Self-Policing Statutes: Do They Reduce Pollution and Save Regulatory Costs?[1]

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Abstract

State-level statutes provide firms that engage in environmental self-audits, and that self-report their environmental violations, a variety of regulatory rewards, including "immunity" from penalties and "privilege" for information contained in self-audits. This paper studies a panel of State-level industries from 1989 through 2003, to determine the effects of the different types of statutes on toxic pollution and government inspections. We find that, by encouraging self-auditing, privilege protections tend to reduce pollution and government enforcement activity; however, sweeping immunity protections, by reducing firms' pollution prevention incentives, raise toxic pollution and government inspection oversight.

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1. INTRODUCTION

Recent changes in environmental law enforcement encourage polluters to self-report their violations to government authorities. Across U.S. States, self-reporting inducements vary from promises of modest reductions in sanctions to complete immunity from sanctions and privilege protections for information uncovered in a firm's environmental self-audit. Environmental groups argue that many of these protections amount to a free pass for polluters that negates incentives for firms to avoid pollution violations and requires increased government oversight of firms' environmental practices (EPA, 2000). Proponents argue instead that these protections are necessary for firms to audit their own environmental performance, audits that in turn yield environmental dividends in the form of quick detection and remediation of pollution violations and potentially the identification and avoidance of pollution outbreaks before they occur (Weaver, Martineau, and Stagg, 1997). Moreover, because self-auditing firms can uncover and self-report pollution violations, enforcement of environmental laws can be achieved with less government investment in oversight and monitoring (Kaplow and Shavell, 1994; Malik, 1993).

These two perspectives offer competing empirical predictions, one that self-policing statutes raise pollution and government environmental monitoring activity, and the other that they lower them. The objective of this paper is to test these predictions by distinguishing between cross-state differences in self-policing policies in a panel of State-level industries. We estimate two equations, one for total toxic emissions and the other for the number of government environmental inspections, both aggregated across facilities to the level of State-specific industries. We find some merit in the arguments of both environmentalists and proponents of self-policing protections. Some protections, by promoting environmental self-auditing, are found to lower levels of toxic pollution even though they also prompt lower rates of government environmental monitoring, while others deplete firms' pollution avoidance incentives to such an extent that they raise pollution and prompt compensatory increases in government oversight.

Despite the controversy surrounding self-policing policies, and a burgeoning theoretical literature on the subject,[2] surprisingly little empirical work has studied their impact. A notable exception is a key paper by Stafford (2005), who estimates the impact of self-policing policies on the probabilities of facility-level inspection and violation using a panel of Resource Conservation and Recovery Act (RCRA) data. A number of crucial differences between our analysis and Stafford's (2005) motivate our work. First, Stafford (2005) controls for overall State-level inspections in estimating her facility-level inspection equation. Hence, she implicitly controls for the effects of self-policing policies that are our primary focus, namely, impacts of self-policing statutes on government inspection policy. To capture State-specific inspection policy as targeted at different industries, we use data at a State-specific industry level. Second, we study a more direct measure of environmental performance: emissions of regulated toxic air pollutants, rather than the occurrence of a RCRA violation. Although RCRA violations may have a relationship to ultimate toxic emissions, it is not clear-cut. Many violations, including those that concern reporting and record-keeping, are not directly related to emissions. Those that do concern practices that affect emissions are not weighted in Stafford's (2005) violation measure. Rather, this measure is a zero-one variable that equals one if any violation occurs and does not capture effects of multiple or more serious violations. Hence, self-policing policies may yield less frequent technical violations of RCRA, even though they lead to increased toxic emissions. Third, beyond our different data and longer study period are a number of key differences in estimation method, including (for example) our accounting for fixed individual and time effects.[3]

Other related papers include Pfaff and Sanchirico (2004), who compare and contrast self-disclosed and government-detected violations; Stretsky and Gabriel (2005) and Short and Toffel (2005), who estimate an equation to explain the probability of self-disclosure; Helland (1998), who estimates a joint model explaining facility-level self-reporting and inspections; and Stafford (2006), who estimates the impact of self-policing policies on the probability of self-disclosure (generally positive). However, none of this excellent work seeks to identify the effect of self-policing policies on pollution and government inspection activity, our objective.

To frame the empirical issues addressed in this paper, we begin with a conceptual discussion of policy trade-offs that identifies opposing influences of government self-policing policies on average harm (our theoretical proxy for emissions) and government inspections. Based on conjectures about which influences dominate, we posit three Hypotheses on the effects of self-policing policies, which we proceed to test in our empirical work (Sections 3-4 below).

2. HYPOTHESES

Properly designed enforcement regimes that elicit self-reporting enjoy a number of potential efficiency advantages. They can yield direct enforcement economies (Kaplow and Shavell, 1994; Malik, 1993), indirect enforcement economies (such as saving on costs of imprisonment, Kaplow and Shavell, 1994), more frequent remediation / cleanup (Innes, 1999a), better tailoring of penalties to heterogeneous violators (Innes, 2000), and savings of wasteful avoidance expenditures (Innes, 2001a). To obtain these benefits of self-reporting, firms must generally adopt costly environmental self-auditing programs that not only reveal pollution violations, but enable quick remediation and potentially the prevention of accidents that would otherwise occur. Such self-audits also provide much cleaner and clearer documentation of a firm's environmental practices. Legal scholars have argued that this documentation can provide a roadmap for regulatory enforcement that makes prosecution of violations much easier (Hawks, 1998).

To enable self-reporting, by encouraging self-auditing, State laws variously provide two types of protections. The first is a reduction in sanctions to self-reporters vis-à-vis violators who are discovered by government inspectors. Theory generally argues for self-reporting sanctions equal to the expected non-reporter sanction, thereby motivating firms to self-report without sacrificing incentives for the prevention of accidents and violations. Accounting for the costs of self-audit programs, however, firms must be offered somewhat lower self-reporting sanctions so that they enjoy strictly positive benefits of self-reporting that can compensate for costs of self-auditing (Pfaff and Sanchirico, 2000; Mishra, et al., 1997; Innes, 2001b). Some State statutes provide self-reporters with reductions in gravity-based penalties that may or may not be in line with those advanced by economic theorists;[4] others provide self-reporters with complete immunity from sanction.

The second type of protection afforded to self-reporters is "privilege." Many States protect the information contained in self-audits and self-reports from regulatory use beyond the narrow confines of the self-reported violation.[5] Privilege can deny regulators the enforcement economies made possible by self-audit documentation. However, privilege can also encourage firms to adopt self-auditing programs.

Both forms of protection have effects on deterrence (firms' incentives to prevent violations) and firms' adoption of self-audit programs. These two incentive considerations in turn affect the two outcomes of interest in this paper, government enforcement effort and firms' environmental performance.[6]

First consider the effects of privilege. The social benefit of privilege is that it can elicit more self-auditing by protecting firms from the government's use of self-audits for prosecution. By eliciting more self-auditing, privilege can lower average accident harms due to more rapid detection of pollution accidents and pro-active management that avoids pollution outbreaks in the first instance. However, there are opposing costs. Privilege limits the government's ability to use available information about a firm's environmental performance in enforcement. As a result, privilege can limit the sanctions that the government can impose on a firm when pollution accidents occur. This reduces deterrence, which in turn raises average harms from self-auditors' accidents. Effects of privilege on average harm are thus unclear analytically, even controlling for government inspections: average harm is lowered due to more self-auditors and can be raised due to reduced deterrence.

Effects on government inspections are also unclear. Reduced deterrence favors increased government inspection effort as the government compensates for its limited ability to impose sanctions by more intensively monitoring and regulating firms' accident prevention strategies ("care"). However, privilege reduces the effectiveness of government inspections in achieving either accident sanctions or sanctions for insufficient care; this motivates less monitoring, a substitution of self-auditing for government inspections. We conjecture that the self-audit promotion (harm reduction) and substitution effects of privilege dominate in practice:

Hypothesis 1. Privilege lowers both emissions and government inspections.

Consider next the consequences of complete immunity. Because immunity can be obtained only when firms self-report, and self-reporting is made possible by self-audits, immunity promotes the adoption of self-audit programs. However, complete immunity exempts self-reporters from accident sanctions, so that effective sanctions are reduced to only costs of cleanup and correction. This reduces self-auditing firms' incentives to invest in accident prevention (deterrence). Immunity thus lowers average pollution harm by prompting more self-auditing, but raises it by reducing deterrence.

Immunity also has opposing effects on government monitoring. Reduced deterrence raises incentives for government monitoring and regulation of self-auditors' "care." However, with immunity, government monitoring can no longer give rise to accident sanctions on self-reporting self-auditors, even though they can still be sanctioned for deficient care. As a result, the average deterrence-promoting effectiveness of inspections declines, thereby favoring less monitoring.

We expect the large deterrence depletion effects of immunity to dominate:

Hypothesis 2. Complete immunity raises emissions and government inspections.

Finally, many States have enacted an intermediate policy that provides limited immunity protection, but not "complete" immunity. These limited immunity statutes mimic the U.S. Environmental Protection Agency's (EPA's) guidance on reducing the "gravity based" penalties of self-reporting violators. Gravity based sanctions are those beyond the economic benefits that a firm derives by committing a violation.[7] As with privilege, we expect that these limited protections spur increased self-auditing that lowers average harm, an effect that may dominate the resulting deterrence depletion. Hence, comparing the limited immunity policy to one of no self-auditing inducements, we posit the testable prediction:

Hypothesis 3. Limited immunity statutes (that follow EPA guidance) lower emissions and government inspections.

3. THE DATA AND THE ECONOMETRIC MODEL

Dependent Variables. We construct a U.S. panel dataset over the period 1989-2003 where the cross-section units are State-level industries measured using three-digit SIC codes. Due to missing observations and omission of outliers, this gives us an unbalanced panel of 92 manufacturing industries (SIC codes 200-399) in the fifty states for a total of 19,472 observations.

We focus principally on outcomes at a State-specific industry level, as opposed to State aggregates or a more disaggregated facility level. We expect State regulators and regulated firms to respond to local industry-specific environmental, political and economic circumstances, which implies that regulatory (inspection) policy and emissions intensities will vary by State and, within a State, by industry. Aggregating up to a State level prevents us from controlling for individual State-specific industry effects and sacrifices information. Disaggregating to a plant level is instructive – and for this reason we present plant-level regressions below[8] – but gives us limited additional information because our explanatory variables cannot be measured at a facility level.[9]

A given industry's emissions for a given State are obtained by summing the reported releases of a given set of toxic chemicals from facilities that are located in the State and report the industry (three digit SIC) as their primary line of business. The release data is obtained from the EPA's Toxic Release Inventory (TRI). We consider the 172 chemicals that are regulated and monitored under the Clean Air Act's (CAA's) National Emission Standards for Hazardous Air Pollutants (NESHAPS, CAA Section 112(b), 40 CFR Part 61) and listed on the TRI throughout our study period.[10] We construct two measures of toxic emissions, both in total weight (millions of pounds of on-site emission), one for the 172 NESHAPS chemicals and the other for the 56 carcinogenic NESHAPS chemicals.[11] All of these chemicals are released to a common medium (air) and are subject to Federal emission standards, monitoring requirements (under the CAA) and reporting requirements under the Emergency Planning and Community Right to Know Act (EPCRA).[12] Because many fewer State-level industries release carcinogenic toxics (versus any NESHAPS toxics), the number of observations for carcinogenic emissions is substantially lower (9550).

Our focus on chemical releases regulated under the CAA reflects our interest in firms' incentives to avoid and curb pollution for which they can be monitored and sanctioned. In contrast, "unregulated" chemicals (under the CAA, even if reporting is technically required under the EPCRA) are not the object of government inspectors' attention or, therefore, the pollution abatement and avoidance efforts of firms. In addition, the regulated CAA chemicals involve tighter reporting requirements and enforcement that improve data quality and give us consistent pollution measures over our sample period.

For robustness checks, we consider two additional emission measures. The first is a toxicity-weighted aggregate of NESHAPS toxic releases; for a variety of reasons, we are skeptical about the merits and interpretation of this measure (note 11). The second is an alternative to our carcinogenic release measure designed to reflect more acutely toxic emissions, namely, releases (by weight) of chemicals that have high toxicity weights (greater than 500); this measure includes 69 of our initial 172 NESHAPS chemicals.

We measure government enforcement activity with the number of State and Federal inspections under the Clean Air Act. State and industry specific inspections numbers are obtained by summing across each industry's facilities in each State. The source for inspections data is the EPA's IDEA database. These inspections are determined by State and Federal regulators (in the EPA and State environmental agencies) in view of pre-determined Federal regulations and State-level policies that have been enacted by State legislatures and voters (including the self-policing policies of central interest here).

Econometric Model. For our two endogenous variables, we posit the following structural model:

$$E_{it} = X_{Eit}\, \beta_E + I^*_{it}\, \alpha_E + \varepsilon^*_{Eit}\,, \tag{1}$$

$$I^*_{it} = X_{Iit}\, \beta_I + E_{it}\, \alpha_I + \varepsilon^*_{Iit}\,, \tag{2}$$

where i denotes a cross-section unit (State-specific industry), t denotes time (year), E_{it} denotes emissions, $I^*_{it} = \ln(I_{it}+k)$ represents inspection intensity (with k>0 and I_{it} denoting inspection counts), X_{Eit} and X_{Iit} denote exogenous determinants of emissions and inspections respectively, and $(\varepsilon^*_{Eit}, \varepsilon^*_{Iit})$ are disturbances with zero conditional mean. In principle, inspection activity can promote emission reductions (as documented by Gray and Deily, 1996, and Deily and Gray, 2007, among others). In addition, the anticipation of higher emissions may spur more government enforcement scrutiny.

The structural model can be solved for the reduced form:

$$E_{it} = X_{it}\, \delta + \varepsilon_{Eit}, \tag{3}$$

$$I^*_{it} = X_{it}\, \gamma + \varepsilon_{Iit}, \tag{4}$$

where X_{it} is the union of X_{Eit} and X_{Iit}.

In what follows, we estimate the reduced form equations (3)-(4), rather than the structural forms (1)-(2), for three reasons. First and foremost, our interest ultimately is to measure the overall impact of self-policing statutes on emissions and inspections, including indirect effects of altered enforcement strategies (on emissions) and of changed emissions (on inspections). Second, as a practical matter, identification of inspections is problematic; for any instrument, a case can be made for direct relevance to emissions. And third, although we have good measures of regulated toxic air pollution, we lack measures of the "criteria air pollutants" (CO, SO2, NOX) also regulated under the CAA; the reduced form equations (3)-(4) implicitly capture effects of the latter pollutants.[13]

Explanatory Variables. We have five general classes of independent variables: (1) individual and time effects, (2) measures of industry scale within each State, (3) State attributes, (4) industry attributes, and (5) self-policing policy variables. In all models, we incorporate fixed time effects. Subject to caveats discussed below (Section 4B), we also incorporate fixed individual effects for all cross-section (State-industry or facility) units. This treatment accounts for unobserved heterogeneity and any time trends.

We construct three measures of industry scale: the number of industry facilities in each state (*Facility*), and measures of State-level industry output (*Sales*) and employment (*Empl*).[14] To obtain State-level sales and employment, we use facility numbers to allocate nationwide sales and employment numbers (constructed from the financial database for publicly traded companies, COMPUSTAT).[15] We expect larger industries to have higher emissions and greater inspection scrutiny, implying positive coefficients on *Empl* or *Sales*. However, controlling for industry size, we have no prior expectation on the effects of facility numbers, whether industries with more (and hence smaller) facilities will tend to produce more or fewer emissions and be subject to more or fewer inspections.

We include a number of State attributes. First, *Pop* measures the State's population. More populous States are expected to be more sensitive to toxic pollution and, hence, due to heightened public and regulatory pressure, to elicit lower levels of pollution and higher rates of inspection. Second, *Gspmm* measures gross State product in mining and manufacturing industries. Following Alberini and Austin (1999), we include this measure of output in the more polluting activities even though we control for individual effects; higher *Gspmm* may serve to focus regulatory efforts on pollution, potentially raising inspection activity and reducing releases.

Third, we include two measures of political attitudes. *Repvot* is the ratio of votes cast for the Republican candidate to total votes in the most recent presidential election. And *Sierra* is the State's per capita Sierra Club membership, a measure of the State's environmentalist constituency. We expect a higher *Repvot* to favor a pro-business regulatory environment, leading to higher emissions and fewer inspections. On the other hand, *Sierra* may yield more public scrutiny of industry environmental performance, thereby spurring fewer emissions and either fewer or more inspections as public scrutiny either spurs or substitutes for regulatory enforcement.

Fourth, *Income* is per capita income, reflecting overall economic activity and potentially intensifying either pro-business impulses or environmental preferences. Fifth, *Expend* is overall State government expenditures, and *Nrexp* measures State expenditures on natural resource programs, including conservation and regulation of exploitive industries. Higher overall expenditures may enable larger enforcement budgets and thereby enable more inspections that lead to reduced emissions. We include *Nrexp* to proxy for competition in State environmental budgeting between natural resource services and enforcement of clean air laws; for example, *Nrexp* may crowd out air-related enforcement expenditures and thus lead to fewer environmental inspections.[16] Last, *Strict* is a dummy variable indicating whether or not a State imposes strict environmental liability. Strict liability, as opposed to liability based on negligence, can favor higher emissions if firms are predominantly smaller with limited financial resources (so they can escape liability) or lower emissions if firms have deep pockets (Alberini and Austin, 1999), and may substitute or complement government enforcement efforts.[17]

We include four industry variables. *RD* is industry research and development expenditure. *Age* represents the vintage of industry assets, as measured by the ratio of net to gross assets (see Khanna and Damon, 1999); industries with newer assets (and hence, less accumulated depreciation) have *Age* values closer to one. *Herf* is the four-firm Herfindahl Index, a measure of industry concentration. And *Growth* is industry sales growth over the prior year. Newer assets contain more recent pollution abatement equipment and, hence, are expected to reduce toxic releases. Similarly, more research intensive and rapidly growing industries are expected to be more facile in abating pollution. More concentrated industries may be more heavily regulated because they are perceived to be more facile in adapting to tighter emission standards; on the other hand, concentrated industries may be more effective at lobbying for more lax regulation. Hence, expected effects of concentration (*Herf*) on emissions and inspections are unclear.

Finally, our key self-policing policy variables are dummies indicating whether or not a state has a particular statute in place.[18] Three general classes of self-policing statutes are indicated. First, does a State provide privilege protections to information contained in environmental self-audits? If so, our *Privilege* variable takes a value of one. Second, does a State explicitly provide reductions in gravity-based penalties to qualified self-reported violators, consistent with the EPA's 1995 Audit Policy? If so, our *Limited Immunity* variable takes a value of one. Third, alternatively, does a state provide complete immunity from penalty for qualified self-reported violations? If so, our *Immunity* variable takes a value of one. For each of these variables, we also make finer distinctions. In some states, audit privilege and immunity laws only apply to civil and administrative penalties, while in others, these laws also apply to criminal penalties. Variables distinguishing these effects are denoted by the suffixes, -*ac* (for administrative and civil) or –*aco* (for administrative, civil, and other). Limited immunity policies are similar in their provisions, but vary in their applicability. Some States apply benefits to all businesses (which we measure with the dummy, *LI-ab*), while others apply only to small businesses (*LI-sb*).

In practice, the distinction between *Limited Immunity* and *Immunity* statutes goes beyond language on the extent of immunity when it is granted (reducing gravity-based penalties versus waiving sanctions altogether). *Immunity* statutes are generally less restrictive in terms of the eligibility requirements for relief. Following EPA guidelines, *Limited Immunity* policies are much more specific and encompassing with regard to the information that a firm must provide, including self-audit material that goes beyond the initial disclosure.[19] Unlike *Immunity* statutes, these policies also stipulate specific timelines for disclosure and correction (note 4).

Like Stafford (2005), we treat our self-policing policy variables as exogenous. Inclusion of fixed effects mitigates the potential for endogeneity. Moreover, our industries are individually small contributors to overall State pollution; the average industry share of State emissions in our data is 3.5 percent. The corresponding average industry share of State inspections is 3.8 percent. There is the potential for cross-industry (intra-State) correlation in inspections for which we account in our estimation. In view of this potential, the adoption of self-auditing policies might reflect lawmakers' anticipation of tightened enforcement budgets and reduced inspection activity. In response to this point, we first note that the underpinning conjecture is belied by the analysis of Stafford (2006) who finds no significant effect of environmental budgets on the probability of State adoption of self-auditing policies. Nevertheless, beyond fixed effects, we incorporate a full set of exogenous State-level variables to control for this and other potential determinants of State adoption decisions. In particular, we control for State government spending, both overall (reflecting general budget pressures) and on natural resource programs (reflecting pressures on environmental budgets).[20]

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Table 1 describes our endogenous and explanatory variables. Table 2 gives corresponding sample statistics. Table 3 describes which States have adopted environmental *Privilege* or complete *Immunity* for self-reported violations. Table 4 describes which States have officially adopted a *Limited Immunity* policy following EPA guidance. Note that 22 States have adopted *Immunity* laws, and all but two of these, New Jersey and Rhode Island, have also enacted *Privilege* protections. Four additional States have enacted *Privilege* statutes, but not any *Immunity* protections. Beyond timing differences in adoption of self-auditing statutes, distinct effects of *Privilege* and *Immunity* are thus identified in our data by 6 out of the 26 States that adopted at least one of these statutes. Among these 26 States, 18 limit the protections to administrative and civil proceedings. In addition, 19 states have enacted EPA-sanctioned *Limited Immunity* statutes, with sixteen of these offering no additional (privilege or immunity) protections to environmental self-auditors. In all but two of these states, the enacted *Limited Immunity* benefits apply to all regulated businesses. In all, 42 States have enacted some form of policy inducement to environmental self-auditing.

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Most of the State statutes on self-policing were enacted in the middle of the 1990s, with some time series variation. This period follows development and promotion of Environmental Management Systems (Anton, Deltas, and Khanna, 2004) and other programs for firms to better monitor and control the effects of their activities on the environment (such as the ISO-14001 program, King, Lenox and Terlaak, 2005). Improved environmental self-auditing and control opportunities may have contributed to the spread of State policies that reward firm self-auditing initiatives. In our empirical models, we capture these forces with industry, time and in some cases, industry by time fixed effects; the latter account for general industry-specific technological change.

4. METHODS AND MODELS

A. The Emission Equation. For the reduced form emission equation (3) and for each of our emission measures ("total" NESHAPS chemicals and carcinogenic NESPHAPS chemicals), we estimate four linear models at a State-industry level, all with fixed cross section and time effects. The first model is a parsimonious specification that includes only the three key policy variables and measures of industry scale (*Sales*, *Facility*, and *Empl*), State population (*Pop*) and income (*Income*). The next two models include all of our posited explanatory variables, the first with our three aggregated policy variables (*Privilege*, *Immunity*, and *Limited Immunity*) and the second with all six self-policing policy variables (our "base model"). The fourth "expanded" model incorporates an expanded array of regressors, including lagged inspections, squares of several key variables, and industry-by-time fixed effects that control for arbitrary technological change.[21] Finally, we present a facility-level estimation of our "base model."

Although cross-observation error covariation does not bias coefficient estimates in our linear model, it can lead to substantial bias in standard error estimates (the Moulton or "false precision" problem, highlighted in Bertrand, Duflo, and Mullainathan, 2004). In our models, there is the potential for both time-series and cross-section covariation. We are particularly concerned about covariation within an industry, across States, as unobserved economic and regulatory phonomena may lead to common industry-level impacts. There is also the potential for covariation between industries within a State, as enforcement policy is decided at a State level. To account for such covariation, we cluster our errors at the multi-way (State and industry) levels (Cameron, Gelbach, and Miller, 2006). This approach accounts for arbitrary time series covariation within cross-section units and arbitrary cross-section covariation within cluster groups (within an industry across States or across industries within a State).[22]

Tables 5A and 5B present results from our emission equation estimations for total NESHAPS releases (Table 5A) and carcinogenic NESHAPS releases (Table 5B). Table 5C presents key coefficient estimates in "base model" estimations using toxicity-weighted NESHAPS releases and high-toxicity NESHAPS releases (those chemicals with toxicity weights greater than 500).

B. The Inspection Equation. Three central issues arise when estimating the inspection equation (4). First, inspections at each plant take a count form with no negative values and mostly zeroes and ones. Aggregating to a State-level industry gives us a somewhat more continuous distribution of values, but still one with a substantial number of zeroes (a third of our sample) and counts that are predominantly less than five (72 percent).[23] Second, as with emissions, there is the potential for cross-observation error covariation to bias standard error estimates produced by standard methods. Intuitively, one might expect that inspection policy, within a State, may be correlated across industries. Without accounting for this covariation, standard error estimates can understate the true extent of error in coefficient estimates (Bertrand, et al., 2004). Third, we have a panel structure with individual effects.

To account for these potential concerns, we present four types of estimation, all of which include fixed time effects. First, we estimate a count model that takes the standard linear exponential form of equation (4) (Cameron and Trivedi, 1998). Because Poisson fixed effects models suffer from an equi-dispersion constraint that we reject in statistical tests, we present Negative Binomial conditional fixed effects estimations that do not impose this constraint.[24] Second, we estimate two types of models with multi-way State-by-industry clustering (Cameron, et al., 2006): (1) Negative Binomial (NB) with fixed

State and industry effects, and (2) fixed effects linear models. Third, accounting for all concerns at the cost of lost information, we aggregate our data to a State level, giving us (loosely speaking) a continuous dependent variable that eliminates within-State cross-section covariation.[25] With these data, we estimate a fixed effects linear model and cluster the errors to account for generalized autocorrelation. Finally, as a robustness check, we also estimate a number of models at a facility level.

Tables 6A-6C present results from the estimations. Table 6A presents three NB fixed effects models: (i) a "reduced" model, (ii) a "base" model with all explanatory variables and the three unpartitioned self-policing policy variables, and (iii) an expanded model that includes lagged emissions, squares of key policy variables, and the six partitioned self-policing indicators. Table 6B presents clustered NB and linear fixed effects estimations; both "expanded" models include industry-by-time effects that control for technological change. Table 6C present results from facility-level and State aggregate regressions.[26] In all cases, we estimate dynamic models in view of generally significant lags.[27]

5. RESULTS

We begin by summarizing the estimation results for our main variables of interest, the self-policing policy variables *Privilege*, *Immunity*, and *Limited Immunity*:

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1) *Privilege* reduces both total toxic air pollution and carcinogenic toxic air pollution (see Tables 5A-5B). These effects are statistically significant, but driven by protections for administrative and civil offenses and not additional protections for criminal offenses. The estimated effect of administrative and civil privilege

protection is to reduce total toxics by approximately 35% and total carcinogenic toxics by approximately 90% (both as proportions of sample mean emissions). However, in our State-specific industry regressions, the estimated impact of *adding* criminal privilege is to *increase* total toxics by 23% and carcinogenic toxics by 54%, offsetting two-thirds and 60 percent of the original emission reductions, respectively. Indeed, the estimated effect of the expansive privilege protection (*P-aco*) on the NESHAPS aggregates is statistically insignificant. Qualitatively similar conclusions are obtained using high-toxicity NESHAPS emissions (Table 5C). For toxicity-weighted emissions (our suspect measure), we find a positive effect of broad privilege (including criminal protection), but still a negative (though insignificant) effect of privilege for administrative and civil offenses on the weighted releases.

2) *Immunity* raises both total and carcinogenic toxic air pollution (Tables 5A-5B). These effects are large, generally statistically significant, and attenuated if criminal immunity is added to immunity in administrative and civil disputes. The estimated impact of administrative and civil immunity is to raise total toxic pollution by approximately 28 percent on average and total carcinogenic toxics by over 100 percent (Models 3 and 4, Tables 5A-5B). However, the addition of criminal immunity offsets these increases by between 53 and 75 percent of the original increase for total toxics and between 85 and 93 percent of the original increase for carcinogenic toxics. These qualitative conclusions are robust to plant-level regressions (Model 5 in Tables 5A-5B) and alternative emission measures (Table 5C). For our suspect measure, toxicity-weighted emissions, the addition of criminal immunity not only completely offsets the effects of immunity

for administrative and civil offenses; it leads to an estimated reduction in weighted emissions.

<<COMP: Place Table 6A about here>>

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3) *Privilege* reduces inspection rates. Regardless of the data (State-industry level, facility-level, or State aggregate) or econometric model (count or linear), we estimate significant negative effects of *Privilege* on inspections (see Tables 6A-6C). In the undifferentiated (three policy) models, *Privilege* is estimated to reduce inspections by between 13 and 23 percent.[28] In all models with State-industry data (Tables 6A-6B), we estimate smaller impacts if privilege for criminal offenses is also attached; for example, in the third count (NB) model in Table 6B, privilege for administrative and civil offenses is estimated to reduce inspections by 18 percent, but adding criminal privilege is estimated to offset more than half (9.6 percent) of this reduction. Using the plant-level data, however, we find no significant effect of adding criminal privilege; without the additional protection, *Privilege* is estimated to reduce inspections by approximately 21 percent (Table 6C, Model 3); with it, the estimated effect is 24 percent, a statistically insignificant difference.
4) *Immunity* raises inspection rates in all models. In the undifferentiated (three policy) models, estimated impacts range from increases of 14 to 29 percent in the State-industry models (Tables 6A-6B) and from 54 to 61 percent in the facility-level models (Table 6C). These effects are statistically significant except in the State aggregated linear estimation that discards cross-industry information. In all models, we estimate smaller impacts if immunity for criminal offenses is also

applied (*I-aco* vs. *I-ac*). For example, in the six-policy State-industry count model (Model 3 of Table 6B), immunity for administrative and civil offenses (*I-ac*) is estimated to raise inspection rates by 41 percent, and all but three percent of this increase is offset if criminal immunity is added (*I-aco*).

5) We generally do not find statistically significant impacts of the *Limited Immunity* policies on emissions or inspections, with coefficients sometimes positive and sometimes negative. There are a few exceptions, but none that are robust across different data types or econometric models.[29]

Note that *Immunity* is quite tightly associated with *Privilege* in our data, both in terms of time and space, with only two States adopting *Immunity* without *Privilege*. As a result, we interpret our estimates of the effect of immunity as impacts of *adding* immunity to privilege, as opposed to impacts of free-standing immunity.

How can one explain the impacts of criminal protections that are identified here? Intuitively, fear of criminal prosecution, even if a remote possibility, makes self-auditing particularly worrisome to firm managers as it can provide prosecutors with a roadmap to environmental crimes that managers may themselves be unaware of prior to an audit (see Starr and Cooney, 1996, for example). Both criminal privilege and criminal immunity can help in allaying these fears, but criminal immunity may be the more important of the two. If so, criminal immunity will provide a significant spur to the adoption of self-auditing, while the addition of criminal privilege will have a relatively small effect on these incentives (beyond the impact of privilege in administrative and civil matters). With both criminal protections depleting pollution prevention incentives, the spur to self-auditing can dominate in the case of criminal immunity – leading to a net effect of lower pollution – while the deterrence depletion effect can dominate in the case of criminal privilege. To counter the deterrence depletion effect of criminal privilege, the government may increase its monitoring of firms' environmental practices to ensure that they meet desired standards of "care." In contrast, self-auditing – with immunity – negates the effect of government monitoring on accident sanctions and thereby reduces inspection incentives; hence, the added spur to self-auditing provided by the addition of criminal immunity can lead in turn to reduced inspection activity.

To sum up, *Privilege* has salutary effects on pollution and requisite government inspections; it lowers both of them. In contrast, complete immunity has adverse effects on pollution and regulatory costs as it raises both of them. Our results thus support our initial Hypotheses 1 and 2. *Privilege* is most effective in lowering pollution and inspections if it is applied only narrowly, to civil and administrative cases and not to criminal offenses. For *Immunity*, implications for the breadth of application are less clear. *If* a State is going to enact a complete immunity statute, it may be advantageous to apply immunity broadly – to administrative, civil *and* criminal cases; the *addition* of criminal immunity is estimated to lower both toxic emissions and government inspection effort. However, relative to complete immunity, the more confined protections afforded by *Limited Immunity* statutes that mimic EPA guidance are generally estimated to lower both pollution and inspections.[30] This suggests the tentative prescription that combining the *Limited Immunity* policies with administrative and civil *Privilege* protections can have the salutary effects of spurring lower toxic pollution and saving regulatory costs.[31]

In addition to self-policing and scale effects, Tables 5 and 6 reveal impacts of a few other variables. Industry research (*RD*), concentration (*Herf*), and newer assets (*Age*) are found to reduce toxic emissions (Tables 5A-5B). All of these variables are associated with greater facility to abate pollution, spurring tighter regulation, consistent with results of other studies focusing on impacts of innovation (see, for example, Carrion-Flores and Innes, 2009). As conjectured at the outset, State expenditures on natural resource programs (*Nrexp*) crowd out enforcement of clean air laws, leading to fewer inspections (Tables 6A-6B); however, we find no significant resulting impact on toxic air pollution, perhaps because State regulators compensate by focusing inspections more on toxic, rather than criteria, air pollution. Last, in all inspection models other than the one estimated using State aggregates, per-capita Sierra Club membership (*Sierra*) and Republican vote shares (*Repvote*) are negatively associated with inspection activity (significantly so in the Negative Binomial models). These results are consistent with pro-business constituencies successfully pushing for less regulatory oversight and private environmental pressure (*Sierra*) serving as a substitute for government enforcement (see Innes and Sam, 2008, for a similar result). Neither effect is found to significantly increase toxic air pollution; indeed, private political pressure on firms' environmental performance (as measured by *Sierra*) is found to reduce toxic air carcinogens, more than compensating for reduced inspection activity. However, the size of this estimated impact is small: A doubling of per-capita Sierra Club membership is estimated to lower carcinogenic emissions by one-fifth of one percent (.002 in the three-policy Model 2, and .0016 in the six-policy Model 3).

6. CONCLUSION

Regulators and environmental groups criticize State-level self-policing statutes because they enable firms to hide their environmental crimes in the case of privilege, and deny them incentives to prevent pollution outbreaks in the case of immunity. As a result, they argue, these policies lead to more pollution and require more government monitoring of regulated firms to ensure that appropriate pollution abatement activity takes place. In contrast, proponents of these statutes argue that they are necessary if firms are to audit their own environmental practices with auditing programs that are costly but yield substantial dividends by identifying and correcting pollution outbreaks that would not otherwise be discovered. These pollution-reduction benefits of self-auditing are made possible by statutes that protect firms from thereby incriminating themselves and give regulatory rewards to the self-reporting of self-discovered violations. In addition, because firms audit themselves, the statutes may also permit environmental law enforcement to be done effectively with fewer government inspections.

These two competing arguments embed incentive effects that are also competing and yield theoretically ambiguous impacts of self-policing policies. Immunity encourages firms to adopt self-auditing programs that can lower the harm from pollution outbreaks. However, immunity also reduces self-auditing firms' incentives to avoid pollution violations in the first place, thereby increasing average pollution and motivating more government monitoring of firms' pollution prevention activities. We conjecture that *complete* immunity has such powerful deterrence-depletion effects – because it assesses firms no penalty at all for pollution violations, other than costs of correction – that the second (pollution-raising) effect will dominate the first (self-auditing-promotion) effect. Hence, we expect immunity to raise both toxic pollution and government inspections.

Similarly, privilege encourages firms to adopt self-auditing programs, but makes it more difficult for regulators to identify and sanction slovenly firm performance in pollution prevention (care). With less effective "care" regulation, firms have less incentive to exercise care, and the government has less incentive to regulate it. Privilege can thus lower pollution by eliciting more environmental self-auditing, but raise pollution by reducing deterrence. If the first effect dominates – as we conjecture – then pollution will fall and governmental monitoring is also likely to decline both because harm-reduction benefits of monitoring are smaller and because monitoring is less effective in spurring pollution prevention.

Our empirical results confirm both of our conjectures to varying degrees. Privilege protections are estimated to reduce toxic emissions and government inspections. These results indicate salutary effects of these policies as they spur savings of both environmental costs and regulatory resources. In contrast, complete immunity is estimated to raise toxic emissions and government inspections. These effects tend to confirm environmentalists' criticism of excessively liberal self-reporting inducements as protecting polluters to society's detriment. Overall, our results suggest that providing firms with positive incentives for environmental self-auditing, by protecting their audits from use by government prosecutors, can be a valuable component of environmental law enforcement as it reduces both pollution and enforcement costs. They also suggest the need for care in the design of self-auditing inducements and argue against blanket privilege and immunity protections and instead in favor of more targeted and limited protections.

Footnotes

[1] The authors thank George Frisvold, Alan Ker, Tauhid Rahmann, Jodi Short, Gary Thompson, two anonymous reviewers, and the Editor, Al Klevorick, for valuable comments on prior versions of this paper. The usual disclaimer applies.

[2] See the initial papers of Kaplow and Shavell (1994) and Malik (1993), and recent papers by Pfaff and Sanchirico (2000), Mishra, Newman and Stinson (1997), Friesen (2006), Livernois and McKenna (1999), and Innes (1999a, 1999b, 2000, 2001a). See also the related literature on self-regulation (e.g., Maxwell, Lyon and Hackett, 2000; Maxwell and Decker, 2006).

[3] Stafford (2005) controls for state effects, but not industry or time effects. In addition, we consider a variety of time-varying industry forces and State variables omitted in Stafford's analysis, including measures of industry scale, concentration, growth and R&D, and State population and political composition that can be important in driving environmental regulatory policy.

[4] To obtain these benefits, firms must satisfy various technical requirements, including: disclosing the violation within 21 days of discovery; correcting the violation within 60 days; taking steps to avoid a recurrence of the violation. In addition, the violation must not have been found by a third party and must not be an "imminent and substantial endangerment to public health or the environment" (EPA, 1995).

[5] Privilege makes environmental audit reports inadmissible as evidence in administrative, civil, and sometimes criminal proceedings, including those for environmental enforcement actions (see Koven, 1998). However, privilege does not exclude documentation that is part of an audit report, but also contained in other reports required by law. Privilege thus does not protect the factual material in an audit report (Frey and

Johnson, 2000). To the extent that the government acquires information independently from an audit report, but the information overlaps with the factual content of the audit, privilege can nevertheless provide a legal roadblock to use of the information as the firm can dispute its source. Privilege also protects firms from disclosure of information contained in an audit to third parties. Although States differ in the breadth of their statutes, privilege is typically voided when a violation is not diligently corrected (Weaver, et al., 1997).

[6] An expanded version of this paper (available upon request) contains a conceptual model that identifies the competing influences of self-policing statutes on government inspections and pollution, as discussed below.

[7] EPA (2000) policy provides conditions under which pieces of potential environmental sanctions are reduced or eliminated. Sanctions for "economic benefits" obtained by committing violations are not affected by the EPA policy. Gravity-based penalties are over and above the economic benefit. According to the EPA (2000), "they reflect the egregiousness of the violator's behavior and constitute the punitive portion of the penalty." Gravity based sanctions for self-reporting violators are eliminated if a firm meets nine conditions, including the presence of a systematic intra-firm self-auditing program; if all but this last condition are met, 75 percent of gravity based sanctions are voided. State-level complete immunity statutes eliminate all sanctions under weaker conditions (note 19).

[8] We are very grateful to the referees for suggesting these regressions.

[9] This is a well-known problem for empirical researchers in environmental economics. Tying EPA data on facility-level inspections and emissions to firm-level financial data is virtually impossible in large datasets such as ours.

[10] In an earlier version of this paper, we also consider total TRI emissions and identify qualitatively similar effects to those found with our new data.

[11] Recent environmental economics studies use toxicity weighted TRI releases to measure toxic emissions (e.g., Innes and Sam, 2008). For a narrowly defined set of chemicals with common properties – such as the 17 33/50 chemicals of interest in much of this literature – toxicity weighted aggregates are arguably useful indicators of pollution harm. However, we are interested in impacts of self-auditing statutes on overall toxic air pollution at an industry level, requiring measurement of a broad set of chemical releases. For this purpose, a variety of inconsistencies between (and limitations of) methods for constructing toxicity weights for different chemicals makes toxicity weighted aggregation highly problematic (see www.peri.umass.edu for details and documentation on these methods). Toxicity weights for non-carcinogens are based on two measures (RfC and RfD) that have different units (mg/m^3 vs. mg/kg-day), each defined as "an estimate (with uncertainty spanning perhaps an order of magnitude)" of non-threatening continuous daily exposure over a lifetime. Carcinogens also have two different types of toxicity weights (IUR and SF), also measured in different units (mg/m^3 vs. mg/kg-day), both indicators of risk (versus the no-adverse-effect indicators for non-carcinogens). The indicators for carcinogenic risk are based on the "upper bound excess lifetime cancer risk" from continuous exposure where there is a non-linear dose response. Hence, when emissions are non-continuous, or there are different variances in the estimation of cancer risks for different chemicals (with upper bounds higher when there is more variation, ceteris paribus), these measures are problematic. Also missing from these weightings are crucial determinants of risk, including stack height, human population concentrations, winds, and so forth. Perhaps the least meaningful weight is for asbestos, which has the

highest toxicity index by orders of magnitude (1000000), is in completely different units than others (fibres per litre) and is based largely on qualitative judgment. For these reasons, toxicity weights are often incomparable across chemicals, whether within a class (carcinogenic and non-carcinogenic) or across classes, and even in isolation are limited as measures of relative risk. For our purposes, therefore, we believe that the most reliable and meaningful measures of toxic air pollution are based on simple weight.

[12] TRI release measures are sometimes criticized because the reporting requirements of the EPCRA are subject to limited enforcement and often represent "legal emissions" in the sense that, when monitoring is not required, reported releases are based on estimates driven by the technological requirements of pollution permits. However, NESHAPS chemicals are regulated; both EPCRA and the CAA require reporting of the true emissions when known; and the EPA prosecutes firms that do not truthfully report (see, for example, EPA New England Press Release, Oct. 5, 2001, "EPA Seeks Monetary Penalty Against Durham, Conn. Company for Violations of Environmental Regulations").

[13] The additional (unobserved) class of criteria pollutants can be thought of as adding a structural equation,

$$C_{it} = X_{Cit}\,\beta_C + I^*_{it}\,\alpha_C + \varepsilon^*_{Cit}\,,$$

and an additional argument, $C_{it}\,\alpha_2$, to equation (2). The reduced form in (3)-(4) does not change in structure.

[14] All financial variables in our analysis are real (1995=100).

[15] Specifically, we scale nationwide industry sales and employment by the proportion of industry facilities belonging to each state,

$$S_{ijt} = n_{ijt} / \{ \sum_{j} n_{ijt} \},$$

where the ith and jth indexes refer to industry and State, respectively, and n denotes number of facilities.

[16] Potential effects on emissions are unclear. By lowering inspections, higher *Nrexp* may lead to higher emissions. However, higher *Nrexp* may also indicate public sensitivity to environmental issues and, due to generalized community and political pressure, promote pollution abatement.

[17] If strict liability substitutes for government oversight – spurring fewer inspections and regulatory actions – it may also indirectly spur higher emissions. In our empirical work, however, we do not find evidence for this "enforcement substitution" effect.

[18] These variables are constructed from data in Frey and McCollough (2003) and a review of State Codes.

[19] The EPA requires, at a minimum, "access to all requested documents; access to all employees of the disclosing entity; assistance in investigating the violation, any noncompliance problems related to the disclosure, and any environmental consequences related to the violations; access to all information relevant to the violations disclosed, including that portion of the environmental audit report or documentation from the compliance management system that revealed the violation; and access to the individuals who conducted the audit or review" (EPA, 1995). In contrast, *Immunity* statutes do not define what cooperation is specifically required for relief (with the exception of Rhode Island) and in almost all cases, limit required cooperation to the investigation of the self-reported violation. Ohio's statute is typical, requiring cooperation in "investigating the cause, nature, extent, and effects of the non-compliance" (Frey and McCollough, 2003).

[20] We also control for political and economic circumstances that capture relevant incentives for policy adoption. Stafford (2006), for example, finds that the relative size of pollutant industries in the State economy, a State's political (Republican voting) tendencies, and various cross-section State attributes (such as the State's commitment to environmental policies, as measured by Lester's (1994) breakdown) can be important drivers of State adoption of self-policing statutes. We control for these forces using, respectively, the mining and manufacturing share of State economic activity, the Republican vote share, and fixed effects. The Sierra Club membership and strict environmental liability variables provide additional controls for political and environmental policy impulses, while population and income control for other relevant socio-economic pressures for self-auditing reform.

[21] We are indebted to an anonymous referee for proposing this model. Industry by time effects are at a two-digit SIC level.

[22] The Moulton problem is potentially even more acute with plant-level data, and again is addressed with multi-way (State by industry) clustered errors. We have also estimated dynamic counterparts to equation (3). Because qualitative results are similar, lagged emissions are insignificant, and clustering accounts for autocorrelation, we only present the simpler non-dynamic models in Table 5.

[23] The truncation at zero is less pronounced when one accounts for our fixed effects. Dropping State-level industries that are never inspected over our sample period (as done with fixed effects), 12 percent of our sample have inspection numbers equal to zero. However, 63 percent of the sample have inspection numbers less than five. With plant-level data, the count structure is much more pronounced, with 70 percent of inspection counts equal to zero and over 99 percent less than five.

[24] We have also estimated Poisson and Negative Binomial random effects models and obtained qualitatively similar results. We attempted Tobit estimations with clustered errors, but the estimations did not converge. Available software does not permit estimation of clustered conditional fixed effects count models. We did not attempt clustered explicit (unconditional) fixed effects count models because they yield inconsistent parameter estimates due to the incidental parameters problem (Hausman, et. al, 1984).

[25] To obtain State aggregates, industry variables are weighted by shares of State facilities with the exception of the dependent variables and the scale variables (*Sales*, *Facility*, and *Empl*), which are summed across the State-level industries.

[26] We again thank the referees for suggesting many of these additions. Attempts to estimate NB fixed effects and plant-level models with the industry-by-time dummies did not converge. Because the count structure of the data is particularly pronounced in the plant-level regressions, we focus on clustered NB models with fixed State, industry and time effects in Table 6C.

[27] For the NB models, the lagged regressor equals the log of one plus lagged inspections, consistent with the exponential model form. Dynamics alter our theoretical formulation, eq.s (1)-(4). With no dynamics in emissions (note 22), eq. (1) persists. Inspection dynamics alter equations (2)-(4) with the addition of the right-hand lag, I^*_{it-1} and in the "expanded" models, E_{it-1}. In the emissions equation, when we include lagged inspections, the presence of serial correlation requires that we instrument the lagged variable; finding an insignificant coefficient on the inspection lag, when it is included, we present models without this variable. For inspections, remaining serial correlation will generate correlation between the error and both lags (emissions and inspections). Absent

serial correlation, the lags can be treated as exogenous in the NB models; in none of these models do we reject the null of no autocorrelation (see presented z statistics, distributed standard normal under the null). For the linear models, the Wooldridge test statistic for autocorrelation is statistically significant and, regardless, fixed effects estimation implies correlation between the lagged mean-differenced inspection variable and the error; we therefore instrument the lagged endogenous variables with lagged data in the estimations presented (following standard practice, see Greene, 2003).

[28] In the Negative Binomial models, estimated proportional marginal effects equal exp(b)-1, where b is the coefficient estimate on the policy dummy.

[29] For emissions (Tables 5A-5C), the exceptions are: (1) Broadly applied *Limited Immunity* (*LI-ab*) is estimated to have a significant positive impact on plant-level total toxics (Table 5A, Model 5) and State-industry carcinogenic toxics in our expanded model (Table 5B, Model 4); (2) the narrowly applied policy (*LI-sb*) is estimated to have a significant positive impact on high-toxicity State-industry emissions (Table 5C); and (3) the composite (*Limited Immunity*) policy is estimated to have a significant negative impact on toxicity-weighted emissions (Table 5C). For inspections (Tables 6A-6C), there are two exceptions: (1) In our three-policy fixed effects count models of Table 6A, we estimate significant negative coefficients on the *Limited Immunity* policy dummy; however, once we account for the Moulton problem by clustering the errors, the statistical significance of the parameter estimates vanishes (Table 6B); (2) in the plant-level regressions (Table 6C), the narrowly applied policy (*LI-sb*) has a significant positive coefficient. When *Limited Immunity* coefficients are positive, they are almost always small by comparison to counterparts for *Privilege* and *Immunity*.

[30] Comparing coefficients on broad immunity (*I-aco*) and limited immunity (*LI-ab*) in the six-policy emissions equations, there is generally not a statistically significant difference between them. However, comparing overall *Immunity* to *Limited Immunity* in models with three policies, the latter is estimated to lower pollution relative to the former in all cases (differences that are statistically significant). Similarly, in our inspections models, *Limited Immunity* leads to fewer inspections relative to *Immunity* counterparts.

[31] In theory, *Privilege* is of no benefit absent some immunity protection; without the latter, there are no incentives for adoption of self-auditing programs.

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Table 1. Definitions of Variables

Level of aggregation	Variable	Description	Source
Sic-state	Emissions	Millions of pounds of toxic on-site air emissions of NESHAPS chemicals (Total or Carcinogenic only)	TRI (www.epa.gov/tri/)
Sic-state	Inspections	State and Federal Clean Air Act inspections	IDEA Database
Sic-state	Facility	Number of facilities registered in the IDEA Database	IDEA Database
State	Pop	State population (millions)	Economagic (www.economagic.com)
State	Income	State income per capita (millions of dollars)	Economagic (www.economagic.com)
State	Expend	State expenditures (billions of dollars)	Economagic (www.economagic.com)
State	Nrexp	State expenditures on natural resources (millions of dollars)	US *Statistical Abstracts*, various years
State	Gspmm	Gross State Product in mining and manufacturing (trillions of dollars)	Bureau of Economic Analysis (www.bea.gov/bea/regional/gsp/)
State	Sierra	State Sierra Club membership per capita	Sierra Club
State	Repvot	Ratio of popular vote cast for republican candidate to total votes in the most recent presidential election	US *Statistical Abstracts*, various years
Sic	RD	Industry R&D expenditures (billions of dollars)	Compustat
Sic	Age	Industry age of assets (Net assets/Gross Assets)	Compustat
Sic	Herf	Industry four-firm Herfindahl Index	Compustat
Sic	Growth	Industry growth in sales	Compustat
Sic-state	Empl	Industry number of employees by state (millions)	Compustat and IDEA
Sic-state	Sales	Industry total sales by state (trillions of dollars)	Compustat and IDEA
State	Strict	Dummy variable indicating strict liability	Environmental Law Institute (ELI)
State	P-ac	Dummy variable indicating Privilege applicable to administrative and civil penalties	State Codes, various years
State	P-aco	Dummy variable indicating Privilege applicable to administrative, civil and criminal penalties	State Codes, various years
State	I-ac	Dummy variable indicating Immunity applicable to administrative and civil penalties	State Codes, various years
State	I-aco	Dummy variable indicating Immunity applicable to administrative, civil and criminal penalties	State Codes, various years
State	LI-sb	Dummy variable indicating Limited Immunity Policies only valid for small businesses	State Codes, various years
State	LI_ab	Dummy variable indicating Limited Immunity Policies applicable to all businesses	State Codes, various years
State	Limited Immunity	Dummy variable indicating Limited Immunity Policies (LI-sb or LI-ab)	State Codes, various years
State	Immunity	Dummy variable indicating Immunity (I-ac or I-aco)	State Codes, various years
State	Privilege	Dummy variable indicating Privilege (P-ac or P-aco)	State Codes, various years

Table 2. Summary Statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
Inspections (plant-level)	251355	0.476	1.281	0	111
Inspections (sic/state-level)	19472	4.797	10.737	0	281
Emissions unweighted (sic/state-level)	19472	0.407	0.891	1.58E-10	10.63372
Emissions unweighted (plant-level)	114675	0.069	0.229	4.00E-12	6.518379
Emissions carcinogenic (sic/state-level)	9550	0.101	0.314	2.00E-11	5.836833
Emissions carcinogenic (plant-level)	24456	0.039	0.14	4.30E-12	5.727293
Emissions weighted (sic/state-level)	19418	116.845	1053.864	1.35E-06	54032.14
Emissions 500 + (sic/state-level)	11892	0.03	0.112	1.58E-10	2.854608
Expend	19472	0.31	0.067	0.178659	1.229198
Age	19472	0.764	0.116	0.073604	1
Herf	19472	5.866	2.326	2.51391	10
Sales	19472	0.001	0.003	3.12E-09	0.072215
RD	19472	0.732	2.682	0	18.16555
Growth	19472	0.294	2.068	-0.97476	29.37388
Facility	19472	6.183	9.593	1	224
Empl	19472	0.002	0.007	3.45E-08	0.202307
Sierra	19472	0.002	0.003	0.00031	0.052502
Pop	19472	7.342	6.563	0.45369	35.48445
Nrexp	19472	0.33	0.388	0.023329	2.894366
Income	19472	0.023	0.004	0.015314	0.036931
Gspmm	19472	0.038	0.032	0.000679	0.172008
Repvote	19472	0.458	0.089	0.106178	0.678899
Strict	19472	0.741	0.438	0	1
P-ac	19472	0.146	0.353	0	1
P-aco	19472	0.133	0.34	0	1
I-ac	19472	0.137	0.344	0	1
I-aco	19472	0.073	0.26	0	1
LI-sb	19472	0.011	0.104	0	1
LI-ab	19472	0.182	0.386	0	1
Privilege	19472	0.279	0.448	0	1
Immunity	19472	0.21	0.407	0	1
Limited Immunity	19472	0.193	0.395	0	1

Table 3. Audit Privilege and Immunity Laws: Provisions and Years of Adoption

State	Year of adoption	Privilege	Immunity	Provisions: Administrative and Civil Penalties	Provisions: Other legal actions
Alaska	1997	x	x	x	
Arkansas	1995	x		x	x
Colorado	1994	x	x	x	x
Idaho	1996*	x	x	x	x
Illinois	1995	x		x	x
Indiana	1994	x		x	Criminal penalties removed in the 1999 amendments
Iowa	1998	x	x	x	
Kansas	1995	x	x	x	x
Kentucky	1996	x	x	x	
Michigan	1996	x	x	x	Criminal penalties removed in the 1997 amendments
Minnesota	1995	x	x	x	x
Mississippi	1995	x	x	x	Criminal penalties removed in the 2003 amendments
Montana	1997**	x	x	x	
Nebraska	1998	x	x	x	x
Nevada	1997	x	x	x	x
New Hampshire	1996	x	x	x	
New Jersey	1995		x	x	
Ohio	1997	x	x	x	
Oregon	1993	x		x	Criminal penalties adopted in 1997 amendments and removed in 2000
Rhode Island	1997		x	x	
South Carolina	1996	x	x	x	Criminal penalties removed in the 2000 amendments
South Dakota	1996	x	x	x	
Texas	1995	x	x	x	Criminal penalties removed in the 1997 amendments
Utah	1996	x	x	x	
Virginia	1995	x	x	x	
Wyoming	1995	x	x	x	

Source: Frey and McCollough (2003)
*In sunset since 1997
**In sunset since 2001

Table 4. Limited Immunity Policies: Provisions and Years of Adoption

State	Year of adoption	Applies only to Small Business	Applies to All Business
Arizona	2002		x
California	1996		x
Connecticut	1996		x
Delaware	1994		x
Florida	1996		x
Hawaii	1998		x
Indiana	1999		x
Maine	1996	x	
Maryland	1997		x
Massachusetts	1997		x
Minnessota	1995		x
New Mexico	1999		x
New York	1999	x	
North Carolina	1995		x
Oregon	2002		x
Pennsylvania	1996		x
Tennessee	1996		x
Vermont	1996*		x
Washington	1994		x

Source: Frey and McCollough (2003)

* In sunset from 1998 to 2000

Table 5A. Total NESHAPS Emissions

		State-Industry				Plant-Level
Variables		Model 1	Model 2	Model 3	Model 4	Model 5
Inspection $_{t-1}$					-0.0029	
					[0.0034]	
Expend			-0.409	-0.44	-0.390	-0.0768
			[0.43]	[-0.4]	[0.415]	[0.0628]
Age			-0.23	-0.23	-0.207**	-0.0431**
			[0.14]	[0.14]	[0.099]	[0.0218]
Herf			-0.0228*	-0.0228*	-0.025**	-0.0028
			[0.013]	[0.013]	[0.0106]	[0.0023]
Sales		-35.17***	-36.09***	-36.18***	-20.940	-1.046
		[12.3]	[11]	[11.2]	[28.363]	[1.7853]
Sales 2					-428.5	
					[473.185]	
RD			-0.0390***	-0.0391***	-0.0591***	-0.007**
			[0.0083]	[0.0083]	[0.018]	[0.0028]
RD 2					0.0018	
					[0.0016]	
Growth			0.00209	0.00207	-0.0018	0.0005
			[0.003]	[0.003]	[0.0022]	[0.0005]
Facility		-0.00326	-0.00341	-0.00335	0.0018	0.0001
		[0.004]	[0.0038]	[0.0039]	[0.002]	[0.0002]
Empl		9.26	13.39**	13.38***	0.8790	0.796
		[6.78]	[5.61]	[5.57]	[10.57]	[0.675]
Empl 2					93.92	
					[73.46]	
Sierra			0.414	0.68	0.566	0.0816
			[1.21]	[1.26]	[1.201]	[0.069]
Pop		-0.0288*	-0.0321	-0.0328	-0.019	0.0114***
		[0.0156]	[0.021]	[0.023]	[0.022]	[0.0029]
Nrexp			0.0569	0.0507	-0.006	0.0043
			[0.036]	[0.05]	[0.037]	[0.0123]
Income		33.37***	42.14***	38.49***	91.98*	3.911*
		[11.24]	[14.8]	[13.3]	[54.512]	[2.17]
Income 2					-895.8	
					[807.524]	
Gspmm			-1.046	-0.546	-2.281	-0.317
			[2.2]	[2.04]	[1.845]	[0.203]
Repvot			0.262	0.286	0.146	-0.0374
			[0.2]	[0.19]	[0.156]	[0.0394]
Strict			-0.0293	-0.0264	-0.046	-0.018**
			[0.043]	[0.043]	[0.046]	[0.0085]
	P-ac			-0.143***	-0.140**	-0.0171*
Privilege		-0.0936**	-0.088*	[0.065]	[0.066]	[0.0093]
	P-aco	[0.043]	[0.046]	-0.0472	-0.031	0.002
				[0.041]	[0.035]	[0.0072]
	I-ac			0.113*	0.110*	0.0155*
Immunity		0.0743	0.0685	[0.063]	[0.063]	[0.0087]
	I-aco	[0.046]	[0.050]	0.0531	0.027	-0.009
				[0.055]	[0.047]	[0.0086]
	LI-ab			0.0102	0.021	0.008***
Limited		-0.00979	-0.00555	[0.089]	[0.026]	[0.0035]
Immunity	LI-sb	[0.026]	[0.027]	0.00914	-0.002	-0.0016
				[0.026]	[0.092]	[0.0082]
Obs		19472	19472	19472	17229	114675
R2		0.12	0.13	0.13	0.18	0.04

Note: *,**,*** denote significant at the 10%, 5%, and 1% levels (two-sided). Robust standard errors are in brackets, clustered multi-way State-by-industry (Cameron, et al., 2006). All models include fixed cross-section (State-industry or plant) and time effects. The expanded Model 4 also includes industry by time dummies (in place of time effects). The first four models use State-specific industry level data, and the fifth model uses plant-level data. Lagged inspections instrumented in the expanded Model 4.

Table 5B. Carcinogenic NESHAPS Emissions

Variables		State-Industry				Plant-Level
		Model 1	Model 2	Model 3	Model 4	Model 5
Inspection $_{t-1}$					-0.0019	
					[0.0027]	
Expend			0.0422	0.0499	0.111	-0.009
			[0.2694]	[0.2995]	[0.2293]	[0.085]
Age			-0.103**	-0.106**	-0.0879	-0.043***
			[0.048]	[0.048]	[0.0747]	[0.011]
Herf			-0.0193***	-0.0192***	-0.0126***	-0.005***
			[0.0052]	[0.0051]	[0.0026]	[0.002]
Sales		5.892	4.388	4.182	10.38	-0.078
		[6.285]	[3.351]	[3.342]	[8.5914]	[0.634]
Sales 2					-245.9*	
					[145.089]	
RD			-0.0265***	-0.0262***	-0.0593***	-0.014***
			[0.0075]	[0.0074]	[0.0108]	[0.004]
RD 2					0.0021***	
					[0.0005]	
Growth			0.00197	0.002	-0.0003	0.0007**
			[0.0013]	[0.0013]	[0.0007]	[0.00036]
Facility		-0.00268	-0.00269	-0.00267	-0.0003	0.00021
		[0.0027]	[0.0027]	[0.0027]	[0.0013]	[0.00032]
Empl		-2.535	0.463	0.428	-3.561	0.634**
		[3.231]	[1.642]	[1.664]	[3.0836]	[0.322]
Empl 2					38.99	
					[26.201]	
Sierra			-1.037**	-0.829*	-0.449	-0.051
			[0.484]	[0.506]	[0.63]	[0.103]
Pop		-0.014	-0.00489	-0.00801	0.0027	0.010*
		[0.0104]	[0.0167]	[0.0164]	[0.017]	[0.006]
Nrexp			-0.0113	-0.00451	-0.0456	-0.001
			[0.0377]	[0.0283]	[0.034]	[0.012]
Income		10.26	15.32*	15.15*	21.07	1.089
		[6.337]	[8.122]	[7.819]	[29.039]	[2.781]
Income 2					-188.7	
					[471.343]	
Gspmm			-1.673	-1.439	-1.462	-0.723
			[1.435]	[1.568]	[1.25]	[0.64]
Repvot			-0.0147	0.0323	-0.0714	-0.059*
			[0.0848]	[0.0889]	[0.0812]	[0.034]
Strict			-0.0102	-0.00891	-0.0324	-0.001
			[0.0308]	[0.0298]	[0.0454]	[0.011]
	P-ac			-0.0911*	-0.0909*	-0.036*
Privilege		-0.0639*	-0.0621*	[0.05242]	[0.05]	[0.019]
	P-aco	[0.0357]	[0.0334]	-0.0363	-0.0304	-0.006
				[0.02811]	[0.0246]	[0.017]
	I-ac			0.104**	0.104**	0.036***
Immunity		0.0649*	0.0614*	[0.0447]	[0.043]	[0.0126]
	I-aco	[0.0332]	[0.0316]	0.0156	0.0073	-0.007
				[0.04441]	[0.0414]	[0.022]
	LI-ab			0.0235	0.0294*	0.0086
Limited		-0.00817	0.00169	[0.04071]	[0.0169]	[0.0058]
Immunity	LI-sb	[0.018]	-0.0102	0.0233	0.0345	0.011
				[0.015]	[0.0614]	[0.012]
Obs		9550	9950	9550	8148	24456
R2		0.09	0.13	0.13	0.19	0.05

Note: *,**,*** denote significant at the 10%, 5%, and 1% levels (two-sided). Robust standard errors are in brackets, clustered multi-way State-by-industry (Cameron, et al., 2006). All models include fixed cross-section (State-industry or plant) and time effects. The expanded Model 4 also includes industry by time dummies (in place of time effects). The first four models use State-specific industry level data, and the fifth model uses plant-level data. Lagged inspections instrumented in the expanded Model 4.

Table 5C. Self-Policing Policy Coefficients Using Toxicity-Adjusted NESHAPS Emissions

		Toxicity Weighted Emissions		High Toxicity (500+) Emissions	
Variables		3 Policies	6 Policies	3 Policies	6 Policies
	P-ac		-90.37		-0.012**
Privilege		26.250*	[77.854]	-0.003***	[0.006]
	P-aco	[15.069]	126**	[0.001]	0.004
			[57.933]		[0.008]
	I-ac		116.2***		0.017**
Immunity		-12.130	[43.898]	0.007*	[0.008]
	I-aco	[22.336]	-110.4**	[0.004]	0.0001
			[55.721]		[0.002]
	LI-ab		3.907		0.003
Limited		-39.68**	[27.471]	0.00013	[0.004]
Immunity	LI-sb	[20.095]	-19.52	[0.004]	0.009*
			[66.748]		[0.005]
Obs		19418	19418	11892	11892
R2		0.01	0.01	0.03	0.03

Note: *,**,*** denote significant at the 10%, 5%, and 1% levels (two-sided). State-industry data, "base case" models (Models 2-3). Robust standard errors are in brackets, clustered multi-way State-by-industry (Cameron, et al., 2006). All models include fixed cross-section (State-industry) and time effects.

Table 6A. Inspections Equation: Negative Binomial Fixed Effects (State-Specific Industry Data)

Variables		Model 1	Model 2	Model 3
$Emission_{t-1}$				-0.0338***
				[0.0078]
$Inspection_{t-1}$		0.280***	0.261***	0.252***
		[0.011]	[0.011]	[0.011]
Expend			-0.181	-0.402
			[0.44]	[0.45]
Age			-0.0434	-0.0434
			[0.067]	[0.067]
Herf			0.00795*	0.00491
			[0.0045]	[0.0046]
Sales		0.0412	0.685	54.91***
		[6.85]	[6.96]	[12.2]
Sales 2				-885.8***
				[234]
RD			-0.00538	-0.0246*
			[0.0064]	[0.013]
RD 2				-0.000399
				[0.00071]
Growth			0.000972	0.0000452
			[0.0022]	[0.0022]
Facility		0.0286***	0.0283***	0.0268***
		[0.00088]	[0.00085]	[0.00086]
Empl		11.27***	11.62***	26.73***
		[2.32]	[2.37]	[4.00]
Empl 2				-133.4***
				[29.1]
Sierra			-5.869***	-4.960**
			[2]	[2]
Pop		0.00448	-0.0312***	-0.0361***
		[0.0058]	[0.01]	[0.01]
Nrexp			-0.228**	-0.241**
			[0.096]	[0.097]
Income		6.05	-25.49**	147.1***
		[9.35]	[10.5]	[37.7]
Income 2				-3049***
				[657]
Gspmm			12.09***	11.23***
			[1.32]	[1.34]
Repvot			-1.089***	-1.377***
			[0.2]	[0.22]
Strict			-0.00963	-0.0422
			[0.028]	[0.029]
	P-ac			-0.258***
Privilege		-0.140***	-0.154***	[0.038]
	P-aco	[0.024]	[0.023]	-0.0787***
				[0.027]
	I-ac			0.286***
Immunity		0.133***	0.157***	[0.043]
	I-aco	[0.028]	[0.028]	0.047
				[0.036]
	LI-ab			-0.013
Limited		-0.0469**	-0.0745***	[0.025]
Immunity	LI-sb	[0.021]	[0.022]	-0.114
				[0.14]
Obs		13039	13039	13039
z autocorr.		1.4141732	1.45504	1.2332671

Note: *,**,*** denote significant at the 10%, 5%, and 1% levels (two-sided). Standard errors are in brackets. All models include fixed cross-section (State-industry) and time effects.

Table 6B. Inspections Equation: State-Specific Industry Estimations with Clustered Errors

		Pooled Negative Binomial			Linear	
Variables		Model 1	Model 2	Model 3	Model 4	Model 5
$Emission_{t-1}$				0.006		0.858
				[0.013]		[1.738]
$Inspection_{t-1}$		0.663***	0.661***	0.638***	0.043	0.04
		[0.064]	[0.064]	[0.059]	[0.063]	[0.062]
Expend			-1.283	-1.714	14.57	14.09
			[3.773]	[3.239]	[10.331]	[10.642]
Age			-0.0253	-0.065	0.059	-0.02
			[0.077]	[0.092]	[0.73]	[0.752]
Herf			-0.011**	-0.013**	0.011	0.025
			[0.0049]	[0.006]	[0.052]	[0.067]
Sales		-1.169	-1.097	29	-352.9**	572.5*
		[16.858]	[18.487]	[21.556]	[161.921]	[339.062]
Sales 2				-775.10**		-19107***
				[407.863]		[6182.883]
RD			-0.0086	-0.032**	0.092	0.293*
			[0.009]	[0.016]	[0.069]	[0.152]
RD 2				0.000006		-0.019**
				[0.001]		[0.008]
Growth			-0.00187	-0.004	-0.017***	-0.065*
			[0.002]	[0.004]	[0.005]	[0.035]
Facility		0.0263***	0.0262***	0.027***	0.867***	0.861***
		[0.0091]	[0.009]	[0.008]	[0.145]	[0.134]
Empl		5.397	6.724	20.390**	41.91	-203.5
		[5.461]	[5.792]	[9.411]	[63.04]	[137]
Empl 2				-155.90**		1743**
				[72.391]		[714.838]
Sierra			-8.34***	-7.917***	-7.375	-2.584
			[1.437]	[1.340]	[16.44]	[16.384]
Pop		0.0983	0.152	0.161	0.885*	0.897**
		[0.097]	[0.119]	[0.109]	[0.458]	[0.447]
Nrexp			-0.442	-0.457	-4.572***	-4.382***
			[0.406]	[0.380]	[1.631]	[1.406]
Income		15.14	-18.52	172.6	643.6**	1908.*
		[50.986]	[42.975]	[148.859]	[263.078]	[1128.309]
Income 2				-2808		-19590
				[2251.823]		[17928.603]
Gspmm			-0.474	-2.403	6.541	-2.346
			[4.946]	[4.20]	[31.809]	[31.439]
Repvot			-1.671***	-1.698***	-0.864	-1.992
			[0.644]	[0.596]	[2.787]	[3.595]
Strict			0.0631	0.045	0.163	0.034
			[0.109]	[0.095]	[0.496]	[0.478]
Privilege	P-ac			-0.197**		-1.315*
		-0.148***	-0.154***	[0.100]	-1.019**	[0.695]
	P-aco	[0.053]	[0.044]	-0.086**	[0.478]	-0.812
				[0.042]		[0.8405]
Immunity	I-ac			0.346***		1.791**
		0.189***	0.218***	[0.132]	1.393**	[0.704]
	I-aco	[0.084]	[0.076]	0.032	[0.609]	1.041
				[0.095]		[1.003]
Limited Immunity	LI-ab			-0.019		-0.373
		-0.0246	-0.0658	[0.089]	-0.645	[0.597]
	LI-sb	[0.09]	[0.076]	0.014	[0.555]	0.183
				[0.052]		[0.627]
Obs		17229	17229	17229	17229	17229
z/F autocorr.		1.195	1.057	1.466	86.071	91.07
R2					0.421	0.45

Note: *,**,*** denote significant at the 10%, 5%, and 1% levels (two-sided). Robust standard errors are in brackets, clustered multi-way State-by-industry (Cameron, et al., 2006). NB pooled models include state, industry and time effects. Linear models include fixed cross-section (State-industry) and time effects. Both “expanded” Models 3 and 5 include industry by time effects. Lag inspections and emissions are instrumented in the linear models.

Table 6C. Inspections Equation: Plant and State Level Data

Variables		Pooled NB (plant-level) Model 1	Model 2	Model 3	Linear (state-level) Model 4
Inspection $_{t-1}$		0.915***	0.914***	0.914***	0.0692
		[0.034]	[0.034]	[0.034]	[0.113]
Expend			-1.968	-2.173	102.9
			[5.098]	[4.792]	[187]
Age			0.281	0.284*	224.8
			[0.172]	[0.173]	[153.1]
Herf			0.00001	0.0002	-8.658
			[0.01]	[0.01]	[6.201]
Sales		10.19	13.33	13.26	-40,221
		[19.436]	[18.757]	[18.907]	[37,874]
RD			-0.014*	-0.014*	-9.76
			[0.008]	[0.008]	[7.597]
Growth			-0.004**	-0.004**	2.553
			[0.002]	[0.002]	[4.073]
Facility		0.002*	0.002*	0.002*	33.65***
		[0.001]	[0.001]	[0.001]	[7.42]
Empl		-0.312	0.811	0.789	27,052*
		[6.408]	[6.423]	[6.482]	[16,041]
Sierra			-11.810***	-11.960***	-452
			[2.124]	[2.159]	[990]
Pop		0.243*	0.334*	0.349*	9.736
		[0.142]	[0.203]	[0.212]	[20.29]
Nrexp			-0.639	-0.663	-153.1**
			[0.589]	[0.589]	[66.61]
Income		-75.73	-71	-55.54	4,965
		[52.092]	[59.237]	[55.852]	[7,526]
Gspmm			-3.44	-4.171	1,462
			[6.374]	[6.49]	[1,327]
Repvot			-0.465	-0.27	69.47
			[0.791]	[0.725]	[93.83]
Strict			0.027	0.036	-7.667
			[0.105]	[0.105]	[17.53]
	P-ac			-0.231*	
Privilege		-0.246***	-0.266***	[0.125]	-43.91*
	P-aco	[0.062]	[0.075]	-0.273***	[23.7]
				[0.106]	
	I-ac			0.531***	
Immunity		0.434***	0.474***	[0.191]	42.33
	I-aco	[0.115]	[0.136]	0.352**	[26.16]
				[0.154]	
	LI-ab			0.031	
Limited		0.056	0.039	[0.115]	-9.644
Immunity	LI-sb	[0.088]	[0.093]	0.268***	[22.1]
				[0.104]	
Obs		227141	227141	227141	696
z/F autocorr.		-0.758	-0.856	-0.961	17.677
R2					0.47

Note: *,**,*** denote significant at the 10%, 5%, and 1% levels (two-sided). Robust standard errors are in brackets, clustered multi-way State-by-industry in the plant-level regressions (Cameron, et al., 2006) and at the State level in the State aggregate regression. NB pooled models include state, industry and time effects. The State aggregate linear model includes fixed cross-section (State) and time effects, and instrumented lag inspections.