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Abstract

This paper explores empirically the impact of enforcement efforts on environmental compliance, focusing on the role of regulator reputation spillover effects. We find that, on the margin, the impact of a fine for water pollutant violations is about a two-thirds reduction in the statewide violation rate in the year following a fine. This large result obtains through the regulator's enhanced reputation; the deterrence impact on other plants in a state is almost as strong as the impact on the sanctioned plant. Focusing only on the response of the sanctioned plant, as in previous studies, may therefore seriously underestimate the efficacy of fines and other sanctions. This paper also examines the relative effectiveness of monitoring and enforcement instruments. Non-monetary sanctions contribute no detected impact on compliance, and the marginal fine induces substantially greater compliance than the marginal inspection.

JEL Classifications: K32; D83; Q25

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1. Introduction

Regulatory punishment for pollution violations is a mainstay of nearly every industrialized nation's environmental policy. A rich theoretical literature examines enforcement in general and environmental enforcement in particular, but there is a surprising lack of empirical research on the impact of fines on compliance. This paper takes up the issue, explicitly linking imposed fines and other penalties to subsequent polluting behavior. We ask two main questions. First, '*How* do fines deter environmental violations?' In particular, we examine the extent to which a sanction on a given plant spills over to deter violations at other plants through the regulator's enhanced reputation. Second, '*How much* do fines deter environmental violations?' Here, we quantify the initial improvement in environmental compliance that results from increased enforcement.

We study the efficacy of fines for two reasons. First, economists generally believe that effective regulations require regular and large monetary sanctions. Therefore, we attempt to validate and assign empirical magnitudes to important theoretical predictions. Second, there is a current policy movement away from enforcement 'with teeth' to informational and advisory enforcement. For example, recent evidence indicates that actual monitoring and sanction levels have been declining in several areas. For 1992-2002, Internal Revenue Service (IRS) criminal tax prosecutions fell by over 50 percent and the number of levies collected fell by about 80 percent. By 2000, Occupational Safety and Health Administration (OSHA) safety inspections had fallen by over 50 percent from a peak in the mid-eighties. Finally, Environmental Protection Agency (EPA) civil enforcements declined by more than one-third over the last decade.[1]

We investigate our questions for the case of conventional water pollutants, in part because water quality remains a significant issue in the US. According to the EPA, 75 percent of the US population lives within 10 miles of an impaired waterway. In addition, over 40 percent each of assessed river and stream mileage, lake acreage, and estuarine square mileage is unsafe for fishing and/or swimming [30]. Water quality is vitally important for human health, economic welfare, and ecosystem sustainability.

Our strategy is to link fines and other actual enforcement activities to subsequent compliance behavior. One naturally expects a fine would help to deter future violations by the sanctioned plant. Moreover, a fine may also credibly signal the regulator's overall willingness to levy penalties on other plants. We therefore examine both the overall reputation impact and the agent-specific impact of fines.

Our approach differs substantially from previous empirical research. Most notably, prior studies do not consider the overall regulator reputation-building impact of a levied sanction. Our results show that a single fine on one plant strongly enhances the regulator's credibility with all plants, amplifying that fine's impact. Any additional deterrence effect specific to the targeted plant is relatively small, suggesting that plants carefully observe and learn from the experiences of their neighbors. We show that other plants in a regulatory jurisdiction respond nearly as strongly to a sanction as the fined firm itself. Focusing only on the response of the sanctioned firm, as the previous literature has done, may seriously underestimate the efficacy of fines and other sanctions.

Important policy implications arise from the identification and empirical measurement of regulator reputation spillover effects. In particular, a surprisingly large increase in water quality could be achieved from a relatively small additional investment in enforcement. When reputation spillover effects are considered, a marginal fine induces an average two-thirds reduction in the statewide violation rate in the year following a fine.

Our paper also differs from previous empirical research in that it considers the contribution of fines relative to other types of monitoring and enforcement instruments. Since sanctions differ in severity, cost-effective enforcement strategies require knowledge of the relative deterrence associated with different enforcement instruments. Our results indicate that non-monetary sanctions contribute no detected impact on compliance. Further, over the observed range of monitoring and enforcement activities, the impact of the marginal inspection is small relative to that of the marginal fine.

The paper proceeds as follows. Section 2 reviews the relevant literature and explores the regulatory background of our case study industry. Section 3 discusses the data, its sources, and the assumptions involved in its collection. Section 4 examines plants' compliance decisions, and Section 5 presents the econometric models. Section 6 presents the results and interpretations, and Section 7 concludes.

2. Literature & Background

Literature

To our knowledge, all previous work on environmental compliance and plant-level enforcement has examined only the impact of enforcement activities on the sanctioned plant. The overall reputation-building impact of a levied sanction is not considered. We attempt to fill this void. Our results show that this regulator reputation spillover effect is a critically important aspect of environmental enforcement; in other words, the reputation effect highlighted in this work is the primary deterrence mechanism.

There is relatively scant empirical literature on environmental enforcement. Studies by Magat and Viscusi [12] and Laplante and Rilstone [10] investigated the impact of inspections and the threat of inspections, respectively, on the water pollution compliance rates of pulp and paper plants. Gray and Deily [5] extended the analysis to include non-monetary enforcement actions.[2] Each of these studies indicated that lagged enforcement and monitoring activity increased plant compliance. The studies did not include fines, however, the enforcement instrument believed to ultimately induce the greatest rates of compliance.

More recently, Nadeau [13] found that monitoring and enforcement activities reduced the duration of air pollution noncompliance. Fines are included in the analysis, but they are not investigated separately from other enforcement activities. Critically, Nadeau [13] also did not address the issue of cross-plant reputation building, which in our analysis is the most important deterrent effect. Kleit et al. [9] examined the determinants of regulator behavior – i.e. what factors determined whether a financial penalty is levied and what factors determined the amount of civil penalties. While this study used similar data, it contained no analysis on firm responses to regulator activities (fines or inspections). Stafford [22] showed that an increase in the maximum possible penalty decreased violations for hazardous waste pollutants. The study examined the number of violations before and after an administrative rule change, and consequently contained no data on actual enforcement or penalties. Stafford [22] looked at how potential legal liability affected compliance; we examine how actual enforcement affects compliance.

Our study, and all of the research reviewed above, focuses on the intensive margin (direct) effects induced by environmental enforcement. Other studies, however, examined extensive margin (less direct) effects. Deily and Gray [4] investigated the role of local political and economic conditions in the environmental oversight of U.S. steel mills. The results most relevant here suggested that plants predicted to incur substantial regulatory action were more likely to shut down. Stafford [21] found that state spending on environmental programs can deter plant location, and Gray and Shadbegian [6] found that firms allocated lower production shares to states with more stringent regulations.

Background

Conventional water pollutants for the U.S. pulp and paper industry are the focus of our case study. We choose this industry for several reasons. The pulp and paper industry is the largest discharger of both biochemical oxygen demand (BOD) and total suspended solids (TSS) into U.S. waterways, releasing over 16 million cubic meters of wastewater daily. Additionally, pulp (standard industrial classification (SIC) 2611), paper (SIC 2621), and paperboard (SIC 2631) mills exist in a wide range of states and fall under the jurisdiction of many different permitting authorities. Major production areas are located where raw materials (fiber-furnish) are most plentiful: the southeast, the northwest, the northeast, and the north central region.

Permitting, inspection, and enforcement activities are conducted by a variety of regulatory authorities. Most of these authorities are state environmental agencies; the rest are regional EPA offices.[3] Monthly self-monitoring reports are the primary source of compliance information. Frequent on-site regulator inspections are intended to ensure the accuracy of these self-reports. Inspections also identify maintenance issues, serve as a source of information for future permitting, and provide an avenue to gather evidence to support enforcement actions (USEPA 1990).

Enforcement actions range from levying fines to making warning telephone calls. Beyond fines, the most serious actions are considered "formal" by the EPA. The most common of these intermediate enforcement actions (IEAs) are formal administrative orders, formal notices of noncompliance, and administrative consent orders.

In the period we study, each permitting authority was required by law to inspect major dischargers at least once a year. Five types of inspections directly apply to the non-toxic discharges of a standard industry. The first type is a reconnaissance inspection. This brief inspection typically lasts less than one day, and simply involves a visual inspection of the facility, its effluent, and its receiving waters. Compliance evaluation inspections and performance audits involve a more in-depth analysis of a plant's compliance. These inspections include the visual monitoring of a plant's self-reporting records to determine accuracy and quality. Regulators check that equipment required by the permit is in place and being properly operated. Additionally, performance audits involve an inspector observing a plant's sample collection. No regulator sampling is conducted, and these inspections are likely to last between two and twelve days. The final monitoring methods, compliance sampling and bio-monitoring inspections, require approximately thirty days to complete and involve all of the actions and observations of the other types, in addition to regulator sampling.

Inspections are, to some degree, predictable. Before any regulatory monitoring can occur, the inspector must conduct a pre-inspection discussion with the management of the plant, outlining the inspection's plan. Also, specific inspections must be conducted based upon administrative factors or specific evidence of an existing violation. Historically, the vast majority of resources have been devoted to inspections motivated by administrative factors. In fact, a Supreme Court ruling requires that the EPA base its monitoring activities on "neutral selection," wherein the choice of plants to be inspected is based upon geographic factors and the length of time since the last inspection [28]. Purely random inspections are prohibited.

3. Data

The Permit Compliance System

The EPA's Permit Compliance System (PCS) serves as our data source. Established in conjunction with the Clean Water Act and its subsequent amendments, the PCS tracks monthly plant-level self-reported emissions, permitted effluent limitations, inspections, and enforcement actions. In contrast to some previous literature, compliance is observed in all periods. Specifically, ensuing inspections and sanctions are not required for the observation of violations.[4] Although the EPA administers the PCS, state agencies contribute much of its information. Our sample consists of data generated by twenty-three regulatory jurisdictions. Fifteen of these jurisdictions contain plants directly regulated by the states in which they are located. The other eight jurisdictions contain plants regulated by EPA regional authorities.[5]

Our sample of PCS data is for the period 1988-1996. The dataset contains the relevant information for biochemical oxygen demand (BOD) and total suspended solids (TSS) emissions. We consider the conventional pollutants BOD and TSS because all pulp and paper mills produce wastewater with significant amounts of these discharges. Our sample consists of 217 "major" pulp, paper, and paperboard mills in our sample states. The EPA identifies plants as major if they have a flow of one million gallons or more per day or pose a significant impact to water quality

[29]. We only consider major plants because these facilities are required to report their own emissions levels for operating pipes each month. We consider all states with four or more major pulp, paper, or paperboard mills.[6]

The 217 plants emit from 253 pipes; some plants operate multiple pipes. Our dataset records separate observations for BOD and TSS emissions. Since each plant has some degree of separate control for different pollutants and different pipes, compliance by pipe and pollutant will be the basic unit of analysis. We will, however, econometrically account for pipe and pollutant correlations.

Self-Reported Data

Although self-reporting for major plants is mandatory and our dataset is quite complete, missing data sometimes occurs in the PCS. Legally, this is allowed when an effluent pipe is closed entirely. While it is possible that plants sometimes fail to report on an operating pipe, a probit analysis of missing reports yielded no evidence of strategic non-reporting. In particular, neither lagged effluent levels nor lagged inspections predict missing data.

Another question with self-reported data is whether plants strategically misreport effluent discharges. Kaplow and Shavell [8] demonstrated that agents can be induced to report their own violations without materially affecting their incentives to refrain from violations. Further, intentional misreporting is punishable by large criminal sanctions, including jail time. These criminal penalties are borne directly by employees, unlike the effluent sanctions we study. Consequently, there are strong incentives for truthful reporting. See Cohen [3] for a more detailed discussion.[7]

The ideal test of self-reporting would be a secret and random check of effluent concentrations by the regulator. However, given the available data, only imperfect checks of the accuracy of self-reporting are possible. It seems likely that plants report truthfully in the presence of a regulatory inspector. Suppose, in contrast, that plants tend to under-report emissions when there is not an inspection. This strategic behavior would result in a positive correlation between inspections and contemporaneously reported emissions levels. It is theoretically possible that plants rapidly reduce their emissions to the average reported level when an inspector is present. This behavior would be undetected in our analysis, but any residual correlation, after accounting for exogenous covariates, indicates strategic plant behavior.[8]

In the econometric section, we therefore test whether current inspections, after controlling for possible inspection targeting, have explanatory power for reported compliance decisions. We find a rather small and statistically insignificant correlation (with t-statistics less than 1.00), suggesting the plausibility that most plants do not respond strategically to the presence of an inspector. We therefore fail to reject the accuracy of self-reporting.[9]

Nonetheless, self-reported data may be problematic, particularly when estimating the impact of enforcement-induced deterrence. For example, it is possible that plants are particularly likely to intentionally misreport if they believe they are particularly likely to face a (potentially severe) sanction for a reported violation. In this case, our estimates of enforcement impacts would be upwardly biased. However, we check whether compliance rates after levied sanctions change in the presence of an inspector.[10] For observations with fines in the past year, supplemental t-tests for equal mean compliance rates when an inspector is present versus when an inspector is absent can not be rejected. Further, it seems likely that a regulator particularly willing to severely sanction pollution violations also would be more likely to severely sanction intentional misreporting.

Sanction Data

Our analysis considers civil fines attributable to BOD or TSS non-compliance. This excludes sanctions for other types of violations such as paperwork errors, reporting errors, or poor equipment maintenance. Although enforcement actions in the PCS are not explicitly linked to particular violations, we are able to identify penalties explicitly linked to effluent violations. We therefore included all effluent sanctions preceded by one or more BOD or TSS violations in the previous year.[11]

In addition to fines, we include all intermediate enforcement actions attributable to BOD or TSS violations. Such actions include all non-fine enforcement actions that the PCS codes as "formal." Of the eleven IEA categories of action, the most common are formal administrative orders, formal notices of noncompliance, and administrative consent orders.

Summary Statistics

The reader may find the enforcement summary statistics presented in Table 1, broken down by EPA and state jurisdictions, useful. Additional details are presented below. The average number of inspections per year is approximately 1.1 (2132 inspections across 217 plants in 9 years). Eight permitting authorities levied BOD/TSS fines, and eighteen authorities levied BOD/TSS formal intermediate enforcement actions (IEAs). To check the dataset's completeness, we confirmed that all twenty-three authorities record enforcement actions of some sort, including sanctions for non-effluent violations.

Just under half of our sample plants, 99 out of 217, violated their effluent limitations at least once in the sample period. Violations occurred in all twenty-three jurisdictions. In an average month, over two percent of plants are discovered to be in violation. Violations also appear to be seasonal: one-third as many violations occur in September than occur in January. These numbers indicate that although compliance is generally high in a given month, over time the number of violations is significant.

4. Compliance Decisions

We take the standard view of the plant as a rational economic decision-maker which violates its effluent standard when the benefits of doing so exceed the expected penalties. This is consistent with the traditional law and economic framework, originating with Becker [1], Stigler

[23], and Posner [18]. See also Polinsky and Shavell [17] for a recent synthesis of literature. In our setting, marginal benefits are known to the plant and reflect increased production possibilities and decreased abatement expenditures. Marginal costs are at least partially uncertain to the plant and consist of the expected damages associated with sanctions. Plants, however, can update their uncertain beliefs by observing regulatory actions.

The relevant marginal benefit is the gain from exceeding the permitted average effluent limitation over the course of a month. Given adequate maintenance, conventional pollutant violations are rarely the result of catastrophic equipment failure, so truly accidental violations are unusual. We interpret violations as a choice variable, influenced by production level, equipment maintenance, product mix, and human attentiveness decisions.

The marginal cost is the expected sanction for noncompliance. However, one must interpret the role of any sanction with caution. The direct pecuniary costs of fines may not alone be the true economic penalty. For example, the expected cost may reflect indirect costs such as bad publicity or degraded relations with the regulator. Additionally, as Polinsky and Shavell [16] point out, it may be rational for a regulator to increase sanctions for repeat offenders. So, it is plausible that intermediate enforcement actions (IEAs), although not a direct economic sanction, may also impose costs on the plant as a signal of future fines or through indirect costs.

The regulatory environment is uncertain to the plant. Both the probability and magnitude of a sanction for a violation are imperfectly known. One reason is that local regulators have considerable discretion under the law over the existence, type, and severity of sanction. In addition, there are likely to be shocks to the regulatory system, including shocks from local political and economic conditions; see Deily and Gray [4] and Gray and Deily [5]. It is important to note that, in our sample, few detected violations are sanctioned. Indeed, one purpose of this paper is to investigate the consequences of increasing the rate and severity of sanctions.

The plant learns about the uncertain regulatory environment through experience. Our approach to learning is based on Sah's [20] work on social osmosis in crime. The main credible source of information is the actual enforcement history of the regulator. When a plant observes a fine on any plant within the same jurisdiction, it may upwardly adjust its beliefs regarding the expected sanction for one of its own violations. Similarly, if the regulator leaves several violations unpunished, the plant may downwardly adjust its expectations. The plant may draw additional idiosyncratic information from sanctions or lack of sanctions for its own violations.

Thus, plants' perceptions of the regulatory environment drive their compliance decisions. So long as plants believe the regulatory environment may be changing, they place more weight on recent experiences.[12] Consequently, the effect of past enforcement actions on current beliefs decays.

To summarize, the rational plant chooses to violate when the benefit of doing so exceeds the expected costs. Benefits are known to the plant. Costs are uncertain, but plants form expectations of sanction costs. These expectations are updated based on recent enforcement actions levied against the plant and its neighbors.

5. Econometrics

Our overall econometric strategy is to link fines and other actual enforcement decisions to subsequent compliance behavior. The most important of these actions are levying fines, imposing intermediate sanctions, and conducting inspections. Enforcement strategies vary across both states and time. For evident reasons, plants respond to the enforcement actions within their own jurisdiction only.

We examine plant-level data, as opposed to state-level data, for several reasons. The dependent variable in our analysis is a 0/1 plant-specific compliance indicator. We are then able to examine a fine's reputation spillover effects and plant-specific effects. We are also able to predict the probability of inspection for a particular plant, much like a plant itself might do. In addition, we are able to control for potential endogeneity of inspections at the plant-level. Finally, plant-level analysis allows us to better capture the effects of plant and source heterogeneity. For purposes of comparison, we later provide a simple aggregate analysis as a robustness check.

Explanatory Variables

Complete summary statistics are presented in Table 2. We begin our discussion, however, by examining fine variables. Fines may have a deterrent effect on both the fined plant and on other plants regulated by the same authority. We therefore decompose the deterrent effect of fines into two parts: A regulator reputation effect common to all plants and an idiosyncratic, plant-specific effect. The reputation spillover effect is a decrease in violations by all facilities within a regulatory jurisdiction. The idiosyncratic individual-specific effect is a decrease in violations by the particular plant fined for non-compliance. This effect might reflect increasing sanctions for plants with an offense history.

In order to capture the regulator reputation effect, we include a dummy variable indicating whether any plant within a given jurisdiction was fined in the previous year. In addition, we capture plant-specific effects by including a dummy variable indicating whether that particular plant was fined in the previous year. Because our reputation spillover effect variable includes the sanctioned plant itself, the plant-specific (own-effect) variable measures the supplemental impact of sanctions on the fined plant, above and beyond a generic reputation effect common to all plants. We suspect that the deterrent effects of a fine may decay over time; therefore, we include all fine variables lagged an additional year.

One might expect that the magnitude of a fine, as well as its mere existence, impacts compliance decisions. A sanction's magnitude goes directly to the expected cost of a violation. So, in a parallel analysis we replace the fine dummies with corresponding magnitude variables. These variables are expressed as the logged sum of fines.[13] The regulator reputation effect variables are the logged sum of fines on *all* plants in the jurisdiction in the past year and lagged one additional year. Similarly, the plant-specific variables are the logged sum of fines on *the* particular plant in the past year and lagged an additional year.

Our second set of enforcement variables is the number of intermediate enforcement actions (IEAs), such as formal notices of non-compliance. Parallel to the fine analysis, we include both regulator reputation and plant-specific IEA variables lagged one and two years. These variables are the count of IEAs in the relevant time period.

We also consider the impact of inspections at the plant-level. Even without the threat of a sanction, inspections may prevent some violations. For example, an inspector may notice an easily correctable problem. Additionally, inspections are often a necessary precursor to the levying of sanctions. So, we include a variable for the number of inspections lagged one year and two years. We also incorporate the predicted probabilities of an inspection, calculated from a probit regression of possible determinants, such as the time since last inspection. Finally, we include a dummy for current inspections as an explanatory variable, which allows us to examine whether plants respond strategically to the presence of an inspector. This is the basis of our check of self-reporting anomalies discussed in the data section.

We incorporate several other explanatory variables. First, we include a dummy variable with a value of 1 when the effluent is BOD, and a value of 0 for TSS. To capture changes in the plant's technology over time, we use the ratio of actual to permitted emissions lagged 12 months.[14] We also include a corresponding dummy to allow for pipes closed 12 months prior. Production capacity for the plant, gathered from an industry directory [11] is also a covariate since large plants may enjoy economies of scale in abatement or may be more visible targets for enforcement actions. We incorporate a corresponding dummy for the few plants where the capacity data was missing from the directory.

We also include community characteristics. Although the most direct role for these factors is through influence on the previously modeled rigor of enforcement, it is possible such characteristics may affect firms through community pressure, citizen suits, and similar avenues.

From the 1990 United States Census, we include per capita income, median housing values, percent urban, and percent white at the county level. From the U.S. Department of Labor, Bureau of Labor Statistics, Local Area Unemployment Statistics, we include monthly unemployment data at the county level.

Finally, we include several additional control variables. State-level dummy variables control for heterogeneity across authorities, and year dummies capture time trends and some additional unobserved heterogeneity.[15] Seasonality terms control for variability in production rates over the course of a year. The Bureau of Labor Statistics' producer price index, PPI, accounts for monthly variation in output price and the U.S. Department of Commerce, Bureau of Economic Analysis' annual gross state product for paper and allied trades controls for production fluctuations. Lastly, we include dummies for a plant's standard industrial classification, SIC codes, since potentially important differences in technologies and compliance costs may exist across our 3 industrial sub-categories.[16]

Regression Model

The decision to violate is a dichotomous choice of the type typically estimated using the familiar probit analysis. The latent variable is expected profits conditional on a violation minus expected profits conditional on no violation. This model is sensible even if a random event, such as equipment failure, causes the plant to violate. Such shocks are included in the error process, and can be interpreted as an extremely high cost of compliance for that period.

For latent variable y*, the basic model is $y_{it}^* = \mathbf{X}_{it}\boldsymbol{\beta} + \alpha_i + \varepsilon_{it}$. Here, i indexes the unit of observation (a pollutant/pipe combination), and t indexes time (months). A violation occurs, $y_{it} = 1$, if $y_{it}^* > 0$. Compliance occurs otherwise, $y_{it} = 0$. The term α_i can be thought of as a time-invariant random effect. The term ε_{it} is the usual idiosyncratic shock, which may be serially correlated over time. Since the unit of analysis is a given pollutant at a given pipe, it is important to account for correlations across pipes and pollutants within a plant for both the random effect α_i and the idiosyncratic shock ε_{it}. For the special case of all within-plant correlations equal to one, this empirical model nests an alternative specification with strictly plant-level observations, rather than pipe/pollutant observations.

Consistency Considerations

In order to produce consistent estimates, careful attention must be paid to the model's error structure. There are several potential sources of inconsistency. First, the time-invariant random effect α_i may be correlated with the row vector of explanatory variables $\mathbf{X}_{it}$. The time-varying shock ε_{it} may similarly be correlated with $\mathbf{X}_{it}$. Further, if serial correlation exists in ε_{it}, even lagged inspections may then be correlated with the current error term. Any of these correlations would produce asymptotically biased estimates.

The time-invariant element α_i accounts for plant heterogeneity. Since this random effect partially reflects variation in plants' costs of compliance, it is likely that this term is correlated with the average inspection rate for that plant, as well as other regulatory variables. For example, consider the possibility that regulators frequently inspect plants that generally seem to be more likely to violate. Helland [7] finds evidence for such targeting. Failure to account for targeting may produce bias. In a linear model, one could control for this problem with fixed effects. Unfortunately, including fixed effects in a panel probit regression yields inconsistent estimates of the slope coefficients.

We control for this possible bias using Chamberlain's [2] conditional random effects (CRE) probit model. Since our goal in introducing plant-specific effects is to control for missing variables potentially correlated with the explanatory variables, the Chamberlain approach specifies a distribution for α_i conditional on $\mathbf{x}_i$. Our density function for y becomes marginal on

α_i, and the subsequent random effects specification yields consistent estimators for our original parameters of interest. This allows us to estimate the effect of changes in important explanatory variables while ‘sweeping out’ their potentially correlated time averages.

Conditional random effects are persistent effects at the plant-level, and they generally achieve the same intuitive outcome as fixed effects. Instead of conditioning on the sample averages of all variables (fixed effects), CREs condition on the sample averages of the variables of most direct theoretical concern. So, in our context, we condition the distribution of the error term’s persistent component on the average value of inspections for that plant over the entire sample period. In practice, this control is implemented by including the average inspections for the plant in the same fashion as an explanatory variable; the interpretation, however, is different than a standard covariate. We similarly apply this conditional random effects correction to account for heterogeneity reflected in fines, IEAs, and emissions as well. Thus, the average values by pipe of current inspections, fines, IEAs, and the ratio of actual to permitted emissions are included as conditioning variates in the plant-level regressions. Consequently, time variation, but not cross-sectional variation, contributes to identification of the corresponding variables’ coefficients. For reference, a fixed effects specification would have a similar feature, but cross-sectional variation would not contribute to the identification of any explanatory variables’ coefficients.[17]

A second source of potential inconsistency is correlation between the time-variate error term ε_{it} and the explanatory variables $\mathbf{X}_{it}$. Again, our concern is the inspection process. It is possible that a regulator may inspect a given plant more frequently when that plant is more likely than usual, given the other explanatory variables, to be out of compliance. While the conditional random effects approach controls for general inspection targeting of a plant, it does not control for variation in idiosyncratic targeting over time.

Instrumental variables estimation is the standard approach to control for this type of correlation. The obvious difficulty is identifying valid instruments. We need variables correlated with inspections, but not associated with idiosyncratic targeting. Our chosen instrument is the rate of inspections on other plants in the same jurisdiction for that month.[18] Changes in the inspection rate on other plants partially reflect changes in the overall inspection rate within a jurisdiction. So, inspections on a given plant should be positively correlated with the corresponding instruments. We believe that our instrument is not affected by idiosyncratic targeting because the pulp and paper industry is only one component of the various regulators' monitoring responsibilities. Therefore, an additional targeting inspection at a given plant does not necessarily imply one fewer inspection at other plants.[19]

We also use instruments for the first year of lagged inspections. We find evidence for serial correlation of no more than three months. The correlation between current and lagged residuals rapidly declines from about 0.3 in the first lagged month to about zero by the fourth lag. Given serial correlation, it seems possible that lagged inspections are correlated with the current error process. For example, if current inspections are correlated with εit and εit is correlated with ε_{it+1}, then this month's inspections may be correlated with next month's error term. Intuitively, suppose that the regulator conducts a targeting inspection when it suspects that a plant is unusually likely to be in violation. Since we find positive serial correlation for up to three months, the plant may also be unusually likely to violate one or two months later. Therefore, the first year of lagged inspections may be correlated with the current error term.[20]

Efficiency Considerations

The consistency controls discussed above are crucial; however, efficiency should also be a consideration. We therefore explicitly model the persistent shocks and serial correlation, and the overall likelihood function is the product of the probabilities of the observed outcomes for each plant. Our estimation technique is maximum likelihood.[21] The observations for a particular plant are not assumed to be statistically independent across observations because we allow for plant-level random effects and serial correlation. In this case, the likelihood function is difficult to evaluate numerically with precision because the joint likelihood of outcomes at a plant contains a high-dimensional integral. We can, however, obtain a good estimate by using Monte Carlo techniques to approximate numerically the likelihood (Pakes and Pollard [15]). Specifically, we utilize Stern's [24] factor-analytic simulator because it is well-suited to our correlation structure. The reader should note that neither the simulated likelihood technique nor the specific simulator itself changes the underlying estimation; we are simply employing a numerical technique for evaluating the likelihood function.

Regressions

To summarize, the basic latent variable model is $y_{it}^* = \mathbf{X}_{it}\boldsymbol{\beta} + \alpha_i + \varepsilon_{it}$, where i indexes the unit of observation (a pollutant/pipe combination) and t indexes time (months). If $y_{it}^*>0$, a violation occurs. The columns of the matrix X include all of the explanatory variables discussed above. The most important of these variables are fines lagged one and two years, IEA's lagged one and two years, contemporaneous inspections, predicted probabilities of inspection, and inspections lagged one and two years.

Key plant-level coefficients are presented in Table 3. The sample consists of 32,953 observations from 253 distinct effluent pipes from our 217 plants over the 84 months spanning 1990-1996. Each of these observations reflects emissions of one pollutant type at one pipe for one month. The pseudo-R^2 is approximately 0.08, calculated with state-level fixed effects in the restricted regression. We consider two additional goodness-of-fit measures. The predicted probability of violation for observations with observed noncompliance is over four times greater than the predicted probability of violation averaged over all observations. Similarly, for each plant, we examine the relationship between predicted probabilities of violation during periods of observed non-compliance versus predicted probabilities for periods of observed compliance. On average, the predicted probability of violation for a given plant, when actually violating, is approximately twice the predicted probability of violation (for that same plant) when complying.

The multiple columns of Table 3 present different plant-level specifications. Column one presents results from a naïve probit analysis for the specification with fine dummies, and column three similarly presents results from a naïve probit analysis for the specification with logged magnitude of fines. These particular regression results are provided for reference; they do not contain the important consistency and efficiency controls discussed in the preceding subsections.

Columns two and four of Table 3 present our final regression results with full controls. We implement conditional random effects by including the means, by plant, of current inspections, fines, IEAs, and the lagged ratio of actual to permitted emissions. As discussed, we also incorporate an instrumental variables correction for current inspections and inspections lagged one year following the method of Nelson and Olson [14]. Finally, the error structure accounts for third-order serial correlation, plant-level random effects, and correlation between BOD and TSS emissions within a given plant.

We defer interpretation of plant-level regression coefficients relating to sanctions and inspections to section 6. Our regressions also include control variables such as annual fixed effects, plant capacity, and community characteristics; we briefly discuss them here. Coefficients indicate that compliance rates increase over time and smaller plants are more likely to violate. Significant differences in compliance exist across industrial sub-categories, with paperboard mills (SIC 2631) complying most frequently, followed by paper mills (SIC 2621), and finally pulp mills (2611). We omit state dummies and seasonality controls from Table 3 to conserve space, but all affect the regression results.

Community characteristics are generally insignificant. We find this result unsurprising, because the most direct role for community characteristics is through influence on the rigor of enforcement. Since enforcement is already accounted for in the regression, community characteristics in general add little power to predicting violations. Unemployment is an exception.

Monthly county-level unemployment is significant with a negative sign. This result is perhaps counterintuitive. The most obvious reason for higher unemployment to be correlated with higher compliance is that unemployment is serving as a proxy for production.[22] However, we already include state-level gross state product for pulp and paper industries in the regression, which turns out to be insignificant. Another possible interpretation is that high levels of unemployment result in an increased sensitivity to plants' polluting and social behavior. A more detailed examination is beyond the scope of this paper, and is a promising subject for future research.[23]

6. Results and Interpretation

In this section, we present and discuss the results of our econometric model, as summarized in the second and fourth columns of Table 3. We first focus on our fine variables. In addition to the standard discussion of coefficients and statistical significance, we translate our fine results into a more readily interpretable form. Specifically, we present the results of an experiment to approximate the marginal impacts of individual fines on overall compliance. We then examine our IEA and inspection results. After these discussions, we consider the robustness of our regressions.

Several of our fine variables are strongly significant. Since the probit specification is non-linear, we perform a simple numerical experiment to examine their marginal impacts. For example, to examine an observed fine's effect, we compare a state's average predicted probability of violation with the fine against that state's counterfactual predicted probability in the absence of that fine. Each observed fine provides an opportunity to conduct an experiment. The final impact, then, is the mean over all such experiments.

Enforcement Variables

The coefficient on any plant's recent fine is negative and strongly significant in both the fine dummy and fine magnitude analyses. Coefficients on fines 13-24 months ago are also negative, but they are approximately one-half as large as their more recent counterparts. We refer the reader to Table 3.

Experimentally interpreting the analysis that includes the fine dummies, we find that the regulator reputation-based deterrence component of an additional fine induces an approximately 64 percent reduction in the statewide probability of a violation in the year following a fine.[24] We emphasize that this is an average reduction for all plants within a state; the impact is not limited to a single plant. This reputation signaling effect declines to about 27 percent in the second year. Reputation spillover effects do differ somewhat across states. The experimentally induced changes in statewide violation rates for the year following a fine ranged from 31 percent to 75 percent, and the experimental standard deviation is approximately 13 percent.

For the parallel analysis, the regulator reputation-based deterrence component of a 1.0 percent increase in fine magnitude induces a 0.11 percent decrease in the statewide probability of a violation in the year following a fine. The standard deviation over all such fine magnitude experiments is 0.03 percent, and the induced changes range from 0.04 percent to 0.14 percent. This reputation effect declines to an approximate 0.04 percent decrease in the violation probability in the second year. Overall, these results are consistent with the analysis that includes fine dummies. Interpreting the fine magnitude specification results with the same numerical experiment employed for the fine dummy specification, we find similar results. Based upon reputation spillover effects alone, on average, the additional fine induces a 61 percent reduction in the statewide probability of violation in the year following a fine. This reputation signaling effect declines to about 30 percent in the second year.

In contrast to the strong reputation effects, we find little evidence of idiosyncratic, individual-specific deterrence effects contributing to plants' enforcement expectation updates. Table 3 reveals that coefficients on a plant's own fine are negative, yet insignificant, in both final regression specifications. Given the lack of statistical significance, reliable interpretation is difficult. For comparison's sake, however, we report the following experimental outcome. The addition of the plant-specific component drives the total deterrence of the average marginal fine to a 67 percent reduction in the statewide probability of a violation in the year following a fine. This is only slight stronger than the 63 percent decrease due to the reputation based component.

We detect no impact of less severe intermediate enforcement actions on environmental compliance. Table 3 reveals that all final regression coefficients on IEA variables are statistically insignificant. Further, some results are positive and some are negative. Since the coefficients are not significant and there is no obvious sign pattern, we do not present marginal impacts.

Of course, some means of verification of emissions is necessary in any enforcement system. Therefore, one expects inspections to play an important overall role. Our analysis, however, focuses on the effects of a marginal inspection, since we can only identify impacts over the observed range of variation in the data.

The coefficients on inspections 1-12 months ago are negative and significant in both final specifications. Coefficients on inspections 13-24 months ago are one-sixth as large as the one year effect, and not statistically significant. The marginal inspection plays a role in environmental compliance, but the impact decays rapidly. This seems reasonable since one of the purposes of inspections is to identify maintenance issues.[25]

Sensitivity Analysis

Are the final regression results presented in Table 3 reasonable? The most naïve approach we can think of is a simple comparison of observed statewide violation rates in the year after a fine to observed rates when there was no fine. Clearly this simple experiment ignores many economic and econometric issues; however, this approach is likely to pick up any large signal in the data. Running this comparison, we find the statewide violation rate is on average 62 percent lower in years subsequent to a fine than in other years. For purposes of comparison, our regression results suggest about a two-thirds reduction.

A somewhat more sophisticated, but still simplistic approach, would be to run a state-level linear probability model with multiple covariates. This aggregate analysis allows us to examine the average plant impact of enforcement actions by the permitting authority. Using a fixed-effects panel model, we are able to identify the plants' short-run responses to changes in a permitting authority's enforcement strategy. Since this is a fixed-effects regression, identification comes from within-group variation (the time-series), rather than between-group variation (the cross-section).[26]

Our state-level linear probability estimates are from a GLS regression. The standard errors are robust, using heteroskedastic-consistent correction and a correction for serial correlation. As in the plant-level analysis, we again find large and significant fine impacts, undetected IEA compliance effects, and, in this case, insignificant inspection results.[27] These aggregate regression results are presented in Table 4.

While illustrative, this approach has a few key weaknesses. For example, because data are pooled to the state level, we are unable to disentangle a fine's reputation spillover effect from plant-specific effects. We are also unable to control for the potential endogeneity of inspections. Finally, the more detailed analysis provides the opportunity to better capture the effects of plant and source heterogeneity.

Since our key variable of interest is fines, in our final plant-level analysis we provide two alternative specifications for fine impacts. Specifically, we have presented regressions with a dummy for the presence of fines and also the logged magnitude of fines. Results across these specifications are qualitatively similar; the corresponding fine variables in each specification display similar relative magnitudes and levels of significance. Other coefficients were also qualitatively similar.

A natural concern is that we may be inflating statistical significance by including observations at the pipe/pollutant level, instead of at the plant level. Since our likelihood function includes correlation, the reported standard errors (from the inverse log-likelihood Hessian) should correctly account for this issue. However, as a robustness check, we replicated our results aggregating up to the plant-level, where the new dependent variable is overall plant compliance. Results are quite similar; all enforcement variables have similar coefficients and significance patterns.[28]

Self-reported data may be problematic in the context of estimating deterrence. In the data section, we discussed this issue and the statistical tests that found no evidence of strategic misreporting. In particular, the instrumented coefficient on current inspections is not statistically significant. Here, we present a final robustness check. Our current specification is both efficient and consistent under the null of truthful reporting. Under the alternative hypothesis of strategic reporting, a less efficient but consistent estimator would be a selectivity-corrected probit using only those observations for which an inspector is present. We compare these estimators using a Hausman test of equality and again fail to reject the null hypothesis of accurate self-reported emissions.

Our final sensitivity consideration in the plant-level probit analysis is the impact of conditional random effects and instruments. Conditional random effects are included to account for conditional heterogeneity of the error term. As seen in Table 3, each of the conditional random effect coefficients came in positive and strongly significant. For example, examining the coefficient on the mean of current inspections indicates that, at least in the long term, authorities more frequently inspect those plants that are more likely to be out of compliance. So, we would generally expect regressions without these controls to exhibit a positive bias in the corresponding coefficients. We confirm this expectation by examining the naive plant-level analyses presented in columns one and three of Table 3. We observe that all significant coefficients on fines, IEAs, and inspections are in fact positively biased (or unchanged) without the appropriate controls. A similar correction was the inclusion of an instrument for inspections. As can be seen in the table, inspections were positive in the absence of the instrument.

7. Conclusions

On the margin, a fine produces a surprisingly large decrease in violation rates, on the order of about a two-thirds reduction. The majority of this impact can be attributed to reputation enhancement by the regulator; other plants reduce violations almost as dramatically as the fined plant. We find the strength of this reputation spillover effect to be quite surprising.

In contrast, intermediate enforcement actions have statistically insignificant impacts on compliance. It is important to note that this small influence obtains even allowing for reputation impacts. While one theoretical argument may be that IEAs induce compliance by establishing a plant's offense history or serving as a precursor to fines, it is perhaps not surprising that their empirical impact is weak. They are not themselves meaningful monetary sanctions.

Striking policy implications arise from our results. The marginal impact of a fine is large because of the amplification provided by the regulator reputation spillover effects. In contrast, non-pecuniary actions, such as IEAs (informational enforcement), have little impact with or without a reputation effects. Empirically, large improvements follow even from modest sanctions, as long as they have economic 'teeth.' Consequently, a substantial improvement in water quality might be achieved from a relatively small additional investment in traditional adversarial enforcement. Given this result, it is perhaps an interesting institutional research question why fines are not imposed with greater regularity.

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Footnotes

[1] IRS and EPA enforcement data was obtained through the Transactional Records Access Clearinghouse [25] at Syracuse University. OSHA enforcement data was obtained from the U.S. Department of Labor, Office of the Assistant Secretary for Policy [26].

[2] Gray and Deily [5] also investigated whether plant compliance rates influence regulatory behavior and whether plant characteristics influence compliance or regulator behavior. These are important contributions, but not directly relevant to this study.

[3] States have the option to oversee compliance. EPA regional offices step in for states which decline this option.

[4] Violations are frequently reported in months with no inspections or ensuing sanctions.

[5] We disaggregate the EPA regional offices into the eight states they represent because regulatory information may not necessarily flow freely between states. MA, ME, NH, and TX contain plants regulated solely by the EPA regional offices. AR, LA, NC, and PA contain some plants that are regulated by state permitting authorities and some plants regulated by EPA regional offices.

[6] We exclude states with very few plants for two reasons. First, we are particularly interested in cross-plant regulator reputation spillovers within a state. Second, we need multiple plants to construct the instruments used in the statistical analysis.

[7] The Center for Environmental Information and Statistics has also performed an independent analysis of the reliability of PCS data. [27] They conclude that emissions, limit, inspection, and enforcement data are accurate.

[8] Additionally, it may be possible that the regulator has some signal of plants' discharges and conducts inspections during periods of high emissions. Such targeting would tend to produce a positive correlation. However, in the empirical section we control for this possibility.

[9] Similarly, Laplante and Rilstone [10] conduct a comparison of means test on Canadian pulp and paper emissions when inspectors are present or absent. They also fail to reject the accuracy of self-reporting.

[10] Ideally, we could distinguish sampling-specific inspections from more general inspections. Our dataset does not include this detail, but our understanding is that about 40% of pulp and paper inspections overall conduct some bio-monitoring or pollutant sampling.

[11] We were able to track down legal records for several of these sanctions. In each case, the sanctions were in fact for BOD and TSS violations.

[12] Note that this does not require that the regulatory strategy is actually changing, just that plants are uncertain enough to believe that the strategy may be changing.

[13] To be precise, all fine magnitude variables are log {magnitudes+1}.

[14] Magat and Viscusi [12] suggest this measure as a proxy for the plant's stock of capital related to pollution control and for the general character of its abatement technology.

[15] The year dummies provide a different constant for the first month of each year. The effective constant for the intervening months is produced by a linear interpolation. Technically, this is a linear-spline time trend with one knot per year.

[16] While we do have plants from three industrial sub-categories (SIC codes 2611, 2621, and 2631), it is our belief that the regulatory agency is likely to treat each plant type similarly. In fact, the EPA's enforcement and inspection guidelines are issued for the pulp and paper industry as a whole. For this reason, we do not run separate regressions across SIC codes.

[17] One can think of Chamberlain's [2] model as a statistically consistent generalization of the traditional fixed effects for latent variable models. The relationship between these approaches is most clear if one considers the Hausman-Taylor instrumental variables equivalent of a traditional linear fixed effects model.

[18] Two of the twenty-three jurisdictions contain only one plant, so we drop these two from the plant-level analysis.

[19] An alternative argument is that there is a predetermined number of inspections per period. If this were the case, our instrument would be weakly correlated with the targeting component, because a targeting inspection at a given plant would imply one fewer inspection at other plants. The data, however, suggests that there seems to be no fixed number of inspections per period for any state. This makes sense because regulators have many other industries to inspect. However, if there were a predetermined number of inspections, then the total number of inspections within a jurisdiction, including those on the plant of interest, would be a valid instrument. As a robustness check, we ran our regressions with this alternate instrument. Results do not vary substantively from reported results.

[20] We do not feel this concern applies to sanctions because of the time lag between a violation and its associated sanction. The mean lag between a violation and the imposition of a fine exceeds five months.

[21] An alternative, but less efficient, estimator would be to obtain consistent estimates with an independent probit specification. Standard errors could then be estimated using a method of moments interpretation of the probit scores. This approach would be intuitively analogous to running OLS with correlated errors, and then correcting the standard errors.

[22] In Washington, for example, average plant employment is 220 and nearly 70% of employees work at plants with more than 500 people. However, it is difficult to assess the total local employment resulting indirectly from plant operations.

[23] Reassuringly, inclusion of community characteristics does not qualitatively affect the signs, magnitude, or statistical significance any of the enforcement variables.

[24] In order to maintain parallel structure, we report spillover effects that separately count compliance decisions for each pipe/pollutant combination. As these effects are reported in percent-change form, this choice should be generally innocuous. The selection does place slightly more weight on larger, multi-pipe plants. This seems appropriate to us from both an enforcement and environmental impact perspective.

[25] We also checked whether enforcement variables impact the severity of violations, *in addition* to their impact on compliance decisions. We performed a Heckman two-step estimation, where the

second stage regressed the violation amount on our same explanatory variables. Enforcement variables were not statistically significant.

[26] The fixed-effects model removes any potential bias introduced if there was a specific regulatory reason that some plants are regulated by the EPA and some are regulated by state jurisdictions.

[27] In this aggregate regression, we are unable to provide instruments for inspections.

[28] The only change of significance was for inspections in the past year. The t-stat has fallen (in magnitude) to -1.66.

Table 1. Enforcement Summary Statistics by Permitting Authority

Variable	States	EPA Regions	Total
Authorities	15	8	23
Plants	172	45	217
Violations	299	124	423
Fines	22	2	24
Fine Average	$43,500	$97,500	$48,000
Fine Maximum	$600,000	$100,000	$600,000
Fine Minimum	$500	$95,000	$500
IEAs	28	16	44
Inspections	1,718	414	2,132

Table 2. General Summary Statistics[a,b]

Variable Description	Mean	Standard Deviation
Compliance in this period (dummy)	0.012	0.11
Fine 1-12 months ago on anyone (dummy)	0.010	0.30
Fine 1-12 months ago on self (dummy)	0.014	0.12
Fines 1-12 months ago on anyone (log magnitude)	1.010	3.08
Fines 1-12 months ago on self (log magnitude)	0.137	1.17
IEAs 1-12 months ago on anyone	0.336	0.67
IEAs 1-12 months ago on self	0.305	0.20
Predicted inspection probability	0.120	0.10
Inspection this month	0.120	0.33
Inspections 1-12 months ago	1.484	1.06
BOD or TSS (dummy)	0.479	0.50
Producer Price Index	1.345	.217
Ratio of Actual to Permitted Emissions	0.354	0.30
Capacity (million tons)	0.798	.821
Standard Industrial Classification Code 2611 (pulp)	0.369	0.48
Standard Industrial Classification Code 2621 (paper)	0.476	0.50
Standard Industrial Classification Code 2631 (paperboard)	0.155	0.36
Gross State Product for Pulp and Paper (billions of dollars)	2.159	1.17
County Per Capita Income (thousands of dollars)	11.83	2.12
County Percent Urban	26.4	33.3
County Percent White	85.0	16.5
County Unemployment Rate	7.34	3.03
County Median House Value (thousands of dollars)	62.14	24.1

[a] The sample consists of 32,953 BOD and TSS observations from 253 distinct effluent **pipes** over the 84 months spanning 1990-1996. 217 **plants** are represented.
[b] State-level fixed effects, year dummies, and seasonality corrections are omitted to conserve space. Dummies indicating missing capacity data and pipe closure data (for ratio of actual to permitted emissions data) are also omitted.

Table 3. Important Coefficients and t-statistics for the Plant-Level Regressions [a]

Variable Description	Regressions with Fine Dummies: Naïve Regression [b]	Regressions with Fine Dummies: **Final Regression** [b]	Regressions with Fine Log Magnitudes: Naïve Regression [b]	Regressions with Fine Log Magnitudes: **Final Regression** [b]
Fine 1-12 months ago on anyone (dummy)	-0.495	**-0.509***		
	(-4.23)	**(-3.75)**		
Fine 13-24 months ago on anyone (dummy)	-0.171	**-0.145**		
	(-1.52)	**(-1.10)**		
Fine 1-12 months ago on self (dummy)	0.267	**-0.066**		
	(1.51)	**(-0.32)**		
Fine 13-24 months ago self (dummy)	0.104	**-0.188**		
	(0.56)	**(-0.86)**		
Fines 1-12 months ago on anyone (log magnitude)			-0.045	**-0.046***
			(-4.10)	**(-3.72)**
Fines 13-24 months ago on anyone (log magnitude)			-0.018	**-0.016**
			(-1.63)	**(-1.26)**
Fines 1-12 months ago on self (log magnitude)			0.019	**-0.014**
			(1.06)	**(-0.66)**
Fines 13-24 months ago on self (log magnitude)			0.008	**-0.019**
			(0.44)	**(-0.86)**
IEAs 1-12 months ago on anyone	0.020	**0.048**	0.019	**0.048**
	(0.57)	**(1.19)**	(0.53)	**(1.22)**
IEAs 13-24 months ago on anyone	-0.043	**-0.027**	-0.043	**-0.018**
	(-1.06)	**(-0.38)**	(-1.06)	**(-0.39)**
IEAs 1-12 months ago on self	0.287	**0.074**	0.291	**0.076**
	(4.06)	**(0.76)**	(4.12)	**(0.78)**
IEAs 13-24 months ago on self	0.076	**-0.078**	0.079	**-0.076**
	(0.84)	**(-0.65)**	(0.87)	**(-0.63)**
Predicted inspection probability	0.168	**0.367**	0.168	**0.363**
	(0.61)	**(1.24)**	(0.61)	**(1.23)**
Inspection this month [c]	0.052	**0.231**	0.052	**0.238**
	(0.81)	**(0.55)**	(0.81)	**(0.57)**
Inspections 1-12 months ago [c]	0.091	**-0.185***	0.091	**-0.181***
	(4.03)	**(-2.50)**	(4.02)	**(-2.45)**
Inspections 13-24 months ago	-0.021	**-0.030**	-0.022	**-0.031**
	(-0.88)	**(-1.06)**	(-0.88)	**(-1.10)**

[a] The plant-level sample consists of 32,953 BOD and TSS observations from 253 distinct effluent pipes over the 84 months spanning 1990-1996. The dependent variable is the 0/1 compliance decision for each pipe/pollutant/month combination. For brevity, state dummies, industrial classification dummies, and seasonality corrections are omitted.

[b] Naïve regressions represent simple probit estimations. Final regressions incorporate all necessary controls: instrumented inspections, conditional random effects controls, and correlated error structures.

[c] For the final regressions with full controls, contemporaneous inspections and inspections lagged one year are instrumented as described in the text.

* Significant at the 5 percent level in final regressions.

Table 3 (Continued). Important Coefficients and t-statistics for the Plant-Level Regressions [a]

Variable Description	Regressions with Fine Dummies: Naïve Regression [b]	Regressions with Fine Dummies: **Final Regression** [b]	Regressions with Fine Log Magnitudes: Naïve Regression [b]	Regressions with Fine Log Magnitudes: **Final Regression** [b]
Emissions ratio 12 months ago	0.431	**0.348***	0.432	**0.346***
	(9.47)	**(7.02)**	(9.52)	**(6.99)**
Pipe closure 12 months ago	0.256	**0.294***	0.259	**0.297***
	(3.63)	**(3.71)**	(3.67)	**(3.75)**
Plant capacity (kilotons)	-0.117	**-0.176***	-0.117	**-0.170***
	(-2.96)	**(-3.60)**	(-2.95)	**(-3.50)**
Plant capacity unknown	-0.060	**-0.082**	-0.061	**-0.073**
	(-0.86)	**(-1.03)**	(-0.88)	**(-0.94)**
Pollutant type (BOD or TSS)	0.266	**0.292***	0.267	**0.291***
	(6.14)	**(5.73)**	(6.15)	**(5.73)**
Producer Price Index (PPI)	-0.307	**-0.444**	-0.318	**-0.456**
	(-1.92)	**(-1.88)**	(-1.99)	**(-1.93)**
Gross State Product	0.133	**0.063**	0.137	**0.068**
	(0.83)	**(0.37)**	(0.86)	**(0.39)**
County Per Capita Income	-0.037	**-0.033**	-0.037	**-0.034**
	(-1.12)	**(-0.85)**	(-1.11)	**(-0.87)**
County Percent Urban	-0.000	**-0.002**	-0.000	**-0.002**
	(-0.34)	**(-1.51)**	(-0.31)	**(-1.49)**
County Percent White	-0.011	**-0.005**	-0.011	**-0.005**
	(-3.66)	**(-1.42)**	(-3.70)	**(-1.46)**
County Unemployment Rate	-0.042	**-0.040***	-0.041	**-0.040***
	(-4.00)	**(-3.50)**	(-3.96)	**(-3.50)**
County Median House Value	-0.001	**0.003**	-0.001	**0.003**
	(-0.27)	**(0.86)**	(-0.28)	**(0.87)**
Conditional Random Effects				
Mean of current inspection		**2.118***		**2.168***
		(2.90)		**(2.97)**
Mean of Fine		**1.210***		**0.099***
		(2.81)		**(2.31)**
Mean of IEA		**1.293***		**1.296***
		(4.71)		**(4.72)**
Mean of emissions ratio		**1.031***		**1.067***
		(5.43)		**(5.63)**

[a] The plant-level sample consists of 32,953 BOD and TSS observations from 253 distinct effluent pipes over the 84 months spanning 1990-1996. The dependent variable is the 0/1 compliance decision for each pipe/pollutant/month combination. For brevity, state dummies, industrial classification dummies, and seasonality corrections are omitted.
[b] Naïve regressions represent simple probit estimations. Final regressions incorporate all necessary controls: instrumented inspections, conditional random effects controls, and correlated error structures.
[c] For the final regressions with full controls, contemporaneous inspections and inspections lagged one year are instrumented as described in the text.
* Significant at the 5 percent level in final regressions.

Table 4. Important Coefficients and t-statistics for the Aggregate Linear Probability Model [a]

Variable Description	Fine Dummies	Fine Logged
Fine 1-12 months ago (dummy)	-0.0125*	
	(-2.82)	
Fine 13-24 months ago (dummy)	-0.0080	
	(-1.76)	
Fine 1-12 months ago (logged magnitude)		-0.0012*
		(-2.89)
Fine 13-24 months ago (logged magnitude)		-0.0008
		(-1.92)
IEAs 1-12 months ago	-0.0143	-0.0139
	(-0.50)	(-0.49)
IEAs 13-24 months ago	-0.0445	-0.0444
	(-1.69)	(-1.69)
Inspection Rate this Month	0.0030	0.0030
	(0.44)	(0.44)
Inspection Rate over Last 12 Months	-0.0026	-0.0025
	(-0.67)	(-0.66)
Inspection Rate 13-24 Months ago	-0.0004	-0.0005
	(-0.11)	(-0.13)

[a] The state-level sample consists of 1932 observations representing 23 permitting authorities over the 84 months spanning 1990-1996. The dependent variable is violation rate for that authority/month combination. For brevity, state dummies, time, and seasonality corrections are omitted from the table.