric framework, one requires valid exclusion restrictions that are uncorrelated with these omitted endogenous variables. Third, matching allows for nonparametric interactions between the covariates in Z in the determination of the outcome of interest (Bratberg et al. 2002). Fourth, matching estimators utilize only a sub-sample of the control group that is most "similar" to the treatment group, whereas parametric models utilize all available observations. This may present problems in matching applications when the treatment group is small. Finally, the matching estimators employed herein yield an estimate of the *treatment effect on the treated* (TT), whereas the parametric approach estimates the expected treatment effect for a random observation.

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[9] HHG estimates are also consistent in the presence of correlation between variables in X and time-specific unobservables that are constant across counties if X includes period-specific dummies.

[10] The usual two-step solution of replacing the dichotomous variable with its predicted probability will not produce consistent estimates of β in a Poisson framework (Windmeijer and Silva, 1997).

1.3. The Use of Count Data

One potential shortcoming to models as specified in Section 1.2, however, is that the data contain only the *count* of pollution-intensive plant modifications and closures; not the *percentage* of plants modifying or closing. More formally, we examine $y = f(X)$, where y is the modification or closure count and X is a vector of county attributes including attainment status, rather than $y/N = g(X)$, where N is the total stock of pollution-intensive plants. Nonetheless, assume that absent regulation: (i) all plants pollute the same amount per unit of output on average, and (ii) the size distribution of plants is roughly constant across counties. Then, because attainment status is related to pollution levels, it follows that being out-of-attainment implies a higher stock of existing pollution-intensive plants, N. Consequently, if we find that nonattainment status reduces the modification (closure) count, y, then nonattainment must also be associated with a lower percentage of modifications (closures), y/N. Alternatively, if we find no impact of attainment status on modifications (closures), then it is possible for nonattainment to have either no effect or a negative effect on the percentage of modifications (closures). Finally, if we find a positive impact of attainment status on modifications (closures), then we obtain no information concerning the effect of nonattainment on the percentage of modifications (closures).

2. Empirical Results

2.1. Modification Rates

2.1.1. Propensity Score Matching Estimates

Table 2 presents the first-stage logit estimates used to form the propensity score. The specification is similar to the first-stage equation used in the two-step HHG model (discussed below) with the inclusion of higher order terms and interactions to facilitate the balancing of the covariates across the matched treatment and control groups (Dehejia and Wahba, 2002).

The first matching algorithm pairs each out-of-attainment county with the in-attainment county from the same year and region with the nearest propensity score. Although there are 176 treatment (nonattainment) observations, only 76 have a possible match.[11] Of the 76 matched pairs, we retain those with propensity scores that differ by less than 1%, 5%, 10%, and 20%. Under these cut-offs, 8, 16, 20, and 27 pairs are obtained. Using the same cut-offs but matching each treatment county to itself at a different point in time yields 9, 11, 18, and 25 matches.[12]

[11] All counties in five regions are in nonattainment in various years. Thus, there is no control observation for these counties to serve as a match.

[12] There are at most 143 possible matches using the within-county matching algorithm as three counties are in nonattainment all eleven years of the sample.

Table 3 presents estimated mean differences between the matched treatment and control groups, along with p-values associated with the null hypothesis that the means are equal, for each of the eight matched sub-samples. Examination of the table yields three important insights. First, within-year, within-region matching algorithm balances the mean of all the covariates (at the $p < 0.05$ level) across the treatment and control groups using the 1% and 5% calipers; the within-county algorithm balances the means using the 1%, 5%, and 10% calipers. This includes the variables specifically controlled for in the first-stage logit, as well as property taxes, the proportion of the population with a high school diploma, and highway expenditures. Thus, these algorithms satisfy the balancing test.

Second, the five algorithms passing the balancing test also pass the specification test proposed in Ham et al. (2001) as lagged pollution-intensive plant modifications is balanced. Finally, of these five algorithms, all yield negative point estimates for the average treatment effect on the treated. Moreover, the three within-county estimates are statistically significant (at the $p < 0.05$ level) despite the sample size. These point estimates imply that in a nonattainment county roughly three pollution-intensive plants per year forego modification due to the county's attainment status and the potential added cost that this status may trigger under NSR (1%: $\tau_{DID,i}$=-3.56, p=0.04; 5%: $\tau_{DID,i}$=-3.09, p=0.04; 10%: $\tau_{DID,i}$=-2.33, p=0.03). Given that the average county experiences 1.09 major (pollution-intensive) modifications per year, the foregone activity suggests that the NSR regulation may have a sizeable deterrent effect.

2.1.2. Parametric Results

For comparison, we also utilize several parametric estimators. Empirical results are displayed in Table 4. Columns (1) and (2) display the results using equation (5), thereby treating attainment status as exogenous. Borrowing from the literature on plant location, the specification in column (1) includes county and time fixed effects, county-level attainment status, and a measure of scale (manufacturing employment). The specification in column (2) adds additional controls for real manufacturing wages, population, and real property taxes. Both specifications indicate a statistically insignificant effect of attainment status on plant modifications, and the point estimates are extremely close to zero.[13]

Because the most likely explanation for the divergence between the HHG and matching estimates is that the parametric estimates are biased due to the presence of unobservables correlated with attainment status and plant modifications, we turn to the two-step model in (5'). Consistency of the estimates relies on finding a valid instrument for attainment status. Finding a valid exclusion restriction is not trivial, since attainment status depends on the lagged level of air quality, which in turn depends on

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[13] As the standard errors from the Poisson model are sensitive to over- or under-dispersion in the data, we also estimate negative binomial models, which relax the restriction of equality of the conditional mean and variance functions. The results, available upon request, differed very little.

lagged manufacturing activity, which in turn may affect current plant behavior due to positive Marshall-Arrow-Romer externalities or negative Jacobs externalities.

In an attempt to circumvent this issue, we follow List et al. (2002) and exploit a natural phenomenon: wind direction. Since county-level attainment status is not based on own emissions, but rather observed local air quality readings, emissions from neighboring counties may influence attainment status due to transboundary effects. Since the jet stream flows from west to east, we use the proportion of contiguous Western neighbors that are out-of-attainment to identify the model.[14] Before examining the two-step results, we note that (Western) neighboring attainment status is a highly significant determinant of own attainment status ($p < 0.01$; see Table 2); thus, we are not concerned about problems associated with weak instruments (Stock et al., 2002). Moreover, the positive coefficient is consonant with our logic that transboundary pollution affects the attainment status of Eastern neighboring counties.

In terms of the actual results – displayed in Columns (3) and (4) of Table 4 – the point estimates are –0.11 and –0.13, respectively, and 90% confidence intervals (obtained via 1000 bootstrap repetitions) both encompass zero.[15] When interpreting the magnitude of the two-step coefficients, one must note that the two-step model does not provide an estimate of the treatment effect *per se* since the coefficient refers to a one-unit increase in latent attainment status. Since the predicted mean of latent attainment status (i.e., the mean of $Z_{it}\hat{\gamma}$) is –2.3, a 2.3 unit increase in latent attainment is required to move the average county from in- to out-of-attainment. Multiplying the two-step coefficient in column (4) yields an estimated treatment effect of roughly –0.30, with a 90% confidence interval of [-0.69, 0.05]. This implies a deterrent effect of roughly 0.3 pollution-intensive plant modifications per annum from being out-of-attainment.

Given that the discrepancy between the parametric and matching estimates cannot be explained by the treatment of attainment status as exogenous, at least three possible explanations remain: (i) the matching algorithm, by utilizing only a sub-sample of the control group, produces an estimator that is not subject to "outliers;" (ii) the parametric assumptions of the Poisson model are invalid; or, (iii) the matching algorithm yields an estimate of the average treatment effect on the treated, whereas the one-step parametric models estimate the treatment effect on a random observation from the population and the two-step HHG model estimates the Local Average Treatment Effect (LATE), the effect of nonattainment on those counties whose nonattainment status is driven by the instrument (i.e., "compliers"). If the effect of nonattainment varies across the "compliers" and those counties in nonattainment regardless of the value of the instrument (i.e., "always-takers"), then the two-step model will not identify the average treatment effect on the treated (Angrist et al., 1996).

Unfortunately, we cannot easily discern among these explanations. There is no formal specification test for the Poisson model (other than testing for over- or under-

[14] For counties located on the NYS border, we obtained data on the attainment status of neighboring counties in other states to form the appropriate instrument.

[15] The estimate in column (3) is statistically different from zero using a *one-sided* alternative at the $p < 0.12$ confidence level; the estimate in column (4) at the $p < 0.07$ confidence level.

dispersion), the matched samples are too small to estimate the parametric models in a meaningful fashion, and there is no formal test of the assumption needed for the two-step approach to identify the average effect as opposed to the LATE. One additional parameter we can estimate using the method of propensity score matching for comparison to the above results is the population average treatment effect. If we label "in-attainment" as the treatment and re-do the matching by pairing each in-attainment county with an out-of-attainment county, we can obtain estimates of the average treatment effect *on the untreated*. The weighted average of the treatment effect on the treated and untreated constitutes the overall average treatment effect. Performing this exercise using the within-county algorithm with 1%, 5%, and 10% calipers yields estimates of the average treatment effect of –1.64, -1.25, and –0.87, respectively.[16] Although closer to the two-step HHG estimates reported in Table 4, a sizeable discrepancy persists.

2.2. Closure Rates

In this section we summarize empirical estimates using the spatial distribution of the 2269 plant closures across New York State from 1980-1990. Results in this section have stand-alone importance as they provide an explicit link to the empirical work that has shown uneven regulation leads to a lengthening of the time before power plants are retired (see, for example, Nelson et al., 1993; Maloney and Brady, 1988; Gollop and Roberts, 1983; Stewart, 1981). The intuition behind this finding is that uneven regulation—commonly termed "new source bias"—confers a competitive advantage to existing plants, as they face lower compliance expenditures, *ceteris paribus*. While this finding is robust in the electric utility industry, an important extension is to examine whether these results apply to other stationary sources. This is exactly what this section offers.

2.2.1. Propensity Score Matching Estimates

Table 5 presents estimated mean differences between the matched treatment and control groups for four measures of plant closures, along with p-values associated with the null hypothesis that the means are equal, for each of the eight matched sub-samples presented in Table 3. The four types of closures are: (i) complete closures, (ii) closures due to bankruptcy, (iii) closures accompanied by the plant relocating to another county in New York State, and (iv) closures accompanied by the plant relocating (partially or fully) out of state. As the matching algorithms are identical to those used in Table 3, we focus on the within-year, within-region matching algorithm using the 1% and 5% calipers, and the within-county matching algorithm using the 1%, 5%, and 10% calipers.

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[16] The fact that the average treatment effect is smaller in absolute value than the average treatment effect on the treated implies that the cost of nonattainment (in terms of foregone plant modifications) is greater for those counties actually out-of-attainment.

Of the five algorithms satisfying the balancing test, all yield positive point estimates for the average treatment effect on the treated for each of the four types of closures. However, the estimated effect of NSR on the each of the first three types of closures, which together constitute nearly 80% of all pollution-intensive plant closures, is insignificant. The only statistically significant results (at the $p < 0.05$ level) are for the three within-county estimates for closures accompanied by a relocation out of state. The point estimates for these closures imply that approximately one pollution-intensive plant per county per year closes and relocates out of state due to the fact that the plant is located in a nonattainment county in New York State (1%: $\tau_{DID,i}$=1.22, p=0.00; 5%: $\tau_{DID,i}$=1.00, p=0.01; 10%: $\tau_{DID,i}$=0.72, p=0.01). As the average county experiences 0.17 (pollution-intensive) closures with an accompanied (partial or complete) relocation out of state per year, this does represent a non-negligible effect of the NSR regulation. While the results are mixed, overall NSR does not appear to have an economically meaningful impact on the closure of dirty plants.[17]

2.2.2 Parametric Results

For completeness, we examine closures using the parametric estimators. Empirical results are displayed in Table 6. Columns (1) and (2) display the results treating attainment status as exogenous; Columns (3) and (4) present the two-step estimates using the attainment status of Western neighbors as an instrument.

In terms of the models treating attainment status as exogenous, all of the point estimates are positive, suggesting that NSR may, in fact, increase the closure of dirty plants. However, all of the estimates are statistically insignificant at the $p < 0.10$ level except for the models analyzing closures with a bankruptcy filing. While this is consonant with the viewpoint of NSR advocates, bankruptcy filings constitute less than 10% of all pollution-intensive plant closures in the sample. Turning to the two-step HHG model results, the point estimates are negative for three of the four closure measures—the closures accompanied with a partial or complete move out of state is the exception. Further, the 90% confidence intervals that were obtained via 1000 bootstrap repetitions always encompass zero. The exogenous and two-step HHG results do not support the position that NSR regulation facilitates the turnover of dirty capital.[18]

Overall, our empirical evidence on the distribution of closure rates does not match the results reported in the literature on capital turnover among electric utilities. There are several potential reasons for this difference: (i) aggregate data for pollution-intensive manufacturing may mask important inter-industry and inter-plant

[17] Pooling all closures together and examining the combined effect of nonattainment on aggregate closures yields a statistically significant estimate only using the within-county matching algorithm and a 1% caliper ($\tau_{DID,i}$=1.91, p=0.04).

[18] Pooling all closures together and examining the combined effect of nonattainment on aggregate closures does not alter the findings. For the model in column (3), the estimated coefficient on attainment status is –0.11 (90% confidence interval: [-0.28, 0.08]); for the model in column (4), the estimated coefficient on attainment status is –0.09 (90% confidence interval: [-0.29, 0.08]).

heterogeneity, (ii) plant closure in the manufacturing sector may be a distinctively different decision process compared to capital turnover among power plants, and (iii) the extant empirical evidence for power plants may be due to stagnation in the average heat rate and technical change for electric utilities (see Joskow, 1987).

3. Concluding Remarks

Grandfathering of existing firms has played an important role in regulatory programs in the US in the past several decades. Despite the widespread use of differentiated regulations, little attention has been paid to their effects on plant-level modification decisions in the regulated sector. This particular area of study is acutely important for health and safety regulation, as modest plant-level modifications may yield large declines in the regulated externality.

As a case study into this issue, we examine whether the NSR provision in the Clean Air Act had important effects on plant-level modification decisions in the manufacturing sector. To date, data limitations have precluded any systematic effort to model the effects of NSR on the behavior of existing plants, rendering anecdotes as the sole pieces of evidence concerning the potentially deleterious effects of NSR. Our study offers such an empirical analysis. Examining more than 2500 plant-level modification decisions, we find that a county's attainment status and the potential added cost that this status may trigger under NSR had deleterious effects on plant-level modification decisions. Further, our examination of 2200 closure decisions did not find any consistent evidence that NSR affected the rate of closure of existing dirty plants.

In sum, then, the finding that NSR deterred plant modifications that could have led to reduced emissions, in combination with the nonexistent effects of NSR on pollution-intensive plant closures, suggests the possibility that in the short run the NSR stipulation for existing manufacturing plants has led to *more*, rather than less, pollution.

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Table 1 Description of Variables

Variable	Full Sample	In-Attnmt.	Non-Attnmt.	Definition and Source
Pollution-Intensive Plant Modifications Per County Per Year	1.09 (2.31)	0.81† (1.73)	1.89† (3.36)	Actual count of plant modifications from 1980-1990 labelled as having production activities that is pollution-intensive. Industrial Migration File. New York State Dept. of Economic Development (DED).
Non-Pollution-Intensive Plant Modifications Per County Per Year	2.62 (4.93)	1.91† (3.20)	4.66† (7.71)	Actual count of plant modifications from 1980-1990 labelled as having production activities that is non-pollution-intensive. Industrial Migration File. New York State Dept. of Economic Development (DED).
Attainment Status	0.26 (0.44)	--	--	Intensity of county-level pollution regulations. Dichotomous variable = 1 if county is out-of-attainment of federal standards for ozone, 0 otherwise. Federal register Title 40 CFR Part 81.305.
Employment (100,000s)	1.50 (3.57)	0.08† (1.45)	3.40† (6.22)	Total employment in manufacturing. *County Business Patterns*.
Wage (1000s)	17.01 (4.1.2)	17.34† (3.84)	16.08† (4.74)	Total annual manufacturing payroll divided by the number of employees by county, adjusted for inflation. *County Business Patterns*.
Firms	11.71 (5.39)	12.23† (5.49)	10.22† (4.84)	Total number of manufacturing firms. *County Business Patterns.*
Population (100,000s)	2.87 (4.78)	1.82† (3.40)	5.87† (6.60)	County population. *Current Population Reports*. U.S. Bureau of Census.
Property tax (1000s)	0.56 (0.24)	0.57 (0.27)	0.54 (0.17)	Real property tax collected per capita. *Census of Governments*.
Per Capita Income (1000s)	13.50 (4.76)	13.58 (4.30)	13.24 (5.88)	Real per capita income. *County Business Patterns.*
Highway Expenditures	0.17 (0.09)	0.18† (0.10)	0.15† (0.08)	Total highway expenditures. *County Business Patterns.*
High School Graduates	70.05 (6.81)	70.91† (6.75)	67.58† (6.35)	Percent of population with high school diploma. *Current Population Reports.*

Notes:
1. Data are for the 62 New York counties from 1980-1990. N = 682 (176 out-of-attainment).
2. Numbers represent sample means. Standard deviations are in parentheses.
3. † indicates difference in means across in- and out-of-attainment samples is statistically significant at $p < 0.05$ level using a two-sided t-test.

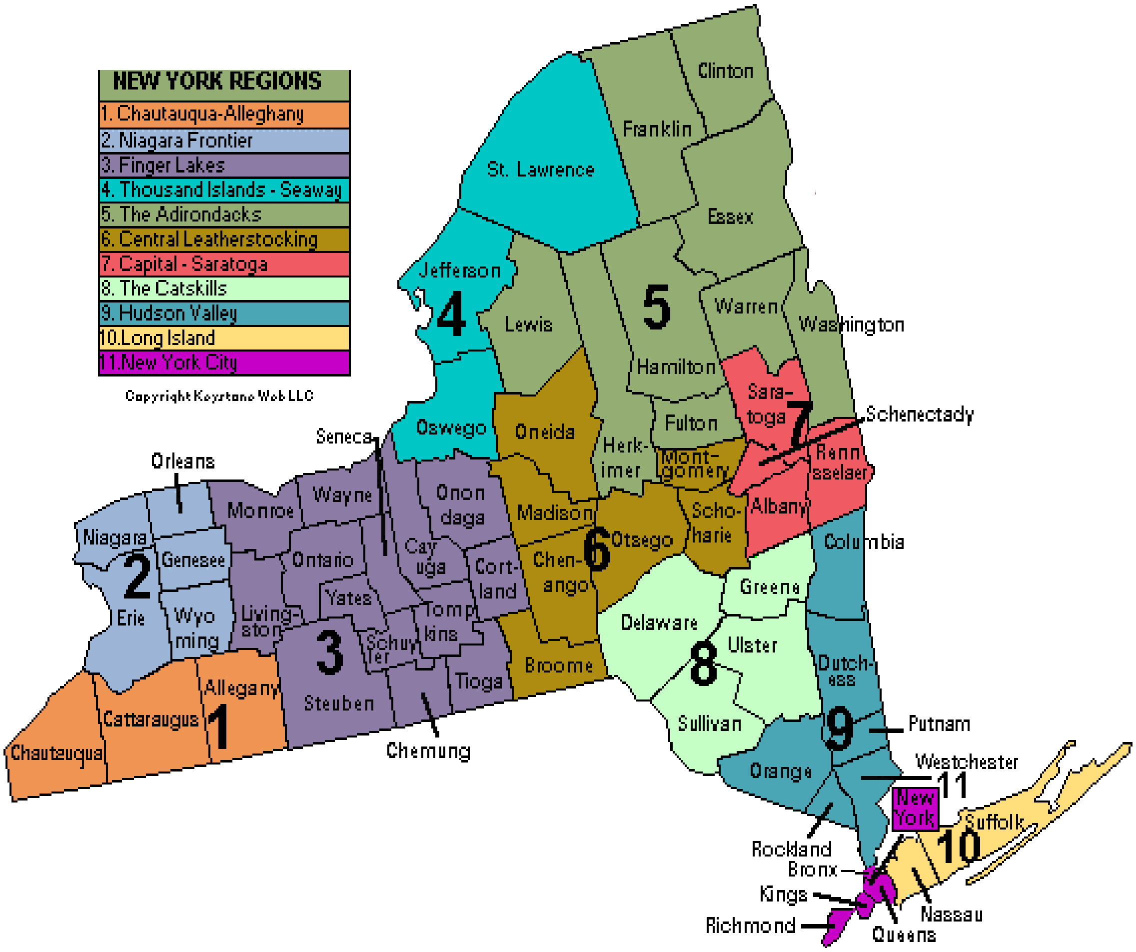

Figure 1. Regional Breakdown, New York State.

Table 2 First-Stage Logit Estimates of the Determinants of Attainment Status

Independent Variable	Coefficient (SE) (1)		Coefficient (SE) (2)	
Neighboring Attnmt. Status	2.85*	(0.33)	—	
Man. Employment	1.99E-06	(1.29E-06)	—	
Property Taxes	-1.85E-03*	(8.75E-04)	—	
Man. Wages	-3.95E-06	(7.08E-05)	3.63E-03	(2.55E-03)
Man. Wages^2			-2.23E-07	(1.41E-07)
Man. Wages^3			4.27E-12	(2.74E-12)
Man. Plants			1.40*	(0.58)
Man. Plants^2			-0.09*	(0.05)
Man. Plants^3			1.84E-03*	(1.04E-03)
Population	1.62E-06*	(5.09E-07)	-1.85E-06	(6.28E-06)
Population^2			7.37E-12	(6.12E-12)
Population^3			-3.14E-18*	(1.82E-18)
Per Capita Income			4.73E-03*	(1.25E-03)
Per Capita Income^2			-1.86E-07*	(9.64E-08)
Per Capita Income^3			2.63E-12*	(1.40E-12)
Man. Wages * Man. Plants			-9.57E-06	(3.20E-05)
Man. Wages * Population			1.08E-09*	(4.53E-10)
Man. Wages * Per Capita Income			-1.61E-08	(6.61E-08)
Man. Plants * Population			-8.61E-07*	(3.54E-07)
Man. Plants * Per Capita Income			1.67E-05	(3.04E-05)
Population * Per Capita Income			-8.88E-10*	(4.10E-10)
Time Effects	Yes		Yes	
Log-likelihood	-180.7		-145.8	
Pseudo R^2	0.54		0.63	
N	682		682	

Notes:

1. Dependent variable is equal to one if county is out-of-attainment of federal ozone standards during the year, zero otherwise. Neighboring attainment status is the percentage of Western contiguous neighbors that are out of attainment.
2. Standard errors are in parentheses beside the coefficient estimates and are adjusted for clustering within counties. * indicates significant at the 10% level using a two-sided alternative.
3. Model (1) is used in the two-step FE Poisson estimation. Model (2) is used to generate the propensity score estimates.
4. Time effects jointly significant at the 1% level.

Table 3a Propensity Score Estimates of Attainment Status Effect on Plant Expansions: Within Region and Year Matching

Independent Variable	Maximum Difference (0.01)	(0.05)	(0.10)	(0.20)
Propensity Score	0.00 (p=0.98)	0.00 (p=0.98)	0.01 (p=0.90)	0.04 (p=0.62)
Dirty Plant Expansions ($\tau_{TT,\bullet}$)	**0.25 (p=0.72)**	**-0.44 (p=0.70)**	**-0.55 (p=0.55)**	**-0.52 (p=0.45)**
Clean Plant Expansions	1.25 (p=0.34)	0.13 (p=0.92)	-0.10 (0.92)	-0.04 (p=0.96)
Net Plant Expansions ($\tau_{DID,\bullet}$)	**-1.00 (p=0.21)**	**-0.56 (p=0.22)**	**-0.45 (p=0.26)**	**-0.48 (p=0.15)**
Lagged Dirty Plant Expansions (1 year)	0.71 (p=0.53)	-0.29 (p=0.79)	-0.24 (p=0.79)	-0.39 (p=0.56)
Man. Wages (1000s)	-0.06 (p=0.98)	-0.91 (p=0.44)	-0.88 (p=0.36)	-0.72 (p=0.33)
Man. Employment (1000s)	29.94 (p=0.63)	4.05 (p=0.93)	-0.37 (p=0.99)	1.16 (p=0.97)
Man. Plants	-0.79 (p=0.82)	-2.37 (p=0.26)	-2.21 (p=0.23)	-1.97 (p=0.24)
Population (1000s)	59.49 (p=0.55)	4.61 (p=0.96)	0.31 (p=1.00)	2.43 (p=0.97)
Per Capita Income (1000s)	-0.66 (p=0.79)	-0.61 (p=0.66)	-0.55 (p=0.62)	-0.40 (p=0.63)
Property Tax	-389.13 (p=0.06)	-186.81 (p=0.10)	-148.00 (p=0.10)	-102.04 (p=0.13)
High School Graduates (%)	-3.61 (p=0.32)	-3.39 (p=0.12)	-4.96 (p=0.01)	-5.80 (p=0.00)
Highway Expenditure	-0.16 (p=0.09)	-0.07 (p=0.16)	-0.05 (p=0.24)	-0.02 (p=0.44)
Number of Matched Pairs	8	16	20	27
Number of Unique Controls	8	15	17	18

Notes:

1. Figures represent mean difference between treatment counties (out-of-attainment) and control counties (in-attainment). P-values in parentheses are for the tests that the mean difference across the treatment and controls groups are equal.
2. "Dirty" plants are those defined as pollution intensive (see text); "clean" are all remaining manufacturing plants.
3. "Unique controls" reports the number of control counties that are matched with at least one treatment county.

Table 3b Propensity Score Estimates of Attainment Status Effect on Plant Expansions: Within County Matching

Independent Variable	**Maximum Difference** (0.01)	(0.05)	(0.10)	(0.20)
Propensity Score	0.00 (p=1.00)	0.01 (p=0.97)	0.04 (p=0.75)	0.06 (p=0.60)
Dirty Plant Expansions ($\tau_{TT,\bullet}$)	**-1.89 (p=0.53)**	**-1.73 (p=0.50)**	**-1.83 (p=0.31)**	**0.32 (p=0.84)**
Clean Plant Expansions	1.67 (p=0.69)	1.36 (p=0.70)	0.50 (p=0.84)	2.84 (p=0.42)
Net Plant Expansions ($\tau_{DID,\bullet}$)	**-3.56 (p=0.04)**	**-3.09 (p=0.04)**	**-2.33 (p=0.03)**	**-2.52 (p=0.30)**
Lagged Dirty Plant Expansions (1 year)	0.25 (p=0.94)	0.67 (p=0.81)	0.86 (p=0.62)	1.71 (p=0.31)
Man. Wages (1000s)	0.54 (p=0.60)	-0.01 (p=0.99)	-0.76 (p=0.37)	-1.56 (p=0.04)
Man. Employment (1000s)	3.11 (p=0.98)	2.53 (p=0.98)	-4.33 (p=0.95)	-18.47 (p=0.79)
Man. Plants	0.59 (p=0.70)	0.48 (p=0.74)	0.29 (p=0.83)	0.22 (p=0.89)
Population (1000s)	-0.65 (p=1.00)	-0.31 (p=1.00)	0.19 (p=1.00)	-3.89 (p=0.98)
Per Capita Income (1000s)	0.33 (p=0.84)	-0.20 (p=0.90)	-0.82 (p=0.44)	-1.60 (p=0.09)
Property Tax	1.22 (p=0.98)	1.00 (p=0.98)	-9.39 (p=0.79)	-13.64 (p=0.78)
High School Graduates (%)	-1.09 (p=0.70)	-0.89 (p=0.71)	-1.16 (p=0.50)	-1.18 (p=0.45)
Highway Expenditure	-0.00 (p=0.97)	-0.00 (p=0.92)	-0.01 (p=0.67)	-0.01 (p=0.37)
Number of Matched Pairs	9	11	18	25
Number of Unique Controls	6	7	10	12

Notes: See Table 3a.

Table 4 Parametric Estimates of the Determinants of County-Level Plant Expansions

	Fixed Effects Poisson			
	HHG		Two-Step HHG	
Independent Variable	**(1)**	**(2)**	**(3)**	**(4)**
Non-Attainment	1.21E-03 (0.13)	-0.02 (0.13)	-0.11 [-0.28,0.04]	-0.13 [-0.30,0.02]
ln(Employment)	-1.33 (1.03)	0.44 (1.39)	-1.70 [-3.61,-0.61]	0.34 [-2.79,3.44]
ln(Wage)	---	-4.35* (1.36)	---	-4.31* [-7.66,1.03]
ln(Population)	---	-0.86 (3.12)	---	-2.09 [-9.55,5.42]
ln(Prop. Tax)	---	-0.97 (1.34)	---	-1.57 [-4.16,0.75]
Period Effects	Yes	Yes	Yes	Yes
County Effects	Yes	Yes	Yes	Yes
Log-likelihood	-582.2	-576.2	-580.7	-574.2
N	682	682	682	682

Notes:
1. Dependent variable is the count of annual pollution-intensive manufacturing plant expansions from 1980-1990.
2. Non-Attainment equals 1 if county is out-of-attainment of federal ozone standard, 0 otherwise.
3. Models (1) and (2) treat attainment status as exogenous (estimated using HHG (1984) quasi-maximum likelihood (QML)); Models (3) and (4) treat attainment status as endogenous (estimated using a two-step QML estimation procedure). The proportion of Western neighboring counties that are out-of-attainment is used as the instrument (see Table 2).
4. Standard errors are in parentheses beneath coefficient estimates; 90% bootstrap confidence intervals – based on 1000 repetitions – in brackets. *indicates significant at the 10% level using a two-sided alternative.
5. ln indicates the natural logarithm of the variable.
6. Time effects are jointly significant in models (1) and (2) at the $p < 0.01$ level.

Table 5a Propensity Score Estimates of Attainment Status Effect on Plant Closures: Within Year and Region Matching

Independent Variable	Maximum Difference (0.01)	(0.05)	(0.10)	(0.20)
Complete Closures				
Dirty Plant Closures ($\tau_{TT,\bullet}$)	**0.50 (p=0.02)**	**0.06 (p=0.82)**	**0.05 (p=0.83)**	**0.00 (p=1.00)**
Clean Plant Closures	-0.13 (p=0.76)	0.56 (p=0.27)	0.45 (p=0.27)	0.30 (p=0.34)
Net Plant Closures ($\tau_{DID,\bullet}$)	**0.63 (p=0.19)**	**-0.50 (p=0.29)**	**-0.40 (p=0.30)**	**-0.30 (p=0.30)**
Lagged Dirty Plant Closures (1 year)	-0.14 (p=0.69)	0.00 (p=1.00)	0.06 (p=0.86)	0.09 (p=0.74)
Closures with Bankruptcy				
Dirty Plant Closures ($\tau_{TT,\bullet}$)	**-0.13 (p=0.33)**	**0.00 (p=1.00)**	**0.00 (p=1.00)**	**0.00 (p=1.00)**
Clean Plant Closures	0.00 (p=1.00)	0.00 (p=1.00)	-0.05 (p=0.32)	-0.07 (p=0.16)
Net Plant Closures ($\tau_{DID,\bullet}$)	**-0.13 (p=0.33)**	**0.00 (p=1.00)**	**0.05 (p=0.57)**	**0.07 (p=0.33)**
Lagged Dirty Plant Closures (1 year)	0.00 (p=1.00)	0.00 (p=1.00)	0.00 (p=1.00)	0.09 (p=0.15)
Closures Accompanied with Move In State				
Dirty Plant Closures ($\tau_{TT,\bullet}$)	**0.13 (p=0.33)**	**0.06 (p=0.33)**	**0.05 (p=0.32)**	**0.04 (p=0.32)**
Clean Plant Closures	0.00 (p=1.00)	-0.06 (p=0.33)	-0.10 (p=0.15)	-0.07 (p=0.31)
Net Plant Closures ($\tau_{DID,\bullet}$)	**0.13 (p=0.33)**	**0.13 (p=0.17)**	**0.15 (p=0.09)**	**0.11 (p=0.18)**
Lagged Dirty Plant Closures (1 year)	0.00 (p=1.00)	0.00 (p=1.00)	0.00 (p=1.00)	0.04 (p=0.32)
Closures Accompanied with Partial or Complete Move Out of State				
Dirty Plant Closures ($\tau_{TT,\bullet}$)	**0.13 (p=0.33)**	**0.06 (p=0.33)**	**0.05 (p=0.32)**	**0.04 (p=0.32)**
Clean Plant Closures	-0.13 (p=0.66)	0.00 (p=1.00)	0.05 (p=0.76)	0.07 (p=0.55)
Net Plant Closures ($\tau_{DID,\bullet}$)	**0.25 (p=0.44)**	**0.06 (p=0.76)**	**0.00 (p=1.00)**	**-0.04 (p=0.78)**
Lagged Dirty Plant Closures (1 year)	-0.14 (p=0.34)	0.00 (p=1.00)	0.00 (p=1.00)	0.00 (p=1.00)

Notes: See Table 3a. Sample sizes are identical to Table 3a.

Table 5b Propensity Score Estimates of Attainment Status Effect on Plant Closures: Within County Matching

Independent Variable	Maximum Difference (0.01)	(0.05)	(0.10)	(0.20)
Complete Closures				
Dirty Plant Closures ($\tau_{TT,\bullet}$)	**1.44 (p=0.30)**	**1.09 (p=0.34)**	**0.72 (p=0.32)**	**0.80 (p=0.17)**
Clean Plant Closures	0.67 (p=0.59)	0.55 (p=0.60)	0.72 (p=0.32)	2.12 (p=0.09)
Net Plant Closures ($\tau_{DID,\bullet}$)	**0.78 (p=0.19)**	**0.55 (p=0.30)**	**0.00 (p=1.00)**	**-1.32 (p=0.19)**
Lagged Dirty Plant Closures (1 year)	0.88 (p=0.49)	0.98 (p=0.36)	0.87 (p=0.20)	0.97 (p=0.08)
Closures with Bankruptcy				
Dirty Plant Closures ($\tau_{TT,\bullet}$)	**0.11 (p=0.33)**	**0.09 (p=0.33)**	**0.17 (p=0.07)**	**0.16 (p=0.04)**
Clean Plant Closures	0.00 (p=1.00)	0.00 (p=1.00)	0.06 (p=0.56)	0.12 (p=0.30)
Net Plant Closures ($\tau_{DID,\bullet}$)	**0.11 (p=0.33)**	**0.09 (p=0.33)**	**0.22 (p=0.05)**	**0.04 (p=0.71)**
Lagged Dirty Plant Closures (1 year)	0.00 (p=1.00)	0.00 (p=1.00)	0.11 (p=0.41)	-0.38 (p=0.00)
Closures Accompanied with Move In State				
Dirty Plant Closures ($\tau_{TT,\bullet}$)	**0.11 (p=0.33)**	**0.09 (p=0.33)**	**0.06 (p=0.32)**	**0.12 (p=0.25)**
Clean Plant Closures	-0.22 (p=0.15)	-0.18 (p=0.15)	-0.11 (p=0.15)	0.08 (p=0.39)
Net Plant Closures ($\tau_{DID,\bullet}$)	**0.33 (p=0.09)**	**0.27 (p=0.09)**	**0.17 (p=0.09)**	**0.04 (p=0.74)**
Lagged Dirty Plant Closures (1 year)	-0.13 (p=0.23)	-0.10 (p=0.36)	-0.06 (p=0.36)	0.06 (p=0.61)
Closures Accompanied with Partial or Complete Move Out of State				
Dirty Plant Closures ($\tau_{TT,\bullet}$)	**0.33 (p=0.18)**	**0.27 (p=0.18)**	**0.11 (p=0.41)**	**-0.44 (p=0.23)**
Clean Plant Closures	-0.89 (p=0.04)	-0.73 (p=0.06)	-0.61 (p=0.03)	-0.40 (p=0.43)
Net Plant Closures ($\tau_{DID,\bullet}$)	**1.22 (p=0.00)**	**1.00 (p=0.01)**	**0.72 (p=0.01)**	**-0.04 (p=0.92)**
Lagged Dirty Plant Closures (1 year)	0.00 (p=1.00)	0.00 (p=1.00)	-0.05 (p=0.72)	0.18 (p=0.41)

Notes: See Table 3a. Sample sizes are identical to Table 3b.

Table 6 Parametric Estimates of the Determinants of County-Level Plant Closures

	Fixed Effects Poisson			
Independent	**HHG**		**Two-Step HHG**	
Variable	**(1)**	**(2)**	**(3)**	**(4)**
Complete Closures				
Non-Attainment	0.12	0.14	-0.16	-0.13
	(0.21)	(0.21)	[-0.29,0.12]	[-0.31,0.13]
Period Effects	Yes	Yes	Yes	Yes
County Effects	Yes	Yes	Yes	Yes
Log-likelihood	-298.14	-296.75	-297.47	-296.38
Closures with Bankruptcy				
Non-Attainment	1.38*	1.31*	-0.27	-0.29
	(0.67)	(0.66)	[-1.97,0.03]	[-2.50,0.15]
Period Effects	Yes	Yes	Yes	Yes
County Effects	Yes	Yes	Yes	Yes
Log-likelihood	-66.08	-62.88	-67.94	-64.52
Closures Accompanied with Move In State				
Non-Attainment	0.20	0.51	-0.19	-0.10
	(0.35)	(0.41)	[-0.63,0.34]	[-0.63,0.54]
Period Effects	Yes	Yes	Yes	Yes
County Effects	Yes	Yes	Yes	Yes
Log-likelihood	-133.81	-130.60	-123.33	-121.62
Closures Accompanied with Partial or Complete Move Out of State				
Non-Attainment	0.13	0.29	0.11	0.09
	(0.35)	(0.36)	[-0.45,0.22]	[-0.52,0.24]
Period Effects	Yes	Yes	Yes	Yes
County Effects	Yes	Yes	Yes	Yes
Log-likelihood	-152.15	-149.65	-152.09	-149.89

Notes:
1. Specifications (1) and (3) include log manufacturing employment as an additional control. Specifications (2) and (4) include log manufacturing employment, log manufacturing wage, log population, and log property taxes as additional controls. $N = 682$ in each model.
2. See Table 4.