Enforcement and over-compliance

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Abstract

Economists generally view environmental enforcement as a tool to secure compliance with regulations. This paper demonstrates that credible enforcement significantly increases statutory *over*-compliance with regulations as well. We find that many plants with discharges typically below legally permitted levels reduce discharges further when regulators issue fines, even on other plants. Also, non-compliant plants often respond to sanctions by reducing discharges well beyond reductions required by law. Thus, increased enforcement generates substantial discharge reductions above and beyond those expected from simply deterring violations.

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1. Introduction

Regulatory punishment for pollution violations is a mainstay of nearly every industrialized nation's environmental policy. Economists generally view such enforcement as a tool to secure compliance. This paper empirically demonstrates that enforcement can significantly increase the degree of statutory *over*-compliance with environmental regulations as well. We show that this effect can be economically rational given discharge randomness or discharge jointness.

Previous research has demonstrated high levels of statutory compliance with Clean Water Act regulations. For example, McClelland and Horowitz [22] found that aggregated biochemical oxygen demand (BOD) discharges from pulp and paper plants were approximately 50 percent of allowable levels. Shimshack and Ward [28] reported that roughly 98 percent of plants were in compliance with total suspended solids (TSS) and BOD regulations during an average month. Given these significant compliance rates, one might expect small overall reductions in discharges from increased enforcement efforts. Under conventional economic wisdom, only violating plants have incentives to respond to an increased probability of fines and then only by reducing discharges to just the legal threshold.

However, we demonstrate that this conventional wisdom is inaccurate. Even in an industry where compliance is generally high, an increase in enforcement through fines can cause a significant reduction in discharges. Enforcement not only induces non-compliant plants to become compliant, it provokes many typically over-compliant plants to reduce discharges even further below their permitted levels. One implication of our results is that analyzing only the effect of enforcement on the compliance decision, as in much of the previous literature, substantially underestimates the impact of enforcement on environmental quality. Another implication is that at least some degree of over-compliance is driven by traditional economic incentives, rather than by altruistic corporate social responsibility. While we make no attempt to explain the persistent low average level of discharges, we do find evidence that significant variation around this central tendency can be explained by variation in enforcement efforts.

Our analysis begins with a conceptual framework that motivates the subsequent investigations. Plants with stochastic discharges face an uncertain and potentially changing regulatory environment. Plants learn about this environment by observing the regulator's recent enforcement history. When a plant observes a sanction on itself or on other plants within its state, it updates its beliefs about the regulator's overall credibility and stringency. The plant bases its target discharge levels, in part, on these updated beliefs.

Next, the paper investigates the empirical relationship between enforcement and discharges. We use a panel of plant-level water pollutant discharges and sanction data from the EPA's Permit Compliance System. The sample spanning 1990-2004 is the most modern in the literature. First, we test the overall strength of the enforcement response using linear regressions. In periods of high regulatory stringency, average discharges fall significantly. Second, using quantile regressions, we demonstrate that most of this response is by plants that statistically over-comply, i.e. plants that usually discharge well below legally required levels. In periods of increased regulatory stringency, the entire statistical distribution of discharges, not just the upper tail, shifts downwards. In other words, plants with discharges below legally permitted levels reduce discharges further when regulators issue fines on other facilities.

After demonstrating that enforcement significantly increases over-compliance, we explore two mechanisms for the link between enforcement and over-compliance: discharge randomness and discharge jointness. Plants with stochastic discharges or multiple pollutants may have economic incentives to reduce contaminants in periods of high enforcement, even if they are typically discharging well below legally permitted levels. We find that increased regulatory stringency induces plants to go further beyond compliance when they face higher risks from violation due to stochastic discharges. Hence, randomness does play a role in the degree of over-compliance attributable to enforcement. We also find that a pollutant's response to enforcement is influenced by the risks from violation on a different pollutant discharged in the same production process. Hence, jointness also plays a role in determining the degree or extent of over-compliance.

2. Context

2.1 Literature

Enforcement

The empirical literature on enforcement emphasizes the direct role of coercive enforcement in reducing violations of standards. Studies by Magat and Viscusi [21] and Laplante and Rilstone [20] investigated the impact of inspections and the threat of inspections, respectively, on water pollution compliance rates and discharges. Gray and Deily [15] investigate non-monetary enforcement actions on compliance rates in the steel industry. Nadeau [23] considered the impact of enforcement activities on the duration of air pollution non-compliance. Stafford [30] showed that an increase in the maximum possible penalty decreased violations for hazardous waste polluters. Earnhart [13] investigated the impact of inspections, enforcement actions, and their threats on the discharges of Kansas wastewater treatment facilities. The above papers represent important contributions to the empirical enforcement literature. However, none of those papers highlight the effect of enforcement on the degree of over-compliance.

Over-Compliance

The empirical literature on over-compliance emphasizes mechanisms that indirectly reduce discharges below statutory levels. Most relevant for this study is the discharge randomness mechanism. For example, plants may hedge to provide a margin of safety against violations due to stochastic discharges. When stochastic shocks are particularly large, a plant may reduce its average discharges in an effort to stay compliant. Brannlund and Lofgren [9] took such impacts into account in estimating the shadow price of pollution, and rejected a zero marginal value. Bandyopadhyay & Horowitz [5] demonstrated that plants with greater discharge volatility had lower average discharges, which suggests that discharge levels alone may not fully capture plant behavior. Therefore, they used the implied probability of violation to measure plant behavior. They studied the effects of polluter and community characteristics on the probability of violation, but did not examine enforcement.

The bulk of the over-compliance literature focuses on explaining persistent over-compliance. Theoretical models by Arora and Gangopadhyay [4], Kirchoff [17], and Cavaliere [11] all showed that consumer preferences for environmental quality can generate over-compliance as a market outcome. Arora and Cason [2,3] found empirical support for this theory; larger firms with greater public contact were more likely to participate in the EPA's 33/50 program. Arora and Cason [1] and Becker [8] used census data to show that demographic composition affected Toxics Release Inventory self-reported emissions and air pollution abatement expenditures, respectively. Similarly, Earnhart [14] demonstrated that community characteristics like unemployment, political factors, community size, and demographics impacted the environmental performance of Kansas wastewater treatment facilities. Perhaps the most economically intuitive explanation for voluntary over-compliance would be very low marginal variable costs of abatement, possibly due to "lumpy" abatement investments. For example, in a putty-clay investment scenario, the plant might over-invest in a fixed technology for fear of future reductions in pollution standards. McClelland and Horowitz [22] statistically rejected this hypothesis of a negligible shadow value for discharges.

The preceding explanations may well explain part of persistent over-compliance. However, these mechanisms move too slowly to explain much of the important short-run variation in observed over-compliance. In contrast, this short-run variation is the focus of our study, and we document that significant variation in the degree of over-compliance is attributable to variation in enforcement stringency. Further, the broader literature interprets over-compliance as discharges below permitted levels due to factors beyond regulation. Our interpretation might be thought of as *statistical* over-compliance, in the sense that there is some underlying risk of violation and sanction motivating reductions beyond what is required by law.

2.2 Background

Water pollutants for the U.S. pulp and paper industry are the focus of our analysis. We choose the pulp and paper industry because it is the largest discharger of conventional pollutants into U.S. waterways, releasing over 16 million cubic meters of wastewater daily. In our sample, water pollution permitting, inspection, and enforcement activities are conducted by state-level regulatory authorities under the auspices of the National Pollution Discharge Elimination System (NPDES). Monthly self-monitoring reports are the primary source of compliance information. 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. Inspections vary in purpose, but sampling inspections are the most significant. Sampling inspections consist of equipment examination, performance auditing, and regulator sampling of discharges.

Enforcement actions range from levying fines to making warning telephone calls. The full deterrent effect of sanctions may be greater than the nominal monetary cost, which is often significant by itself. Fine events may be signals of a broad willingness to be tough on non-compliance. Increased regulatory threats may include enhanced penalties, some of which may be severe. Of course, very few such severe sanctions would be observed if the threat of them is credible.

We take the standard view of the regulated plant as a rational decision-maker that undertakes abatement effort to the point where the marginal cost of such effort equals the corresponding marginal benefit. Plants face an uncertain regulatory environment, so their assessments of the threat of a fine for non-compliance are updated based upon experience. Following Sah's [25] work on social osmosis in crime, we assume that an important credible source of information about the probability of a fine is the enforcement history of the regulator. Since there are likely to be shocks to the regulatory system, including changes from local political and economic conditions, the most informative data about current conditions is from the recent past. Recent sanctions by a regulator, on any plant, affect the regulator's overall credibility and thus impact each plant's perceived threat of a fine.[1] Consequently, recent fines may influence discharges of both sanctioned plants and other plants in the same state. See Shimshack and Ward [28] for an empirical demonstration of this latter regulator reputation effect, also known in the law and policy literature as general deterrence.

Treatment

Pulp and paper plants can meet mandated NPDES pollution limitations by modifying production processes or treating effluents. Historically, most abatement was from external end-of-pipe treatment. More recently, external treatment options have been coupled with modern production practices that mitigate effluent production. In the pulp and paper industry, wastewater treatment typically follows three steps: screening, primary clarification, and secondary biological treatment. Typically, wastewater first passes through bar screens that remove large solids. Second, gravity sedimentation or dissolved air floatation removes most suspended solids. Third, wastewater from the primary clarifiers is fed to facilities that use microorganisms to remove the effluents' organic molecules. The most common of these secondary treatment technologies is the activated sludge process.

Pulp and paper treatment often produces discharges that are volatile from the plant's perspective. Efficiency for common secondary biological treatment processes, for example, is highly sensitive to the number and composition of microorganisms, temperature, acidity, light, nutrient concentrations, substrate (organic matter) concentrations, dissolved oxygen levels, and sludge age [31]. Further, many primary clarifiers and secondary treatment basins are located outside and are therefore sensitive to weather and climatic conditions.

Environmental control in the industry also involves pollution jointness. For example, secondary biological treatment inherently removes both oxygen demanding substances and solids. Further, discharge reductions increasingly occur via process modifications. In pulping, changes for improved environmental performance include alternative raw materials, modern debarking and chip preparation, mechanical raw material transport, liquor spill control, and thermo-chemical changes [29]. In papermaking, the major environmental improvement has been wastewater recycling. These process modifications jointly reduce effluents as a whole.

3. Data

3.1 Our Sample

The EPA's Permit Compliance System (PCS) serves as our specific data source. Established in conjunction with the Clean Water Act and its amendments, the PCS tracks monthly plant-level self-reported discharges, permitted effluent limitations, inspections, and enforcement actions. Our sample includes the most modern data currently available in the public version of the PCS. We consider 251 “major” pulp, paper, and paperboard mills in 28 sample states over 14 years. Specifically, we track plant’s discharges, limits, and enforcement activity for the 168 months between 1990-1996 and 1998-2004.[2] 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. We only consider major plants because these facilities are required to report their own discharges levels for operating pipes each month. We consider all states with two or more major pulp, paper, or paperboard mills.

The dataset contains the relevant information for the conventional water pollutants biochemical oxygen demand (BOD) and total suspended solids (TSS). We choose these contaminants because nearly all pulp and paper mills produce wastewater with significant amounts of these discharges. While there are several measures of effluent discharges and limits, we examine average monthly quantities. All 251 plants report TSS quantities and a subset of 242 plants also report BOD quanitities. For the purposes of analysis, we scale discharges to obtain ratios of actual to permitted discharges, which can be thought of as discharges as a percent of the standard. Since some plants may have multiple outfalls, our final plant-level unit of observation is the maximum discharge ratio for each pollutant across all outfalls.[3]

In addition to discharges, the dataset contains information on administrative fines and inspections. Fines are monetary charges imposed by the state agency, rather than a court, for a violation. We consider fines coded as effluent violations in the PCS. This excludes sanctions for other types of violations such as paperwork errors, reporting errors, or poor equipment maintenance. To isolate fines at least partially attributable to BOD and TSS, we choose those effluent sanctions preceded by one or more BOD or TSS violations in the previous year. We consider all inspections in which the regulator conducts effluent sampling.

All discharge and violation data in the PCS, and thus in the empirical analysis, is self-reported. 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. Further, a USEPA Center for Environmental Information and Statistics [32] independent analysis has confirmed the accuracy of PCS data. Laplante and Rilstone [20] suggested a test for the accuracy of self-reported data based on the difference in reported discharges when an inspector is present or absent. In a regression of discharges on inspections and plant-level fixed effects, we fail to reject the null hypothesis of accurate self-reporting for both BOD and TSS.

3.2 Summary Statistics

Table 1 displays descriptive statistics about actual discharges and fines. Notably, Table 1 indicates very substantial levels of over-compliance. On average, aggregate BOD discharges are less than 40 percent of permitted levels. TSS discharges are about 30 percent of permitted levels. Histograms displaying discharge ratios for a typical month are presented in Figure 1 and Figure 2. In an average month, approximately 1 percent of plants are in violation. Several plants violated more than once. In total, 123 plants violated in one or more months for at least one pollutant during our sample period. Of these, 53 plants recorded violations for both BOD and TSS. Over the entire sample, there were 439 BOD plant/month violations and 226 TSS plant/month violations. Overall, 62% of plant/month violations were BOD alone, 26% were TSS alone, and 12% were both BOD and TSS. Violations declined over time, although non-monotonically. The maximum number of violations for both BOD and TSS occurred in 1990 and the minimum number of violations occurred in 2004. Violations were also not distributed evenly across space, as both the total number of violations and violations per plant were considerably higher for a subset of states.

The bottom portion of Table 1 presents descriptive statistics for administrative fines. There were 39 fines associated with BOD or TSS quantity violations, and these fines averaged about $32,700. Note that these fines should be interpreted relative to the gain in plant-level profits obtained by exceeding a given pollution standard in a given month, not relative to the overall operating revenue of a plant. Fines modestly declined over time. The maximum number of fines in a given sample year was 6, in both 1992 and 1993. The minimum number of fines in a given sample year was 0, in both 1998 and 2004. As noted in Table 1, thirteen states levied fines during our sample period. These thirteen states had mean violations per plant between 2 and 4 times higher (TSS, BOD respectively) than the 15 states that did not levy fines. While we do not know precisely what violation triggered a fine, it seems that fines tended to over-represent violations for both pollutants simultaneously. Eight of our 39 fines were preceded solely by one or more BOD violations in the previous year and 10 of 39 fines were preceded solely by one or more TSS violations in the previous year.

Note that fines primarily enter our empirical specification though a regulator reputation variable that indicates the presence of a fine on another plant within the same state. Because one fine affects all other plants in the state, a significant fraction (8.8 percent) of observations have positive reputation effect fine variables.

The data also display significant volatility. The standard deviations of discharge ratios are 0.30 and 0.28 for BOD and TSS. Plants with typically low discharges account for a large fraction of violations. About one-half of total BOD violations are by plants with median BOD discharge ratios below 50 percent and about sixth-tenths of total TSS violations are by plants with median TSS discharge ratios below 50 percent.

4. Demonstrating Enforcement-Induced Changes in Discharges

In this section, we use panel-data techniques to analyze plants' discharge responses to changes in regulatory enforcement. Following our conceptual framework, a key determinant in this exploration is the regulator's recent enforcement history, a proxy for the likelihood, at any given time, of the regulator issuing a fine for a violation. First, we explore the impact of this regulator reputation effect on mean levels of discharges. Second, we explore the impact of the reputation effect across all ranges of the discharge distribution, from those plants that typically violate to those that greatly over-comply.

4.1 Variables

The dependent variable in each of our analyses is the ratio of actual discharges to the legally permitted level (discharges as a percent of the standard). The key explanatory variable, following [28], is a 0-1 dummy variable that indicates the existence of a fine on another plant j in plant i's state in any of the 12 months prior to t.[4] This measure proxies for plant beliefs, and thus we refer to the variable as the regulator *reputation effect*. The ideal measure of regulator reputation would be plants' perceptions of regulatory stringency. However, perceptions are unobserved and unobservable.[5] Fines are generally quite rare, so the very existence of a recent fine may lead a plant to conclude rationally that the threat of fines is higher than average, given a non-static regulatory environment. We later show that using the dummy approach in estimation is consistent with a two-state model of threat.[6]

We also consider the impact of regulator actions on the sanctioned plant. Thus, we include a 0-1 dummy variable indicating whether that particular plant was fined in the previous year. This idiosyncratic deterrence effect might reflect increasing sanctions for plants with an offense history. Additionally, inspections may affect discharges at the plant-level. So, we include the number of sampling inspections in the previous year as an explanatory variable.

Plant production varies seasonally, thus we include quarterly dummy variables. Technological change may be an issue given our long data series. Thus we include annual dummies to account for broad trends in abatement technology. Further, for all linear regressions, we include plant-specific linear time trends to account for possible variation in adoption of technology across plants.

Finally, we exploit the panel structure of the data by including fixed effects. For all linear regressions, we use plant-level fixed effects. Thus, we obtain identification only from within-group variation. Plant-level fixed effects allow us to capture systematic differences due to factors such as different SIC codes, production capacity, and geographic conditions. Further, a natural concern in plant-level analyses is that regulators may target some plants for stricter enforcement based on their overall environmental performance. Without fixed effects, this targeting might produce a positive correlation between enforcement and discharges simply from cross-plant differences in overall enforcement.

4.2 Linear Regressions

Does enforcement activity reduce the overall discharge ratio on average? Our goal here is to establish the basic relationship between the perceived probability of sanction and pollution discharges. Thus, we run fixed-effects linear regressions of discharge ratios on regulator reputation enforcement variables for BOD and TSS.[7] In addition, we included all the exogenous variables discussed above as controls. Results are presented in Table 2. Computed standard errors are heteroskedastic-consistent. T-statistics appear in parentheses.

Results in Table 2 indicate that the estimated impact of a fine on another plant in the same state on the discharge ratios is negative and strongly significant for both BOD and TSS.[8] The average discharge ratio declines 0.024 in the year following a fine. Given the overall mean discharge ratios, this translates (on average) into an approximately 6 percent reduction in aggregate discharges for BOD and an approximately 8 percent aggregate reduction for TSS.

Idiosyncratic, individual fine deterrence effects are also statistically significant, but less economically significant than the reputation effects which simultaneously impact many plants. Seasonality appears to play a strong role in discharges, as all estimated related coefficients are large and significant. We also find that average discharges for both BOD and TSS trend downward over time.

4.3 Conditional Quantile Regressions

Do fines reduce discharges by plants statistically over-complying? Our goal here is to establish that the predicted fine-induced discharge response applies to over-compliers. The linear regression above demonstrated that average discharges respond to the increased regulatory threat associated with enforcement actions. However, this aggregate result might be driven solely by significant violators responding to the threat of sanctions. We therefore use Koenker and Bassett's [18] conditional quantile regressions to examine the discharge response at various levels of compliance. Standard errors are estimated following [19,24].

In our context, the role of the quantile regression is to decompose the mean response revealed by the linear regression into changes across the state-wide probability distribution of discharge levels. Conditional quantile regressions allow us to estimate different fine slope coefficients for different discharge quantiles. For example, a regression on the 50th percentile estimates the effect of the fine reputation effect on the sample median. Since the sample median of discharges is well into the over-compliance region, a significant predicted fine response for the 50^{th} percentile would indicate that even plants in that statistically over-comply typically reduce discharges after a fine. In addition to the median regression, we also ran the 25^{th}, 75^{th}, and 90^{th} percentile regressions. Here, higher quantiles correspond to higher discharges. We do not examine more extreme quantiles such as the 95^{th} percentile because quantile regressions are generally unstable at the extreme tails of distributions, due to reductions in sampling variation [10].

In the quantile regression analyses, we include state-level fixed effects and state-level linear time trends to identify what happens to the overall discharge distribution within a state. We do not include plant-level fixed effects because such plant-level fixed effects in quantile regressions would yield coefficients that indicate a typical plant's fine responses across the distribution of departures from the individual's usual discharge level. So, a 90^{th} percentile coefficient would be the fine response when plants are emitting a particularly large amount relative to their idiosyncratic typical levels. Our purpose, however, is to investigate if the pollution distribution shifts for plants operating below their discharge standard. In a linear regression context, the overall mean discharge response does not depend on which specific plants adjust. In contrast, the overall change in the shape of the state-level discharge distribution reflected in the quantile regression approach does.

Quantile regression results for BOD and TSS are presented in Tables 3 and 4, respectively. We find strong evidence that plants reduce discharges after an increase in the predicted probability of a sanction for violation across the entire range of the discharges distribution. For both pollutants, enforcement significantly reduces discharges reductions at *every* estimated quantile.[9] Recall that even the 90^{th} percentile is in the over-compliance region, as this percentile represents a discharge ratio of about 0.75 for BOD and 0.62 for TSS. The important lesson from these quantile regressions is that the entire discharge distribution significantly shifts in response to the reputation effect.

Moreover, we find that the response at the highest quantiles tends to be larger than at the lowest. For both BOD and TSS, fines responses at the 25th and 90th percentiles are economically different from one another. For example, the TSS results in Table 4 indicate that the fine response at the 90th discharge percentile is more than 2.5 times greater than the fine response at the 25th discharge percentile. BOD results in Table 3 indicate that the fine response at the 90th percentile is approximately 6/10 greater than the fine response at the 25th percentile. Some, but not all, differences are statistically significant as well (e.g. TSS 25th vs. 90th, TSS 50th vs. 90th). These results are intuitive; plants closer to violating their standard may respond to a greater extent.

The results establish that a fine induces a significant over-compliance response across all quantiles of the discharge distribution, including the lowest. Given this broad-based response, two questions naturally arise: Why would plants which statistically over-comply reduce discharges in response to an increased threat of sanction for a violation? Why would plants that sometimes violate reduce discharges in all periods, rather than simply reducing violations to the standard threshold? Section 5 explores these issues in more depth; we test the extent to which discharge randomness and jointness in pollution production can resolve these puzzles. However, we first explore the sensitivity of our key empirical regularity.

4.4 Sensitivity Analysis
Statistical Plausibility

Are the statistical findings reasonable? An alternative and non-parametric analysis is a simple comparison of means event study. Here, we compare statewide discharges in the year before and the year after a fine in that state, omitting the fined plant itself to ensure a fair comparison. We find that BOD discharges drop 5.2 percent and TSS discharges drop 9.6 percent. These results are comfortably close to those of the regression analysis, which is the preferred method because it accounts for covariates.

Perhaps one might still be concerned that the results are a consequence of some spurious correlation between the timing of fines and some general economic or political condition, not accounted for in our regression models. If that were the case, we might expect discharges in other states to react at the same time to the true cause. Thus, we perform a counterfactual experiment which randomly shuffled the fine reputation variable at time *t* across the pool of all plants. We found negligibly small average linear regression coefficients and t-statistics.[10]

Sensitivity to Assumptions

Our results are robust to alternative specifications for our key fine reputation effect. One natural alternative to our fine dummy approach is a fines per violation measure, where "violation" indicates the presence of a plant/month violation for BOD, TSS, or both. Results are economically and statistically similar to presented results. We also considered the possibility that unfined violations contribute to the regulator reputation effect by including the number of unfined violations per plant as an explanatory variable. Results for the original fine variables are extremely similar. Finally, we tried a linearly diminishing function for the fine variable, rather than a dummy. Results are again economically similar to those presented, and a specification test favors the dummy approach.

Our results are also robust to an alternative approach to plant-specific technological change. Our analysis used plant-specific linear time trends, as well as overall year dummies. An alternative approach of including an auto-regressive term lagged one year, used by Magat and Viscusi [21], yields very similar results.

5. Mechanisms for Enforcement-Induced Changes in Over-Compliance

Can the empirical results documented in the explorations of Section 4 be explained by economic mechanisms? In a simple deterministic one-pollutant model of the firm, over-complying plants would have no reason to react further to enforcement, since they face no threat of sanction. However, plants with stochastic discharges may face some possibility of a fine from accidental discharges over the legal standard [6,7,12]. Many factors such as equipment failures, human error, or poor maintenance may cause realized discharges to differ from *target*, or intended, discharges during any particular time.[11] Moreover, a plant compliant in one pollutant may face some possibility of a fine for violations of a different, but jointly-produced, pollutant. Either of these mechanisms, or both, could in principle explain the reaction of statistically over-compliant plants to changes in enforcement.

Basic economic logic implies that the marginal expected fine should help explain discharges, since plants balance the marginal benefits of discharging with the marginal costs of the expected sanction from violating. There are potentially two uncertain elements to sanctions and thus the marginal expected fine. First, as discussed above, discharges are volatile and may be partially random, even from the plant's perspective. Thus, even if the plant's target discharges z for a given pollutant are below the legal limit, there may be a positive expected penalty $F(z)$, which accounts for volatility in actual discharges around z. Another uncertain element of sanctions is the whether a given violation will be fined. Empirically, many violations are not sanctioned, so a fine occurs with some probability P. In our conceptual framework, a key assumption is that the plant's assessment of P depends on recent enforcement actions. Taking the these two components together, the marginal expected fine is $P \times F'(z)$. Changes in the perceived probability of sanction influence discharges of a risk-neutral plant through this term. So, one reasonable and intuitive way to account for regulator reputation is to include the marginal expected fine for each jointly-produced pollutant in the linear discharges regression rather than a dummy for the presence of a fine.[12]

We also assume a simple two-state threat perception model, with the default setting of low threat because fines are so uncommon. Suppose P can take only take two values: P_{lo} and P_{hi}. The plant believes the threat is high in periods after a fine in the state, which we code with the regulator reputation dummy variable R . Note that the marginal expected penalty terms can then be written in the form $P_{lo}F' + (P_{hi} - P_{lo})RF'$. In our regression, we exploit this simple technique by including both F' and RF' as regression explanatory variables. Then P_{lo} and P_{hi} need not be pre-specified, as they will be implicitly absorbed into the regression coefficients to be estimated. The baseline marginal expected penalty is thus accounted for by including F' in the discharge regression. The interaction with the reputation dummy RF' allows for increased importance of the expected fine when the threat of such a fine is higher. This interaction term is the key explanatory variable of interest.

Randomness

To the extent that randomness explains the over-compliance response, we would expect plants facing a higher risk from random violation to respond more strongly to an increased probability of sanctions. In particular, this impact should be transmitted through the marginal expected fine. In this section, we explore to what extent randomness can empirically rationalize the post-fine discharge responses documented in Section 4. As discussed above, we do so by interacting the reputation dummy R with the marginal expected fine. In sum, we replace R in each discharge regression with F' and RF' for that same pollutant. We then test whether the over-compliance response is better explained through this randomness mechanism than under the original exploratory regressions.

To construct the marginal expected sanction measures, we must first have an empirical measure of the stochastic shocks to discharges. These shocks are the difference between intended discharges and actual discharges. Of course, determining the marginal expected penalty requires integration over an estimate of the statistical distribution of discharges, since any fine would depend on the realized level of random discharges. Our premise is that a reasonable estimate of random shocks is the empirical density of regression residuals.

One might simply assume a fixed distribution for the random shocks about their expected value, using observed residuals to identify parameters of the assumed distribution. For example, one might assume a Gaussian distribution of random shocks and set the variance parameter equal to the mean squared regression residuals. However, this approach would be problematic in our context. The shape and scale of regression residuals differ considerably from plant to plant. Visual inspections of histograms generated from the residuals of regressions similar to those reported in Table 2 indicate some residual densities are highly skewed to the right and some are symmetric. Fitting a simple parametric density to such diverse densities is particularly unsatisfactory because the upper tail of these distributions is critical for correctly assessing the probability of violation due to randomness.

We therefore turn to non-parametric density estimation to estimate plant-specific distributions of random shocks. This approach better captures the variability in the distribution of random shocks across plants. One standard density estimation technique is kernel estimation, which, intuitively speaking, smoothes out a histogram. We apply an adaptive-bandwidth kernel density estimator, which allows the degree of smoothing to vary somewhat across the distribution; see [26] for a more complete discussion. We adopt the adaptive kernel, as opposed to a kernel estimator with a fixed bandwidth, because we are particularly interested in the upper tail of the distribution where data can be sparse. In our analysis, the optimal bandwidth is fit locally by a cross-validation criterion; estimates are generated using the implementation by Van Kerm [33].

Given density estimates, we can construct our empirical measure of the marginal economic risk from random violation, F'.[13] To operationalize this measure, we must specify the fine as a function of the extent of violation. Since this function is unknown, we present results for two specifications. The first is a flat fine for any violation, independent of the extent. The second is a penalty linear in the extent of violation. Applying these specifications to our conditional density estimate for discharges, we numerically calculate the derivative of the expected fine, F′. The marginal expected penalty for a fixed fine is trivially proportional to the density of discharges at the standard. If fines are linear in the extent of violation, with discharges measured on a ratio scale, the marginal expected is proportional to the probability of a violation. Different error or fine structures would lead to different calculations.

Table 5 presents the results of our randomness exploration regressions. We find strong evidence that BOD randomness plays an important role in enforcement-induced changes in over-compliance for that pollutant. Coefficients on the interaction between the fine reputation effect and the marginal expected sanction (R F′) are statistically significant for both penalty specifications. This indicates that, in periods when regulators are perceived as more willing to impose fines, the BOD over-compliance response is greater when plants have higher marginal expected sanctions due to BOD randomness.[14]

Can BOD randomness alone rationalize the enforcement-induced over-compliance response for this pollutant? One check is a specification test of the randomness model (Table 5) against the previous uninteracted model (Table 2) that used only a non-interacted reputation term (R). Performing non-nested P-tests, for both BOD fine specifications, we can reject the uninteracted model against the randomness model. For BOD, randomness does appear sufficient to explain the enforcement-induced over-compliance response.

In contrast to BOD, we find no systematic evidence that TSS randomness plays an important role in enforcement-induced changes in over-compliance for that pollutant. Coefficients on the interaction between the fine reputation effect and the marginal expected sanction ($R\ F'$) are not statistically significant for both penalty specifications. In periods when regulators are perceived as more willing to impose fines, the TSS over-compliance response is not enhanced when plants have higher marginal expected sanctions due to TSS randomness.

Further, the specification tests for the TSS randomness model yield ambiguous results. For the linear penalty specification, a P-test fails to reject the uninteracted model (Table 2) against the randomness model (Table 5). For the flat fine penalty specification, a P-test does reject the uninteracted model against the alternative randomness model. For TSS, randomness does not appear to systematically explain the enforcement-induced over-compliance response. Thus, it seems that enforcement is affecting TSS discharges through some mechanism beyond randomness alone.

Jointness

Another possible explanation for enhanced over-compliance is jointness in pollution production and abatement. As discussed in the background section, BOD and TSS discharges are (at least partially) jointly determined. Wastewater treatment technologies treat both BOD and

TSS simultaneously, and modern production practices to improve environmental performance reduce many pollutants at once. To the extent that a high penalty risk for one pollutant induces a plant to undertake environmental improvements, those actions may reduce the other, jointly determined, pollutant.

We extend the regressions of the previous section to account for jointness, as well as randomness, by including cross-pollutant risk, as derived in the appendix. To do so, we augment the analysis presented in Table 5 to include the other pollutant's marginal expected penalty F' and the interaction of the reputation effect with the other pollutant's marginal expected penalty $R\ F'$. Thus, for example, BOD regressions include the BOD marginal expected penalty F'_B, the BOD interaction $F'_B\ R$, the TSS marginal expected penalty F'_T, and the TSS interaction $F'_T\ R$. TSS regressions are symmetric. Simultaneous estimation of the BOD and TSS equations through a SUR regression would yield no efficiency gain, since the covariates in each equation are identical.

Results of the simultaneous jointness/randomness exploration are presented in Table 6. Note especially rows 1 and 2. Here, we find strong evidence that BOD randomness plays an important role in enforcement-induced changes in over-compliance for *both* BOD and TSS discharges. Coefficients on the interaction of the fine reputation effect and the BOD marginal expected sanction ($R\ F'_B$) (Table 6, Row 1) are statistically significant for both specifications for both pollutants. This indicates that, in periods when regulators are perceived as more willing to impose fines, both BOD and TSS statistical over-compliance responses are greater when plants have higher marginal expected sanctions due to BOD randomness. In contrast, we find no statistically significant evidence that TSS randomness plays an important role in enforcement-induced changes in over-compliance for *either* BOD or TSS discharges. Coefficients on the interaction of the fine reputation effect and the TSS marginal expected sanction ($R\ F'_T$) (Table 6, Row 2) are not statistically significant for both specifications for both pollutants.

Results suggest that enhanced over-compliance in TSS after a fine may be at least partially a side-effect of efforts to avoid violations in BOD discharges, which are jointly determined with TSS. This implication is plausible for four reasons. First, as discussed, empirically observed jointness is consistent with the economic logic for jointly-produced multiple pollutants. Second, the randomness regressions and P-tests previously discussed suggested that something beyond randomness alone was driving TSS enforcement-induced changes in over-compliance. Third, BOD violations occur about twice as frequently as TSS violations, and so represent the predominant concern for violations. Fourth, the volatility of BOD discharges is generally much higher than TSS, so that randomness is may be a more fundamental concern in the case of BOD.

Can randomness and jointness rationalize the enforcement-induced over-compliance responses for both pollutants observed in Section 4? To check, we run specification tests of the randomness and jointness model (Table 6) against the previous model (Table 2) that used only a non-interacted reputation term (R). P-tests for both fine specifications for both BOD and TSS reject the uninteracted model against the randomness and jointness model. For both discharge types, randomness and jointness do appear sufficient to explain the observed enforcement-induced over-compliance response.

6. Discussion and Conclusions

The main contribution of this paper is explicitly linking the enforcement and over-compliance literatures. We empirically demonstrate that many statistically over-complying plants reduce discharges when regulators issue fines, even fines on other plants. Aggregate BOD and

TSS discharges within a state fall approximately 7 percent in the year following a sanction within that state. Most of this reduction is due to enhanced over-compliance, rather than simply a reduction in violations.

These empirical results can be rationalized by economic theory. We find economically and statistically significant evidence that discharge randomness and jointness in pollution production play important roles in the degree of over-compliance. In particular, a simultaneous analysis of these factors indicates that the risk of accidental violation due to BOD randomness is the predominant mechanism of the enforcement-induced changes in over-compliance for both BOD and the jointly determined pollutant TSS.

Significant policy implications follow from our analysis. First, variation in the degree of over-compliance is driven by traditional economic incentives, rather than altruistic corporate social responsibility. Second, randomness and jointness results indicate that BOD reductions have important implications for other pollutant levels. These implications should perhaps be considered in permitting and enforcement. Third, and most notably, enforcement generates substantial discharge reductions above and beyond those expected from simply deterring violations. Ignoring the impact of sanctions on over-compliance considerably understates fines' effect on environmental discharges. If standards are not overly tight, enforcement-induced changes in over-compliance may also translate into larger welfare gains than anticipated. Consequently, a substantial improvement in environmental 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 more regularly.

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Footnotes

[1] One could formally model this learning process in a Bayesian framework. However, for our purposes, the practical value of such a model is low, since the basic lesson that plants update their beliefs in response to new information is quite straightforward. We refer the interested reader to Sah [25] for a formal treatment.

[2] Our comprehensive sample is constructed from a pre-existing 1990-1996 dataset and a newly obtained 1998-2004 dataset. When the more recent subsample was obtained, information for 1997 was no longer present in the publicly available version of the PCS.

[3] In any given month, the vast majority of plants emit a measured pollutant from a single outfall. Further, the composition of discharges across outfalls remains relatively constant over time. Thus, it is unlikely that this convenient aggregation biases our results.

[4] We define this variable over one year because the literature indicates that this reputation signaling effect declines quite rapidly after 12 months.

[5] One can view our measure as a proxy for true perceptions. If this is imperfect, it will bias coefficients towards zero. So, use of a proxy should not spuriously cause affirmative results.

[6] One potential weakness is that the dummy variable approach does not account for the number of violations. An alternative measure that does so, in principle, would be the ratio of fines to violations over the past year. However, as compliance is generally quite high in our dataset, this ratio most often takes the same 0-1 values as the dummy and has a sample mean within 15% of the dummy. Moreover, constructing a ratio requires dropping data in the case of no recent violations. We use fine existence for the results and explore alternatives in the sensitivity section.

[7] We also ran specifications with logged dependent variables. Logs have the advantage of preventing negative predicted discharges. However, as a practical matter, the current specifications predict very few negative discharges. The logged specifications yielded statistically similar results, but the key coefficient magnitudes were larger in absolute value. We ultimately chose the current specification to be conservative and because many detrended and seasonally corrected plant discharge distributions do not appear log-linear.

[8] We construct the reputation effect variable using fines on both BOD and TSS, since plants would extract signals about overall regulator stringency from sanctions on both. Thus, we have a single proxy for an increased probability of sanctions on both pollutants.

[9] It may initially seem puzzling that the 'fine 1-12 months on self' and inspections coefficients are frequently positive in the quantile regressions. However, these results are consistent with the absence of plant-level fixed effects. Without plant-level fixed effects, if regulators target plants for stricter enforcement based on their overall environmental performance, these control variable coefficients may be positive. Helland [16] finds evidence for such plant-specific targeting.

Nonetheless, as a robustness check, we ran regressions that omitted the own fine variable and regressions that included the fined plant in the reputation effect. In all regression analyses, results are economically similar to those presented in the tables.

[10] While the spurious correlation test presents evidence that omitted national shocks are not driving the results, an additional concern may be omitted *state-level* shocks. Fines occur when discharges are particularly high, and suppose particularly high discharges for one plant reflect omitted state-level common shocks (like weather) that induce particularly high discharges for all plants within the state. Therefore, one might naturally expect discharges to be less high in the next period anyway; this is the standard "regression towards the mean" effect [12]. However, this comparison is not what our fixed effects analysis investigates. Our analysis reveals a fine-induced decrease in discharges relative to the plant's conditional average discharges, not relative to the fined period's discharges. Consequently, an omitted state-level common shock could only produce our results if the common shock was accompanied by strong and persistent negative serial correlation. We find no systematic evidence of negative serial correlation in either the short- or the long-term.

[11] If the plant is uncertain about future operating and market conditions, it will also be uncertain about what discharge levels will be desirable in the near future. Since abatement steps such as preventive maintenance or operator training may require lead-time, both accidental discharge variation and uncertain near-future operating conditions are important sources of randomness from the plant's perspective.

[12] A formal demonstration that this intuitive specification can be rationalized if profits are quadratic in discharges is available through JEEM's online archive supplementary material, which can be accessed at http://www.aere.org/journal/index.html.

[13] Some care must be paid in the construction of the distributions underlying our measure of empirical risk from random violation. We do not want a function of the residual for plant i's observation in period t to be included as an explanatory variable in a subsequent regression for that residual. Therefore, the constructed density of random shocks for each observation is based upon plant i's regression residuals for all of that plant's periods not equal to t.

[14] It is possible that econometric volatility overstates volatility from the plants' perspective. Thus, we experimented with adjusting the density of the econometric residuals by scale factors of ¾ and ½. This reduces our estimate of the risk of random violation. In both cases, the impact of randomness captured by the interaction R F′ remains significant for both the flat and linear BOD fine specifications.

Table 1. Summary Statistics

DISCHARGES

Pollutant	Mean discharge ratio	25% Quantile	50% Quantile	75% Quantile	90% Quantile	Violations	Violators
BOD	.384	.168	.334	.545	.751	439	101
TSS	.307	.130	.248	.428	.621	226	75

FINES

Total Fines	States levying fines	Median fine	Std. dev. of fines
39	13	$9,000	$97,061

Table 2. Plant-Level Linear Regression Results

Variable Description	BOD Regression Coefficients	TSS Regression Coefficients
Fine 1-12 months ago on another plant	-0.0235*	-0.0240*
	(-4.72)	(-5.98)
Fine 1-12 months ago on self	-0.0573*	-0.0905*
	(-2.14)	(-2.41)
Inspections 1-12 months ago	0.0011	0.0025
	(0.39)	(0.82)
Season2 Dummy	-0.0447*	-0.0442*
	(-8.08)	(-9.14)
Season3 Dummy	-0.0585*	-0.0631*
	(-6.51)	(-7.66)
Season4 Dummy	-0.0441*	-0.0469*
	(-3.40)	(-3.85)
Year Dummies	13 Year Dummies	13 Year Dummies
Fixed Effects	241 Plant-Level FE's	250 Plant-Level FE's
Linear Time Trends	241Plant-Specific TT's	250 Plant-Specific TT's

[a] The dependent variables are the ratios of actual to permitted discharges for this plant/month combination for the listed pollutant.
[b] A superscript * indicates statistical significance at the 5% level.
[c] The BOD plant-level analysis consists of 30,895 observations from 242 plants over the 168 sample months.
[d] The TSS plant-level analysis consists of 32,995 observations from 251 plants over the 168 sample months.

Table 3. BOD Quantile Regression Results

Variable Description	**25% Quantile**	**50% Quantile**	**75% Quantile**	**90% Quantile**
Fine 1-12 months ago on another Plant	-0.0263*	-0.0234*	-0.0386*	-0.0411*
	(-5.60)	(-3.49)	(-4.67)	(-4.02)
Fine 1-12 months ago on self	0.0466*	0.0782*	0.0612*	0.0290
	(4.24)	(4.93)	(3.14)	(1.23)
Inspections 1-12 months ago (in state)	0.0021*	0.0017*	0.0015	-0.0001
	(3.62)	(2.09)	(1.54)	(-0.10)
Season2 Dummy	-0.0311*	-0.0401*	-0.0561*	-0.0469*
	(-6.00)	(-5.41)	(-6.24)	(-4.31)
Season3 Dummy	-0.0420*	-0.0430*	-0.0604*	-0.0265
	(-4.76)	(-3.41)	(-3.92)	(-1.40)
Season4 Dummy	-0.0272*	-0.0224	-0.0387	0.0022
	(-2.13)	(-1.23)	(-1.74)	(0.08)
Year Dummies	13 Year Dummies	13 Year Dummies	13 Year Dummies	13 Year Dummies
Fixed Effects	27 State FE's	27 State FE's	27 State FE's	27 State FE's
Linear Time Trends	27 State-Specific Time Trends	27 State-Specific Time Trends	27 State-Specific Time Trends	27 State-Specific Time Trends

[a] The dependent variables are the ratios of actual to permitted discharges for this plant/month combination for the listed pollutant.
[b] A superscript * indicates statistical significance at the 5% level.
[c] The BOD plant-level analysis consists of 30,895 observations from 242 plants in 28 states over the 168 sample months.

Table 4. TSS Quantile Regression Results

Variable Description	25% Quantile	50% Quantile	75% Quantile	90% Quantile
Fine 1-12 months ago on another Plant	-0.0218*	-0.0353*	-0.0533*	-0.0574*
	(-5.54)	(-6.86)	(-7.83)	(-4.96)
Fine 1-12 months ago on self	0.1162*	0.1815*	0.2245*	0.1519*
	(12.7)	(15.1)	(14.3)	(5.77)
Inspections 1-12 months ago (in state)	0.0005	0.0014*	0.0026*	0.0031*
	(1.03)	(2.24)	(3.17)	(2.21)
Season2 Dummy	-0.0181*	-0.0339*	-0.0531*	-0.0636*
	(-4.19)	(-5.95)	(-7.06)	(-4.95)
Season3 Dummy	-0.0202*	-0.0478*	-0.0756*	-0.0944*
	(-2.75)	(-4.94)	(-5.94)	(-4.35)
Season4 Dummy	-0.0086	-0.0362*	-0.0575*	-0.0675*
	(-0.81)	(-2.59)	(-3.14)	(-2.17)
Year Dummies	13 Year Dummies	13 Year Dummies	13 Year Dummies	13 Year Dummies
Fixed Effects	27 State FE's	27 State FE's	27 State FE's	27 State FE's
Linear Time Trends	27 State-Specific Time Trends	27 State-Specific Time Trends	27 State-Specific Time Trends	27 State-Specific Time Trends

[a] The dependent variables are the ratios of actual to permitted discharges for this plant/month combination for the listed pollutant.
[b] A superscript * indicates statistical significance at the 5% level.
[c] The TSS plant-level analysis consists of 32,995 observations from 251 plants in 28 states over the 168 sample months.

Table 5. Randomness Exploration Regressions

Variable Description	BOD Regressions		TSS Regressions	
	Flat Fine Penalty	Linear Penalty Function	Flat Fine Penalty	Linear Penalty Function
Fine – Marginal Expected Penalty (R F′)	-6.912*	-1.899*	-2.727	-0.9136
	(-3.08)	(-5.27)	(-1.01)	(-1.81)
Marginal Expected Penalty (F′)	-10.321*	-.9743*	-14.571*	0.2303
	(-4.30)	(-7.94)	(-2.46)	(0.75)
Fine 1-12 months ago on self	-0.0662*	-0.0745*	-0.1123*	-0.0823*
	(-2.52)	(-2.77)	(-3.02)	(-2.16)
Inspections 1-12 months ago	0.0020	0.0026	0.0039	0.0030
	(0.74)	(0.97)	(1.22)	(1.02)
Season2 Dummy	-0.0512*	-0.0496*	-0.0496*	-0.0435*
	(-9.37)	(-9.05)	(-9.31)	(-8.62)
Season3 Dummy	-0.0669*	-0.0648*	-0.0710*	-0.0621*
	(-7.26)	(-7.19)	(-8.80)	(-7.01)
Season4 Dummy	-0.0504*	-0.0489*	-0.0534*	-0.0461*
	(-3.86)	(-3.77)	(-4.64)	(-3.62)
Year Dummies	13 Year Dummies		13 Year Dummies	
Plant-Level Fixed Effects	241 Plant-Level Fixed Effects		250 Plant-Level Fixed Effects	
Plant-Specific Linear Time Trends	241 Plant-Specific Time Trends		250 Plant-Specific Time Trends	

[a] The dependent variables are the ratios of actual to permitted discharges for this plant/month combination for the listed pollutant.
[b] A superscript * indicates statistical significance at the 5% level.
[c] The BOD plant-level analysis consists of 30,895 observations from 242 plants over the 168 sample months.
[d] The TSS plant-level analysis consists of 32,995 observations from 251 plants over the 168 sample months.

Table 6. Jointness & Randomness Exploration Regressions

Variable Description	BOD Regressions		TSS Regressions	
	Flat Fine Penalty	**Linear Penalty Function**	**Flat Fine Penalty**	**Linear Penalty Function**
BOD Fine – Marginal Expected Penalty (R F′)	-6.940*	-2.038*	-4.921*	-0.9700*
	(-3.02)	(-5.29)	(-5.14)	(-4.92)
TSS Fine – Marginal Expected Penalty (R F′)	-0.4371	0.6554	-2.294	-0.1860
	(-0.24)	(1.54)	(-0.80)	(-0.29)
BOD Marginal Expected Penalty (F′)	-9.793*	-0.9834*	-5.130*	-0.1535
	(-4.25)	(-4.95)	(-2.27)	(-1.60)
TSS Marginal Expected Penalty (F′)	-3.545*	0.1858*	-14.438	0.4543
	(-2.83)	(2.06)	(-2.36)	(1.17)
Fine 1-12 months ago on self	-0.0714*	-0.0672*	-0.1201*	-0.0836*
	(-2.70)	(-2.50)	(-3.00)	(-2.02)
Inspections 1-12 months ago	0.0026	0.0031	0.0039	0.0032
	(0.94)	(1.11)	(1.18)	(1.01)
Seasonality Dummies	3 Season Dummies		3 Season Dummies	
Year Dummies	13 Year Dummies		13 Year Dummies	
Plant-Level Fixed Effects	241 Plant-Level Fixed Effects		250 Plant-Level Fixed Effects	
Plant-Specific Linear Time Trends	241 Plant-Specific Trends		250 Plant-Specific Trends	

[a] The dependent variables are the ratios of actual to permitted discharges for this plant/month combination for the listed pollutant.
[b] A superscript * indicates statistical significance at the 5% level.
[c] All plant-level analyses consist of the 30,600 observations with both BOD and TSS .

Figure 1.

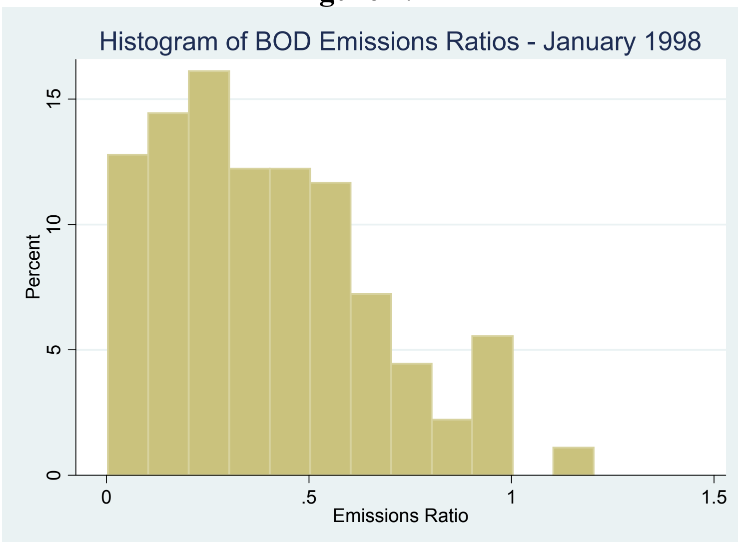
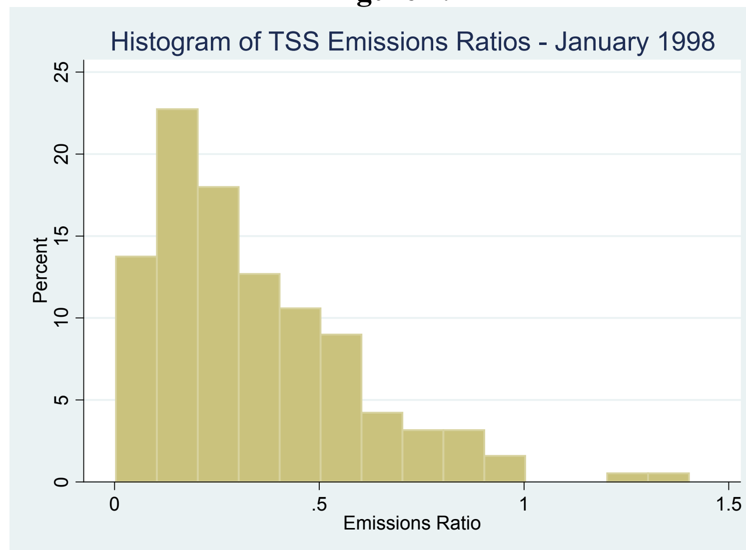


Figure 2.



Figures 1 and 2 display substantial over-compliance with permitted standards for both BOD and TSS. The ratio of actual to permitted discharges nearly always lies in the compliance region (less than 1), and the majority of plants emit less than 50 percent of allowable levels. While the histograms represent discharge ratios for a single month of the sample, other sample months demonstrate similar over-compliance.