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Keywords: Enforcement; Repeat Offender; Compliance; Government Expenditures

JEL Classification: K42 H50 Q58 K32

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Costly sanctions and the treatment of frequent violators in regulatory settings[1]

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"*Agency officials empowered to assess, mitigate, or compromise penalties exercise their powers within the constraints imposed by the enforcement process itself. The process is costly - it requires an investment of staff and financial resources invariably in limited supply … The cost of various explanatory and participatory procedures forces trade-offs between the number of generated cases … and the amount of attention they can give …*" (Diver 1979, pgs. 1471, 1498)

1. Introduction

Regulatory enforcement entails substantial costs. Administrative law agencies spend billions of dollars on investigations, negotiations, and court hearings each year. Sanctions for non-compliance generate significant indirect political economic costs for regulators due to community, industry, and political pressure. Private compliance costs triggered by enforceable environmental, health, and safety laws total one half to two percent of GDP per year, or as much as $360 billion annually. Given the significant public and private costs at stake, understanding the optimal allocation of enforcement resources is a crucial issue for regulation.[1]

This paper considers an unsettled problem for regulators allocating enforcement resources: the treatment of violations committed by frequent versus infrequent violators. Popular wisdom, common notions of fairness, and retributive theories of punishment suggest that regulators should penalize frequent violators' offenses more severely. Actual practice in environmental, energy, financial, occupational, and health regulation follows this prescription closely.[2] Nevertheless, neither extant scholarship nor practice clearly illustrate how a regulator striving to achieve high

[1] Stigler (1970), McKean (1980), Cohen (1986), Polinsky & Rubinfeld (1989), and Magat & Viscusi (1990) provide early discussions of agency enforcement costs. Stigler (1971), Peltzman (1976), Hilton (1972), Joskow (1974), and Leaver (2009) discuss the political economy of regulation. Gray & Deily (1996), Helland (1998), Stafford (2002), Konisky (2007), and Grooms (2015) empirically illustrate political economic factors in contexts similar to our empirical setting. Pizer & Kopp (2008), and U.S. OMB (2016) summarize aggregate costs.

[2] *Appendix A* references select enforcement guidelines. Harsh treatment of frequent violators is standard practice at Commerce (NOAA fisheries), Labor (OSHA), Health and Human Services (FDA), Federal Energy Regulatory Commission (FERC), the Securities and Exchange Commission (SEC), the Environmental Protection Agency (EPA), and other agencies. Kleit et al. 1998; Oljaca et al. 1998; Stafford 2003; Rousseau 2009; Evans 2016; and Blundell 2020 provide evidence consistent with environmental regulators treating frequent or repeat violators more harshly.

compliance with costly sanctions and limited resources might actually determine the optimal balance when punishing frequent and infrequent violators.

In this study, we document and explore two factors that influence the optimal regulatory balance when punishing frequent and infrequent violators. The first factor is the enforcement response effect. Intuitively, a regulator seeking to maximize compliance will want to direct marginal enforcement resources towards those facilities most likely to respond to sanctions. Frequent violators may or may not be more responsive, because frequency of violation may be associated with unobserved variation in the structure of compliance costs. The second factor is a sanction cost effect. A key insight here is that it is costlier to the regulator to maintain a given regulatory threat (i.e. a given expected penalty) for frequent violators. This is because sustaining credible enforcement threats against frequent violators requires the regulator to levy costly penalties frequently. In contrast, sustaining a given regulatory threat for infrequent violators may be relatively cheap because the regulator need not 'back up' threats with costly sanctions as often.

This paper makes three key contributions. First, we provide a novel characterization of the optimal punishment of violations by frequent and infrequent violators in a realistic regulatory setting. Our evidence illustrates how an agency seeking to minimize violations subject to limited enforcement resources might balance enforcement response and sanction costs across frequent and infrequent violators. Second, we show empirically the relative importance of factors driving optimal decisions using data from a Clean Water Act (CWA) regulatory setting. We use the data to build on a growing public enforcement of law literature empirically measuring deterrence effects of sanctions but go beyond past scholarship by exploring deterrence across frequent and infrequent violators and by using the results to calibrate an optimality condition.[3] Third, we illustrate a generalizable framework for regulators considering the optimal punishment of frequent and infrequent violators in their own settings. Rather than assert that our stark CWA results will necessarily generalize, we build an apparatus for stakeholders to form their own conclusions.

We first construct a stylized model that illustrates the optimal regulatory punishment of violations across two types of heterogeneous entities: frequent violators and infrequent violators. The resulting first-order condition shows how enforcement response and sanction cost considerations influence the marginal benefits and marginal opportunity costs of incremental enforcement resources. We use CWA data to calibrate this condition. We focus on water quality because water pollution remains a serious issue in the United States, detected violations by frequent CWA violators are punished severely as a matter of law and practice, and CWA data are observed at high frequencies at the facility-level.[4] Our calibration exercise involves identifying three parameters empirically. Under plausible conditions, two of these parameters can be identified directly from moments of our data. We estimate the third, enforcement response, by econometrically exploring regulated facilities' responses to variation in a proxy for expected penalties. Since the proxy may induce error-in-variables issues and expected penalties might be endogenous, we use a straight-forward application of a split-sample (two-sample) instrumental variable approach for identification. The source of variation in our empirical model is idiosyncratic changes in enforcement intensity plausibly unrelated to the behavior of the facility itself.

Our main empirical finding is that the ratio of marginal costs to marginal benefits (the 'buck per bang') is more than *ten* times higher for a marginal increase in expected penalty directed

[3] Earlier studies empirically measuring deterrence in regulatory or closely related settings are summarized in, for example, Cohen (1998), Gray and Shimshack (2011), Alm (2012), and Leeth (2012). Muehlenbachs et al. (2016) provide a recent example.
[4] See, for example, Polinsky & Shavell (1998) and Shimshack & Ward (2005).

towards frequent CWA violators than for infrequent violators. Three implications follow. First, directing marginal enforcement resources towards deterring violations by infrequent violators may improve the regulatory efficiency of maximizing compliance. CWA authorities currently punish violations by frequent violators far more severely than equivalent violations by infrequent violators; on the margin, this is counterproductive if the goal is to maximize compliance. Second, policy initiatives designed to address declining enforcement resources with an even greater focus on frequent violators may move away from optimal resource allocations. This would certainly be true in our CWA context. Third, focusing only on regulated entities' enforcement response – as in the related empirical literature – may suggest misguided policy conclusions. We find that frequent violators respond more strongly in absolute terms to marginal changes in enforcement pressure than infrequent violators. Thus, a regulator focused on enforcement response alone (i.e. the marginal benefits of punishment) may want to treat frequent violators even more harshly (in a relative sense) than present circumstances. But, this behavior ignores sanction cost effects, which in our setting swamp enforcement response effects on the margin.

Our study relates to a broader established literature exploring the optimal treatment of frequent violators, but our regulatory setting differs and the driving forces differ. The key modeling distinction between our paper and the previous literature is sanctions that are costly to the regulator. First, the most basic law and economic models of public enforcement assume social welfare maximizing agencies, zero enforcement costs, and unconstrained penalties (Shavell 2004). In contrast, we study a more realistic regulatory setting where enforcement agencies are mandated to maximize compliance and where enforcement is costly. Second, a criminal enforcement literature derives the optimal treatment of repeat offenders in the presence of personal wealth constraints and exit costs from incarceration (Emons 2003, 2007; Miceli & Bucci 2005). These and other key assumptions are not applicable to our regulatory setting. Third, an enforcement leverage literature (i.e. Landsberger & Meilijson 1982; Harrington 1988; Harford & Harrington 1991; Polinsky & Shavell 1998; Friesen 2003) studies games between regulators and facilities when penalties are costless but legally capped. Our analysis focuses on regulatory settings where enforcement is itself costly and fines never approach statutory maximums. These conditions are common in many regulatory settings. A fourth strand of the frequent violator literature stresses differing costs of compliance revealed by compliance history (Stigler 1970; Rubinstein 1979; Polinsky & Rubinfeld 1991; Chu et al. 2000). Although we share the idea that past behavior may be informative about regulated entities' response to enforcement, the mechanisms by which this heterogeneity drives regulators' optimal choices differ. Results in the related literature are driven by risks from punishing the innocent or from over-deterrence when gains are socially acceptable. We focus on costly sanctions to the regulator and on legally realistic regulatory objectives.

While our overall point is a broad one, our stylized model and empirical strategy are designed to illustrate this point in the particular context of the Clean Water Act. In this setting, there are significant variations in underlying compliance costs among a relatively small set of facilities. Given long compliance histories we model a common knowledge framework rather than an asymmetric information framework. Moreover, in our setting violations are self-reported, so costs of imposing the sanctions will be significant relative to the costs of violation detection. The framework and analysis here will be less immediately applicable to, for example, regulating worker safety or oil and gas wells across many tens of thousands of entities. In short, we offer a specific implementation and illustration of a broad point in a relatively simple institutional setting. Analyses for other contexts likely require some additional machinery and consideration of simplifying assumptions such as common knowledge.

2. Stylized Optimization Model

In this section, we present a stylized static framework as it is the simplest model that makes economic and policy ideas transparent. We begin with firms that are required to comply with some regulatory rule, such as the case of limits on discharge of pollutants. The firm can take costly actions that affect the probability of violating the rule, such as changes to production, operational procedures, or maintenance. The expected penalty from the regulator for violations is one key determinant of the amount of compliance effort, and it is the focus in this analysis.[5] We use the function $V_j\,(P_j)$ to indicate the average violations of firm j when faced with expected penalty P_j for a violation. To emphasize, P in our model is expected penalty and not the probability of detection. One standard view of firm behavior is that firms maximize profit as a function of violation rate minus expected penalties: $\pi\,(V) - P\ V$, such that $V\,(P)$ is implicitly defined by the first order condition.

Since we seek to understand the treatment of frequent versus infrequent violators, differences across firms in the function $V_j\,(P_j)$ is central. Differing types reflect heterogeneity in firms' compliance costs. The simplest setup that allows us to illustrate the key results treats the violation relationship $V_j\,(P_j)$ as fixed and known. This may be a plausible approximation, for example having been revealed to the regulator through a long history of interactions. In this case, historical compliance status is essentially a proxy for a common-knowledge compliance cost. Similarly, firms understand that the expected penalty for a violation may be higher for firms with poor compliance history. We later revisit the assumption of fixed and known types, and we consider the implications of a simple dynamic model where firms can choose the frequency of violations in order to qualify for a different penalty size.

[5] Of course, other factors such as corporate social responsibility and risk of bad publicity or activist attention also play roles in compliance effort (Kitzmuller & Shimshack 2012).

We next consider a regulator. Although the theoretical literature often uses the objective of social welfare maximization, the legal mandate of most regulatory agencies is to ensure high levels of compliance. So, we model a regulator that seeks to minimize violations across all firms given limited enforcement resources. To keep our analysis focused, and to match many regulatory settings, we consider the case where the cost to regulators of observing violations is small. One such setting is where self-reporting is incentive compatible and the primary monitoring strategy.

Levying fines, even for self-reported violations, is costly. Regulators face direct costs of levying fines via staff time, negotiation costs, court costs, and appeals expenses. Political economic costs of imposing sanctions arise, as industry groups, political appointees, and politicians may pressure enforcement authorities to treat their constituents more leniently.[6] Regulators certainly behave as if there is a resource cost to sanctions. Civil cases are seldom resolved through trial and usually settled (Glover 2001). If penalties were costless, regulators should increase at least some sanctions to the statutory maximum or to the point where there is no enforcement response. However, sanctions are typically far less than the statutory maximum and an empirical literature shows that enforcement responsiveness is typically positive in regulatory settings (Cohen (1998), Gray and Shimshack (2011), and Leeth (2012).

As discussed in more detail below, a related literature, conversations with regulators, and regulatory practice suggest that total legal, administrative, and political costs are substantial and tend to increase with the size of penalty. We therefore use the simple approximation that expected enforcement costs are proportional to expected penalty. A linear approach is a reasonable conceptual starting point as a first-order approximation given optimizing agents, and several

[6] Mintz (2014) and Shimshack (2014) discuss the importance of these costs in our subsequent empirical setting.

pragmatic reasons will suggest linearity appears reasonable in out empirical context. We return to the role of this assumption later.

Regulator's decision problem

Given stylized assumptions, we now write out the regulator's decision problem and optimality conditions. To sharpen and simplify the analysis, we consider a case with two types of firm: infrequent and frequent violators. The regulator's choice variable is the expected penalty P_j to impose for a violation by firm type j. This in turn determines the expected violation rate for each type, $V_j(P_j)$. We assume V is decreasing and convex in P. As noted above, we treat the cost of imposing the expected fine as proportional to the expected penalty, with marginal cost of c and a total cost of $c\,P_j$. Of course, fines can only be issued in the event of a violation, so the expected cost of maintaining a credible threat is $c\,P_j\,V_j(P_j)$.[7]

Denote frequent violators by subscripts f and infrequent violators by subscripts i, with $V_f(P) > V_i(P)$ for all P. The Lagrangian representation of the optimization problem with a resource constraint of R is:

$$(1) \qquad \min_{p_i, p_f} V_i(P_i) + V_f(P_f) - \lambda(cP_iV_i + cP_fV_f - R)$$

This yields first order conditions:[8]

$$(2) \qquad FOC_i: \quad \frac{\partial L}{\partial P_i} = \frac{\partial V_i}{\partial P_i} - \lambda\left(cV_i + cP_i\frac{\partial V_i}{\partial P_i}\right) = 0$$

$$(3) \qquad FOC_f: \quad \frac{\partial L}{\partial P_f} = \frac{\partial V_f}{\partial P_f} - \lambda\left(cV_f + cP_f\frac{\partial V_f}{\partial P_f}\right) = 0 \; .$$

[7] We take it as an understood implicit constraint that a regulator would not penalize frequent violators less severely.

[8] Note the standard insight about shadow costs holds; we would derive the same first-order conditions in a model where the unconstrained objective function balances compliance and enforcement costs in an additive linear fashion, provided the marginal cost of enforcement resources is constant. Note also that our assumption that V is decreasing and convex in P assures that second-order optimality conditions are satisfied at the interior solution.

The first order conditions (2) and (3) have the familiar interpretation that the marginal benefit to the objective function equals the marginal resource cost multiplied by the shadow price of the resource constraint, λ. Note that λ should be negative, as it has the interpretation as the marginal change in optimal violations from increasing enforcement resources.

For a given penalty value, frequent and infrequent violators may differ in $\partial V/\partial P$ and so respond differently to regulator interventions. Differences in $\partial V/\partial P$ are what we term the *enforcement response effect*. It might be intuitively tempting to a regulator to marginally target sanctions to the facility for which this enforcement responsiveness is highest. But, this would ignore the tensions induced by the marginal resource cost.

A key point is that the marginal resource cost will differ systematically between frequent and infrequent violators, which we refer to as the *sanction cost effect*. Costly punishment only occurs conditional on a violation, so it will be relatively expensive to credibly maintain a given level of expected penalty for a violation by a frequent violator because the regulator will be more likely to need to actually impose a penalty. In contrast, it will be relatively inexpensive to maintain the same expected penalty by an infrequent violator because the penalty would need to be imposed less frequently.

To simplify, we rearrange terms and express the first order conditions (2) and (3) in ratio form:

$$(4) \qquad \left(V_i + P_i \frac{\partial V_i}{\partial P_i}\right) \Big/ \frac{\partial V_i}{\partial P_i} = \left(V_f + P_f \frac{\partial V_f}{\partial P_f}\right) \Big/ \frac{\partial V_f}{\partial P_f}$$

Note that λ drops out of the ratio form. Note also that the marginal cost term c drops out of the ratio form and we do not need its value to generate our key insights.[9] With λ and c dropping out,

[9] We later explore an implication of allowing cost to differ by type. In this case, the left side of (4) is multiplied by c_i and the right side of (4) is multiplied by c_f.

(4) shows the standard result that the ratio of marginal cost to marginal benefit is equal across types at an optimum.

A further simplification of (4) is possible. Rearranging terms yields:

$$(5) \qquad P_i + \left(V_i \middle/ \frac{\partial V_i}{\partial P_i} \right) = P_f + \left(V_f \middle/ \frac{\partial V_f}{\partial P_f} \right)$$

Equation (5) is simply a restatement of the requirement in (4) that the ratio of marginal costs to marginal benefits be the same across types. Equation (5) distills the insights of our model into a form that is convenient to operationalize and so serves as the foundation of our subsequent empirical analysis.

3. Empirical Setting and Data

We now turn to the relative importance of factors driving the optimal treatment of frequent and infrequent violators in our Clean Water Act (CWA) setting. The CWA requires all water pollution dischargers to hold permits that specify regulated contaminants, pollution limits, and compliance schedules. Effluent limits are technology-based and influenced by sector, sub-sector, and facility size. Realized pollution discharges are stochastic, but a facility can influence average pollution discharges over some period via production controls, abatement equipment and operation, and process modification. Establishment-level compliance can typically be changed rapidly through incremental attention to training, maintenance, and operations. So, changes in compliance do not require large expenditures on new abatement equipment.[10]

[10] Primary wastewater treatment involves increasingly fine screening, as well as settling tanks that allow solids to sink to the bottom for removal and oils and foams to rise to the top for skimming. Secondary wastewater treatment involves biological processes where microorganisms breakdown organic matter into less harmful bi-products. These processes are sensitive to small changes in temperature, acidity, light, nutrients, substrate age, and weather, so small changes in attention and maintenance can have large impacts on discharges and compliance. Tertiary treatment, involving chemical or physical-chemical treatment procedures, is less common in our setting.

Four further issues about CWA oversight bear noting. First, states have primary authority for all permitting, monitoring, and enforcement activities under the CWA. Despite a federally mandated legal structure, states are the regulatory agencies for all practical purposes. Second, like many other statutes and regulations, the CWA generally requires agencies to minimize violations, rather than to focus on other objectives like social welfare maximization or minimizing net pollution damages irrespective of permitted obligations. Third, administrative penalties are the workhorse of CWA pollution enforcement. Civil and criminal judicial actions are authorized under the Act, but in practice they are rare for standard water pollution violations.[11] Less formal sanctions like warning letters and notices of noncompliance are common, but the literature suggests that these sanctions 'without teeth' may have little effect on compliance decisions of the large industrial facilities that we study (Shimshack & Ward 2005; Gray & Shimshack 2011). Fourth, CWA agencies face tightly constrained budgets. Agencies do not recoup assessed penalties.[12]

Under CWA enforcement guidelines, authorities have considerable discretion over when and how to issue penalties. Guidelines suggest decisions to impose penalties and penalty amount determinations should be a function of environmental harm, the facility's expected financial gain from the associated violation, the facility's compliance history, the penalized facility's ability to pay and economic condition, the facility's intent, fairness considerations, the strength of the legal evidence, and other factors (USEPA 1989). Assessing, processing, and quantifying and these factors involves considerable discretion and necessitates extensive personnel and legal costs, even for low penalty amounts (USEPA 2000; Mintz 2012; Shimshack 2014). Even strictly

[11] Civil and criminal actions are typically reserved for situations with extreme harm (i.e. BP oil spill), deliberate lying or records falsification, or unpermitted operations (Uhlmann 2009).

[12] Additionally, agencies with successful enforcement are not typically rewarded with higher budgets in the future. For example, the real budget for EPA's Office of Enforcement and Compliance Assurance remained relatively unchanged between the late 1990s and the late 2010s (Shimshack 2014). State enforcement budgets are somewhat more variable (Blundell et al. 2021), but we are unaware of cases where budgets increased due to successful enforcement years.

administrative penalties typically require carefully documented legal hearings before administrative law judges or similar state-level authorities. Moreover, sanctions generate significant nonmonetary political-economic costs for regulators due to community, industry, and political pressure. Stakeholders, from firms to presidential staffers, can actively seek to influence enforcement policy and practice (Bressman & Vandenbergh 2006; Mintz 2012; Shimshack 2014).

Data and sample

Our calibration exercise uses facility-by-month compliance and enforcement data from the USEPA's Permit Compliance System, the Integrated Compliance Information System – National Pollution Discharge Elimination System, and the Enforcement and Compliance History Online databases. We observe pollution violations and fines for the universe of CWA "major" industrial wastewater dischargers during our sample period. We focus on major facilities because minor facilities are not necessarily required to report frequently and because states were not necessarily required to relay their reports to EPA databases during our sample period.[13] Supplemental sources of data include county-by-month weather data aggregated from the Global Historical Climatology Network. We obtain state-by-year community characteristics from the Bureau of Labor Statistics, Census Bureau, and Elections Commission.

Our balanced sample covers 1001 industrial CWA major facilities over the 77-month period from January 2000 to May 2006. We observe violations and fines for all facilities and all months. We collected compliance data from January 1999 to May 2006 to allow for lags and enforcement data from January 1999 through December 2007 to allow for both lags and leads.

[13] Industrial majors are those that discharge more than one million gallons of wastewater per day and/or have potential for significant environmental impact.

Sample start and end dates were chosen for data consistency and reliability reasons.[14] We consider all major CWA industrial enterprises in the 29 US states with 7 or more major facilities with continuous effluent compliance measures during our sample time period.[15] Roughly 46 percent of sample facilities produce chemicals, petrochemicals, and associated products; 19 percent produce wood, paper, and associated products; 17 percent produce metals and associated products; and 18 percent produce other products including food and textiles. *Figure 1* summarizes the spatial distribution. Facilities are predominantly located in the eastern half of the US, with a majority located in Gulf Coast and Mid-Atlantic states.

Operationalizing compliance and enforcement

Given available data and CWA institutions, we classify noncompliance events as follows. We define a violation as a regulator-designated significant noncompliance (SNC) occurrence for effluent discharges. The advantages of defining discharge violations by SNC are that the measure covers all pollutants simultaneously and provides an externally standardized measure of severity. Because the exact criteria for SNC effluent designations can be complex, it is difficult to fully explain SNC construction here. However, as a general rule, SNCs are triggered when a facility: exceeds permitted allowances for conventional water pollutants (like biochemical oxygen demand and suspended solids) by more than 40 percent multiple times over some monitoring period; exceeds permitted allowances for other water pollutants (like inorganic chemicals and heavy metals) by more than 20 percent multiple times over some monitoring period; exceeds permitted allowances for any contaminant by any amount for several monitoring periods in a 6 month

[14] Historical CWA data in federal EPA data systems becomes available and reliable on a systematic scale around 1998 or 1999, and data migration between EPA systems beginning in June 2006 caused some pollution and compliance information to be inconsistently tracked.

[15] An instrument in our empirical analysis is based on penalties on other facilities in the same state, so we need our sample to include states with at least a reasonable number of CWA major industrial facilities. We do not consider municipal wastewater treatment plants. Although these facilities are important causes of point source water pollution, municipal ownership and operation are unlikely consistent with the basic economics in our conceptual framework.

window; or exceeds permitted allowances for any contaminant in such a manner as to potentially cause serious water quality or human health issues (USEPA 1996).

For CWA majors, self-reporting is the dominant form of monitoring.[16] Although self-reporting may not be completely accurate, we assume that reasonably accurate self-reporting is plausible in our context. We later revisit this assumption.

Administrative penalties with monetary fines are the main form of meaningful enforcement for SNC water pollution violations. Although administrative actions may include field citations in some states, the overwhelming majority are issued by administrative law judges. Notably for this work, CWA enforcement guidelines dictate sanction frequency and severity should be influenced by offense history. In practice, a single penalty may cover multiple violations. Many SNC violations are never formally penalized.

4. Empirical Analysis

The goal of our empirical exercise is to calibrate the first-order optimality condition from Section 2 using data from the CWA setting described in Section 3. We reproduce our first-order condition in (5) for convenience:

$$(6) \qquad P_i + \left(V_i \middle/ \frac{\partial V_i}{\partial P_i} \right) = P_f + \left(V_f \middle/ \frac{\partial V_f}{\partial P_f} \right),$$

for types i (infrequent violators) and f (frequent violators). This optimality condition has three basic terms that may differ for across types: expected penalties P, violation rates V, and an enforcement response term defined as the marginal impact of penalties on violations, $\partial V / \partial P$. The average value of the first two of these terms can be reasonably approximated with empirical

[16] Self-reporting is also a common monitoring strategy under several OSHA, FTC, FAA, and FDA programs.

moments from the dataset. However, approximating the average enforcement response, $\partial V / \partial P$, requires econometric analysis.

A regression equation explaining facility compliance behavior would ideally relate facility violations V to some function of facility beliefs about penalties conditional on violation, P^b, and other standard explanatory factors.

$$(7) \qquad V = X\mathrm{B} + \gamma P^b + \varepsilon \quad ,$$

where V are violations, X are standard covariates, and P^b is some function of beliefs about punishment conditional on a violation. In our empirical setting, P^b is the log of expected penalty and violations are linear in the logarithm of penalties conditional on a violation.

There are two core problems when identifying γ in (7), the causal parameter of interest. The first is that beliefs, P^b, are not observed directly. The second is that even if beliefs P^b were observed directly, the error term might be correlated with beliefs. Our approach to both predicaments is to replace the unobservable subjective belief P^b with a proxy prediction $\hat{P}$ based on plausibly exogenous observables.

To motivate our proxy variable approach, suppose first that P^b were observed directly. In that case, a standard two-stage least squares approach would address the endogeneity concern. A first-stage would construct proxy variables $E\left(P^b \mid X, Z\right)$, i.e. expectations of beliefs conditional on some exogenous instrument set Z and other explanatory variables X. Although beliefs P^b are not observable, actually realized penalties P^r are observable. Assuming that facilities do not suffer systematic bias in their enforcement expectations conditional on the instruments, $E\left(P^b \mid X, Z\right) = E\left(P^r \mid X, Z\right)$. Thus, under generally plausible conditions, using a proxy prediction from a first-stage procedure regressing actual penalties for violations P^r on an instrument set Z and exogenous explanatory variables X addresses the problems created by observability and endogeneity.

Of course, since penalties can only be imposed in response to a violation, the first-stage described above can only include the relevant subset of observations corresponding to observed violations. As such, the practical procedure of constructing and using proxy predictions $E\left(P^r \mid X, Z\right)$ in a regression model is a split-sample (two-sample) instrumental variables procedure (Angrist & Krueger 1992, 1995; Dee & Evans 2003; Angrist & Pischke 2008).

Implementing the strategy

The mechanics of our split-sample instrumental variables procedure are straight-forward. First, we follow a related literature and identify instruments *Z* based on plausibly exogenous determinants of the state regulators' reputation for toughness at a given time. Second, for facility-months with violations (denoted sample 1), we regress the observed penalties P^r_1 on explanatory variables X_1 and instruments Z_1:

(8) $$P_1^r = X_1\Theta + \delta Z_1 + \mu_1 .$$

We then use estimated coefficients from (8) to form the predicted penalty proxy for the full sample of facility-months (denoted sample 2):

(9) $$\hat{P}_2 = X_2\hat{\Theta} + \hat{\delta} Z_2 .$$

Finally, in the spirit of two-stage least squares, the penalty proxy becomes the explanatory variable of interest in the behavioral equation for the full sample of facility-months:

(10) $$V_2 = X_2\mathrm{B} + \gamma \hat{P}_2 + \varepsilon_2 .$$

This two-stage approach provides consistent parameter estimates. The only substantive difference between the approach outlined above and the standard two-stage least squares inferential procedure is the sample for the first stage.

Of course, practical implementation of this approach requires an instrument Z. To be valid, Z must be correlated with the expected penalty in a given period (i.e. their reputation for toughness with the facility). Expected penalty differs markedly over space and time, and differences are driven by shocks to local political, economic, and budgetary conditions (Shimshack 2014). A natural source of information on expected penalty at a given time is that agency's visible recent history of sanctions (Sah 1991). A related deterrence literature uses penalties levied on *other* firms in the same jurisdiction (i.e. state) in the recent past as an indicator of regulator reputation for strictness (Shimshack & Ward 2005, 2008). We use the same variable as an instrument in the construction of our proxy $\hat{P}_2$ from the first-stage of our two-stage least squares procedure.

We construct Z to plausibly satisfy the standard exclusion restriction that Z must not influence facility behavior in the period of interest through a channel other than expected penalty, at least in a conditional sense. First, we omit penalties on the facility itself to avoid reverse causality induced by regulator targeting. Second, we lag penalties on others to avoid contemporaneous correlation at the state level. Third, we condition on year indicators to avoid spurious correlation caused by common time trends. Nevertheless, we do need to assume that lagged penalties on others in the same jurisdiction are then conditionally uncorrelated with the error term. Put differently, the identifying assumption is that there is no *unmodeled* omitted variable correlated with the error term. Although it remains possible that an unmodeled serially-correlated state-specific omitted shock could induce a correlation with the error term, we later consider the evidence and find the concern unlikely to drive results.

Implementation of our exercise also requires operationalizing compliance types empirically. Our main analysis divides the sample of facilities into two fixed groups corresponding to the total number of violations over the full compliance sample. In this case, we run the main regression separately for each group, i.e. we estimate the marginal impact of penalties on compliance separately for frequent violators and infrequent violators. The initial assumption of time invariant and exogenous compliance types seems reasonable, as the large industrial facilities in our sample have typically been regulated under similar CWA conditions for decades and this long history of interaction would suggest that type had been largely revealed.

That is, it seems unlikely that the regulator is learning much new about facility type over our particular sample period. Nevertheless, we acknowledge that group transitions may occur in some instances, like after management changes or installation of new equipment, so we later explore sensitivity to allowing transitions between types based on recent compliance history. We also consider the evidence for a dynamic model where facilities may strategically choose the frequency of violations in order to qualify for a different penalty size (i.e. a different type).

We note several empirical specification details. To fix ideas in presentation, equations (7) – (10) illustrated the simplest fully linear cases. However, in our econometrics we pragmatically follow the related literature by defining all penalty variables using a natural logarithmic transformation of fines plus one.[17] The log transformation makes the penalty data less skew and adding one sets the transformed value to zero when there is no fine. In first-stage regressions, the dependent variable is log fines attributed to the given violation plus one.[18] In first-stage regressions, the key regressor is the regulator reputation effect proxy constructed by summing

[17] All results in the paper are robust to using inverse hyperbolic sine (IHS) transformations instead of log fines plus one wherever applicable. Given no difference between log fines plus one and IHS when fines are 0, by construction, and mean and median fines conditional on positive values in the thousands of dollars in our sample, practical differences between log fines plus one and IHS are trivial in this context.

[18] We assign fines to specific effluent violations by apportioning each fine to those violations at the facility in the preceding twenty-four months. Although penalties in our dataset are not directly linked with violations, we were able to identify legal records for some of the fines and those fines were indeed for the SNC violations we assigned them to. For our attribution procedure, if a single fine covered multiple violations or multiple fines were preceding by a single violation, we proportionately assigned fine magnitudes to violations. Results are generally robust to assigning fines based on SNC violations in the past 12 months rather than 24 months.

logged fines plus one in the past year over all other facilities in the same state, scaled by the number of facilities.[19] In second-stage regressions, the key dependent variable is a violation by facility j in period t. Since this is a limited dependent variable, we estimate both linear probability and probit models. The key explanatory variable is the predicted penalty proxy for facility j in period t.

Other specification details are as follows. In all regressions, covariates X consist of standard explanatory factors including rainfall, year dummies, seasonality dummies, and industry (SIC) dummies. Some specifications include state or facility fixed effects. Further specifications include time-varying local community characteristics such as unemployment, income, and Republican vote share. In all regressions, frequent violators are initially defined as those with four or more violations (months of violation) during the January 1999 to May 2006 compliance period. Infrequent violators are facilities not defined as frequent violators. We later explore robustness to alternate type definitions. Standard errors are clustered at the state-level unless otherwise noted.

Econometric results

Figure 2 summarizes levels and trends in our enforcement and compliance data. Violations are mildly seasonal, with slightly more SNC violations in the winter months. Violations decline by roughly 25 percent over the early years of our sample but remain roughly constant from 2002 to 2006. In total, we observe 2,410 SNC violations between January 2000 and May 2006. In any given month, about 3.1 percent of facilities have a SNC violation. About 33 percent of facilities violate at least once during the full sample period. Administrative fines are mildly seasonal, peaking in the summer months. Fine numbers decline steadily over the sample period, but fine magnitudes remain constant. In total, we observe 341 administrative fines between January 1999

[19] This regressor represents an instrument rather than a traditional explanatory variable. As such, its key requirements are relevance and satisfying an exclusion restriction, as discussed above. Note that recent violations at other facilities were not typically observable to facilities.

and May 2006.[20] In the full sample, the median penalty was $5,000 with an interquartile range of $1,300 to $19,000. *Figure 2* also documents that, in the raw data, infrequent violators have fewer violations and fewer fines per facility over the sample period. As measured by the median and interquartile range, on average infrequent violators are subject to lower monetary penalties.

We next explore the notion that frequent violators are punished more severely than infrequent violators on a per violation basis in more detail. *Figure 3* summarizes average fines per violation across types in the raw data and considers robustness of the result across different types. Note that one fine may address more than one violation. The graphs in the center column use baseline definitions for frequent and infrequent violators. The key lesson from *Figure 3* is that CWA frequent violators are penalized significantly more harshly than infrequent violators for similar infractions. The top panel illustrates that fines per violation are roughly six times higher for frequent violators than for infrequent violators. Since many violations are not penalized, the top panel includes $0 penalties. The bottom panel replicates the comparisons excluding $0 penalties, i.e. conditional on a fine being levied. Conditional fines per violation are still roughly three times higher for frequent than for infrequent violators.

Table 1 presents main enforcement response econometric results. *Table 1* columns [1] and [2] present estimates from our first-stage regressions. Key results are in the first row. Lagged fines on others in the same state, the sources of identifying variation, are economically and statistically significant predictors of future fines conditional on violations. So, facilities can learn about their expected penalty by observing the regulator's recent behavior. The other coefficient of direct interest is in the third row. Interpreting coefficient magnitude on the frequent violator dummy

[20] These administrative fines typically address discharge violations, but some penalties may also address paperwork and scheduling violations. Penalties for all types of violation can and do help facilities predict overall regulatory toughness and expected penalty conditional on discharge violation.

variable of roughly 1.1, observed penalties conditional on a violation for frequent violators are roughly twice as large as observed penalties conditional on a violation for infrequent violators.[21] The magnitude of the frequent / infrequent violator penalty difference relative to the comparable difference in the raw data (i.e. in *Figure 3*) is due to the inclusion of covariates. *Table 1* columns [1] and [2] also include estimates for controls that are included because they belong in the second-stage equation. First-stage F-statistics for columns [1] and [2] are roughly 12 and 88.

Table 1 columns [3] through [10] present estimates from our second-stage violation / compliance equations of interest. The key results are in the second row. Looking at columns [3] through [6], we find that the penalty proxy is an economically and statistically significant predictor of violations for infrequent violators. Looking at columns [7] through [10], we find that penalty proxy is also an economically and statistically significant predictor of violations for frequent violators. Comparing coefficient magnitudes across types shows that frequent violators are absolutely more sensitive to marginal changes in expected penalty. However, when scaled by average propensity to violate, calibrated as the facility type's average violation rate over the full sample period, infrequent violators are more sensitive to marginal changes in expected penalty. We find a relative deterrence elasticity for infrequent violators of around .15 to .20 and a relative deterrence elasticity for frequent violators of around .08 to .10.[22]

We next discuss effects of control variables in our second-stage violation equations. We find that rainfall is a significant predictor of violations for infrequent but not for frequent violators. This is consistent with accidental or stochastic factors importantly influencing average compliance outcomes for infrequent violators but not for frequent violators. We find few reliably significant relationships between our community characteristic controls and violations. This may be

[21] In log-level specifications, the percent impact of a dummy switch from 0 to 1 is 100[exp(coefficient) - 1].
[22] In level-log specifications, the point elasticity is the coefficient divided by the baseline for the dependent variable.

consistent with community characteristics influencing compliance indirectly through regulatory channels but not directly through other channels.[23] As in the raw data, violations trend downward over time, are seasonal with peaks in winter months, and are more common among chemical and associated product producers.

Empirical Implications

Our empirical results put us in a position to answer our motivating question using CWA data: Should a regulator attempting to minimize violations subject to a fixed budget constraint and costly sanctions reallocate the marginal enforcement dollar towards violations committed by frequent violators or towards violations committed by infrequent violators? To answer the question, we plug our estimates into an empirical analogue of equation (6).

As previously noted, the average value of expected penalties P and violation rates V for frequent violators (type *f*) and infrequent violators (type *i*) can be approximated from moments of our sample data. The marginal impact of penalties on violations, the enforcement response effect $\partial V / \partial P$ for each type, was the subject of our econometric estimation. The only additional issue is that our empirical analogue of (6) requires a transformation given the level-log specification. Note if $V = X\mathrm{B} + \gamma \log P$, then $\partial V / \partial P = \gamma / P$. Thus, equation (6) can be rewritten as:

(11) $$P_f\left(1 + V_f / \gamma_f\right) = P_i\left(1 + V_i / \gamma_i\right).$$

The condition in (11) still has the natural interpretation of a standard first-order condition. The ratio of marginal costs to marginal benefits must be the same across types for the regulator to optimally allocate enforcement resources. If the two sides are not equal, the regulator is

[23] Other papers using CWA data find similar results (Shimshack and Ward 2005, 2008). We do find statistically significant relationships between vote share and violations for infrequent violators and between unemployment and violations for frequent violators. It is possible that vote share and unemployment is correlated with production for these facilities, although time varying production data are not available at the facility level. We note that inclusion or omission of community characteristics has little impact for key enforcement sensitivity estimates.

inefficiently allocating enforcement resources if its goal is to maximize compliance. If the absolute value of the left-hand side of (11) is larger than the absolute value of the right-hand side, then the ratio of marginal costs to marginal benefits from deterring violations by frequent violators is higher in practice than the ratio of marginal costs to marginal benefits from deterring violations by infrequent violators.

Plugging in the empirical parameters necessary to calibrate (11) yields:

$$(12) \qquad \left|3076\left(1-\frac{.1307}{-.0115}\right)\right| \quad > \quad \left|498\left(1-\frac{.0041}{-.00059}\right)\right|.$$ [24]

The left-hand side of (12) is more than ten times larger than the right-hand side.

The results in (12) suggest the ratio of marginal costs to marginal benefits from deterring CWA violations by frequent violators is more than *ten* times greater than the ratio of marginal costs to marginal benefits from deterring CWA violations by infrequent violators. We bootstrapped the test statistic using 5000 replications. The ratio of marginal costs to marginal benefits remained higher for frequent violator infractions than for infrequent violator infractions in more than 99.3 percent of replications. We reject a null hypothesis that CWA regulators are optimally deterring violations across types.

5. Robustness

Revisiting stylized assumptions

The optimization framework and empirical results outlined above rely on three main stylized assumptions. The first stylized assumption is that facilities expect that penalties are based on common-knowledge compliance cost, *i.e.* type is fixed and known. The second stylized

[24] 3076 and 498 represent the mean (expected) penalties per observed violation, by frequent and infrequent violator types. Recall that these values of P are low relative to overall average penalties in the dataset because many violations are not fined and because many fines address multiple violations. 0.1307 and 0.0041 represent the number of significant violations per facility per month, by frequent and infrequent violator types. -0.0115 and -0.00059 are the econometrically estimated enforcement sensitivity parameters obtained previously.

assumption is that total administrative, legal, and political costs are roughly proportional to expected penalty, *i.e.* enforcement costs are significant, increasing in *P*, and linear in *P.* Third, the cost to regulators of observing violations is small due to reasonably accurate self-reporting. Here, we discuss the implications of relaxing these assumptions as well as the rationale for using them.

Fixed and known type. Our main analysis is implemented as if type is fixed and known to the regulator ex-ante. The idea is that average propensity to violate may be largely a function of facility characteristics that change slowly over time, and a long history of regulatory interactions preceding our sample period revealed type for most facilities. An advantage of this approach is that it facilitates a simple one-period model that illustrates our main economic points as sharply and clearly as possible. A disadvantage is that this potentially strong assumption may have both conceptual and empirical implications.

In *Appendix B*, we show how our conceptual analysis can be generalized to consider a regulator committing to a penalty schedule and allowing facilities to optimize along that schedule by choosing violations after accounting for the influence of current violations on possible future penalties. In other words, the regulator's fine schedule is now considered to be a function of violations (rather than fixed) and a facility may choose its frequency of violation in order to qualify for a different penalty size. The core insight of the model with these dynamic elements is that the first order conditions include both the expected penalty *P* and the marginal expected penalty $P'(v)$ from changing violation rate. As such, facility compliance decisions may depend not only on the current penalty but also on the difference in future penalties caused by being an offender now. An additional insight is that the importance of the marginal expected penalty $P'(v)$ diminishes if the regulator's index of violation history updates slowly. Ultimately, the impact of the dynamic term is an empirical question.

We first adopt an approach suggested by Kang and Silveira (2021) and explore empirically if the regulator updates its index of violation history quickly or slowly. For this purpose, we augment our first stage approach to include the facility's own additional violations in the past year. In other words, we explore the association between an observed penalty for a given violation and the facility's past violations in the previous year. As documented in *Appendix Table 1*, this association is not significantly different from zero in our dataset. Coefficients are 0.027 to 0.030 with standard errors of 0.046 to 0.048. Economically and statistically insignificant relationships suggest that the speed with which the regulator updates in violation history index is slow in practice. Observed penalties do not appear to be sensitive to short-run or medium-run variability in violations.

Second, we consider empirically the importance of the key dynamic term: the difference in expected penalty between frequent and infrequent violators, P_f - P_i . Since counterfactual penalties are not directly observable in the data, our approach is to include in the main regressions a plausibly exogenous proxy for the firm's belief about the difference in penalties by type. Our proxy is the difference in the statewide average penalty for frequent offenders minus the same statewide average for infrequent offenders. As in the main analysis, these averages are taken over facilities not including facility *i* to avoid including a facility's own behavior in the construction of the proxy variable for beliefs. Construction of such an average is straightforward; it is simply a function of the sum of all penalties on other frequent (or infrequent) offenders in the same state divided by the sum of all violations by other frequent (or infrequent) offenders in the same state.

We then augment our main regressions presented in Table 1 with the proxy representing the difference in expected penalty across types as an additional explanatory variable in the regression. Results in *Appendix Table 2* show that the association between facility *i*'s violation in period t and the difference in average penalties between other frequent and infrequent violators in i's state is not significantly different from zero for either facility type. Moreover, estimates of our main relationships between i's violations and i's predicted expected penalty are statistically similar with and without the proxy.

Taken as a whole, we fail to empirically detect evidence against the simple static firm response to expected penalties in our CWA context. We acknowledge that dynamic penalty considerations might be important in other settings. For example, Blundell (2020) and Blundell et al. (2020) demonstrate dynamic enforcement increases compliance in a Clean Air Act High Priority Violation context. In contexts where dynamics matter, one could include the difference in penalties across frequent and infrequent offenders as a determinant of violations for both when considering optimality. Under these circumstances, it is straightforward and mechanical to check the optimality condition analogous to (5) that accounts in the first order conditions.

Sanction cost proportional to expected penalty. The proportionality assumption is not critical to our conceptual framework in the sense it would be straightforward to adapt a model with the sanction cost effect to some alternative and known relationship between penalty size and sanction cost. However, this assumption could be relevant for our empirical findings.

A related literature and several enforcement practices appear consistent with enforcement costs increasing with the size of the penalty. Diver notes, "Reduction of a penalty otherwise appropriate may well be justified [by the agency] by savings in … collection costs or the avoidance of a significant risk of failure" (Diver 1979, pg. 1471) and "A high [penalty] assessment may induce a recipient to hire a lawyer or prepare elaborate documentary defenses, when a more moderate assessment could have produced a quick settlement" (Diver 1979, pg 1483). In practice, CWA authorities pursue the minimum sanction necessary to achieve a return to compliance and some deterrence objective (EPA 1989). Maximum allowable sanctions are rarely imposed, if ever. Administrative penalties are strongly prioritized over civil penalties. Enforcement attorneys try to avoid time consuming and costly litigation (Mintz 2012; Shimshack 2014). In our raw data, we detect a positive correlation between fine magnitude and the time lag between the dates of violation and penalty assessment.

Although the literature is largely silent on how costs may more precisely relate to the size of the expected penalty, total enforcement costs (administrative, legal, political) approximately proportional to expected penalty appear consistent with CWA regulatory behavior. Recall that the *expected penalty* in our setting has two components. Expected penalty is the probability of imposing a sanction times the amount of the sanction imposed. An important stylized fact in our case study is that sanctions are usually not imposed at all; most violations are not formally sanctioned. So, perhaps the most direct and obvious lever by which the regulator can change the expected sanction is to change the probability that a sanction is imposed conditional on a violation. As an example, doubling the probability of any given sanction would double the number of times that sanction is imposed, and so plausibly double the associated costs while doubling the expected penalty. More generally, it is intuitive that sanction costs would scale roughly proportionally to changes in expected sanction caused by changing the probability of sanction rather than the penalty size conditional on a financial sanction. Our argument is essentially that regulators *can* change expected penalty at cost proportional to that penalty, and a cost-minimizing regulator would do so unless the probability of sanction was already one. In our CWA case study, the sanction probability is never one.[25]

[25] One might expect to see sanctions imposed with probability one if the cost of imposing a sanction is convex in the magnitude of sanction, so the regulator would choose to have lesser penalties as often as possible to maintain deterrence. But this is not the case. In contrast, if the cost of imposing a sanction is concave in the magnitude of sanction, one would expect to see sanctions as large as possible imposed as rarely as possible to maintain deterence.

Finally, our main findings are robust to the proportionality assumption. Suppose that we knew precisely the cost of enforcing an expected penalty, and suppose the costs are non-linear in expected sanction. In our two-type stylized model this could be represented simply by having the marginal cost term c differ by type (c_i and c_f), or more precisely, differ according to the expected penalty for each type. To update the optimality condition for different cost types, we simply adjust by adding appropriate subscripts, yielding c_i on the left side and c_f on the right side. Carrying this through to a simplification analogous to equation (5), we get:

$$(13) \qquad c_i \left\{ P_i + \left(V_i \Big/ \frac{\partial V_i}{\partial P_i} \right) \right\} = c_f \left\{ P_f + \left(V_f \Big/ \frac{\partial V_f}{\partial P_f} \right) \right\}$$

Our empirical findings are that the term in brackets in the right side of equation (13) was about 10 times as large as the term in brackets in the left side of equation (13). In order to reverse the result that directing marginal enforcement resources towards deterring violations from infrequent violators would improve regulatory efficiency, we would need to have marginal cost c_i be at least 10 times as large as marginal cost c_f. As a matter of practice, this seems implausible.

As a thought experiment, suppose that c represented solely the financial cost per dollar of the expected sanction (and not the total cost, including non-financial costs) and that costs per dollar of penalty are much higher for infrequent offenders. This would imply a strongly concave relationship between the cost of sanctions and the magnitude of sanctions. However, as noted, such a relationship is inconsistent with observed regulator behavior, since sanction costs that are concave in penalty magnitude imply that the most cost-effective way to maintain a given level of

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But, in this case variation in expected penalty would be driven by variation in sanction probability (given capped fines), which again leads to sanction costs proportional to expected penalty. However, neither convexity nor concavity of sanction costs with fine magnitude is consistent with the observed regulator behavior that CWA fines are rarely set to statutory maxima and are also rarely imposed. In constrast, observed regulator behaviour is consistent with a model of sanction costs proportional to expected penalty.

deterrence is maximal fines. This is not observed in the data, as anything approaching maximum fines are rarely, if ever, imposed.

Low-cost monitoring. The assumption of low costs to regulators of observing violations is unlikely to influence our conclusions. A key point of our conceptual framework is that the sanction cost effect would still exist even in a model where monitoring is costly. Regardless of how violations are uncovered, sanctioning frequent violators requires higher and thus more costly expected sanctions to maintain a given threat of punishment, exactly because that threat must be frequently backed up for frequent violators.[26]

One natural concern is that frequent violators anticipating higher penalties may spend more time hiding violations. Such concealment, however, could simply be thought of as an additional enforcement cost in our model. Innes (2001) makes a similar conceptual point, noting that an advantage of self-reporting is that it can reduce the likelihood of costly concealment. In the event that frequent violators are disproportionately prone to concealment, our key result that the ratio of marginal costs to marginal benefits is higher for frequent violator infractions than for infrequent violator infractions is only magnified.

We also note that a related literature suggests that accurate self-reporting is be a reasonable working assumption (i.e. Cohen 1992; Malik 1993, Kaplow & Shavell 1994). Regulators can and do conduct regularly scheduled and ad hoc 'for-cause' on-site inspections. Theory shows that self-reporting systems can be incentive compatible, particularly when penalties for self-reported violations are low and penalties for deliberate falsification are disproportionately high (Malik 1993; Kaplow & Shavell 1994). This is the case under the CWA, as most water pollution sanctions are in the thousands (or tens of thousands) of dollars range. In contrast, deliberate records

[26] Two caveats bear noting. One, we assume the optimization problem has sufficient curvature for an interior solution. Two, we assume that the penalty cannot practically be set so high that there is no risk of violation.

falsification can result in incarceration and penalties exceeding hundreds of thousands or even millions of dollars. In many policy contexts, severe sanctioning for misreporting can include personal criminal liability for an employee who falsifies a report thus creating a wedge between the principal and agent incentives of the owner/manager and the reporting employee (Cohen 1992).

Finally, a growing literature applies forensic economic tools to CWA industrial facility discharges and fails to reject accurate reporting. Studies investigating the accuracy of North American industrial water pollution data and failing to reject a null of accurate reporting include LaPlante & Rilstone 1996; USEPA 1999; Shimshack & Ward 2005; and Evans et al. 2018. We also explored the accuracy of self-reporting in the spirit of these papers and also failed to reject accurate reporting.[27]

Other Empirical Sensitivity

Omitted variables. Our empirical exploration presumes that lagged enforcement actions directed towards others, i.e. the agency 'reputation' proxy, is conditionally exogenous in the second-stage regression and thus satisfies an exclusion restriction. Although we construct this variable omitting the facility itself and with lags to mitigate endogeneity concerns, it remains possible that specific types of omitted variables could bias results. One natural concern is that enforcement intensity and violations may be correlated through time-varying national shocks. However, our analysis includes year indicators. Another worry is the lagged proxy is correlated with the error term in the second stage violation equation via a state-specific omitted variable influencing both overall enforcement intensity and compliance in the state. However, we replicated

[27] For example, one natural presumption is that self-reporting is more likely to be accurate in periods where a regulatory inspector is present at the facility. Provided that plants are unable to rapidly reduce their discharges to the average reported level when an inspector is present, one would expect a positive correlation between the likelihood of a reported violation and the presence of an inspector. In our data, we find no correlation between inspections at i in t and reported violations at i in t. This is true for all inspections and for sampling inspections, where an inspector may be onsite for days or weeks. This is true for infrequent and frequent violators.

the analysis with state-level fixed effects and found similar results. *Appendix Table 3* presents results. Columns [1] to [2] and columns [4] to [5] are generally similar. A specification test fails to reject the null of no difference in coefficients across specifications with and without state-level fixed effects.[28]

A more subtle threat to identification is an unmodelled time-varying, state-specific political or economic shock. To fix ideas, consider a statewide positive economic shock. Such a shock might, for example, induce facilities to violate more often due to enhanced profit opportunities while also injecting resources into the state environmental budget and increasing overall enforcement intensity. We believe such shocks are unlikely to drive our results for several reasons. First, the shock must be persistent to bias the results, since our measures are lagged. Second, some of our specifications include time-varying state-level covariates like unemployment, per capita income, and Republican vote share. These measures are intended to pick up the most obvious of time-varying omitted variables, yet including or omitting them has almost no effect on coefficients for the 'regulator reputation' variable. Coefficients on the penalty proxy in *Table 1* columns [3] vs. [4], [5] vs. [6], [7] vs. [8], and [9] vs. [10] are statistically indistinguishable and within 4 percent of each other. Had time varying statewide shocks been importantly driving results, we would have expected to find meaningful differences in estimates between specifications omitting or including the most obvious of statewide economic or political variables (i.e. unemployment, income, vote

[28] For reference, *Appendix Table 3* also presents results from regressions with facility fixed effects. These specifications remove statistical identification associated with relative persistent facility-level characteristics like ownership and size that might influence a facility's propensity to contest a penalty. Much of the identifying variation has been swept out of the model. Empirical estimates for infrequent violators were statistically similar with and without facility-level fixed effects. Empirical estimates for frequent violators were smaller and somewhat more noisily estimated. Note, however, that using the (smaller) fixed effects estimates for frequent violators in our final calibration exercise (next section) simply increases the magnitude of the final empirical result and does not impact the exercise's qualitative message nor its statistical significance.

shares). Of course, our three measures don't capture all possible omitted variables, but the evidence available to us is at least consistent with small bias due to time-varying omitted variables.[29]

Frequent and infrequent violator definition. Our analysis divides the sample into frequent and infrequent violator types defined by the number of violations over the entire sample period. Our specific cut-off is 4 violations, which splits facilities into groups of roughly 20 percent frequent and 80 percent infrequent violators. To explore robustness, we replicated the analysis with cut-offs at 3 and 5 violations. *Appendix Table 4* presents results. Penalty proxy coefficients for infrequent violators in columns [1] and [3] are statistically similar and within 18 to 31 percent of the main estimates in column [2]. Penalty proxy coefficients for frequent violators in columns [5] and [7] are statistically similar and within 17 to 37 percent of the main estimates in column [6].

We also replicated our analysis allowing type transitions during the sample period. We defined frequent violators as those facilities with a violation in the past year and infrequent violators as those facilities without a violation in the past year. *Appendix Table 4* demonstrates that results are reasonably robust to allowing type transitions. Penalty coefficients for types defined by recent compliance history in columns [4] and [8] remain statistically significant at conventional levels and fall within 1 percent (infrequent violators) and 57 percent (frequent violators) of main estimates in columns [2] and [6].[30] We do not find strong evidence that allowing transitions meaningfully changes our punchlines.

First stage beliefs. Our first stage forms the predicted penalty proxy for the full sample. The same instrument identifies behavioral responses for both frequent and infrequent violator

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[29] In principle, judicial actions from citizen suits could represent a time-varying omitted shock. In practice, CWA citizen suits against the industrial facilities in our sample are extremely rare. CWA citizen suits are most commonly levied against municipal wastewater treatment plants, not industrial facilities, and Langpap and Shimshack (2010) noted, "No other industry [other than wastewater treatment] had more than a handful of private prosecutions for water pollution violations" during a period similar to our sample period.

[30] Note that using the (larger) frequent violator enforcement sensitivity estimate in the calibration exercise mildly reduces the magnitude of the final result but does not affect the qualitative message or significance.

types. While one could imagine more sophisticated but complicated models of belief formation, this simple procedure is econometrically valid provided the first stage produces an unbiased estimator of expected fines for each type conditional on the exogenous variables. As a robustness check, we augmented the first stage regression by also interacting the type indicator with the instrument. The coefficient on the interaction term was neither economically nor statistically significant (t-statistic of 0.4).

Standard errors. To confirm robustness of inference, we replicated our analysis using a bootstrap procedure, preserving the panel structure of the data, in the spirit of Bjorklund and Jantti (1997). The standard error on the key penalty proxy coefficient for infrequent violators in *Table 1*, column [3] changes from .00014 to .00018. The standard error on the key penalty proxy coefficient for frequent violators in *Table 1*, column [7] changes from .0050 to .0057. The bootstrap approach does not generate meaningful change in inference.

Violation magnitude. A possible concern with our analysis is that frequent and infrequent violators may experience violations of different severity. Recall that we use a predefined CWA dichotomous definition of significant noncompliance (SNC). To explore the plausibility that our analysis "compares apples to apples," we matched a subsample of our facilities to a supplemental dataset containing information on numerical discharges and discharge limits for the common conventional water pollutants total suspended solids (TSS) and biochemical oxygen demand (BOD). The average TSS violation (conditional on TSS violation) was 210 percent of the permitted standard for frequent violators and 215 percent of the permitted standard for infrequent violators [N = 831 facilities]. The average BOD violation (conditional on BOD violation) was 178 percent of the permitted standard for frequent violators and 173 percent of the permitted standard for infrequent violators [N = 575 facilities]. Observed violations, conditional on significant noncompliance, are similar in severity across compliance types.

Heterogeneity. An interesting practical question is whether facility characteristics beyond compliance type may be associated with differential responsiveness to enforcement pressure. Unfortunately, our original CWA dataset contains limited information on facility characteristics. In the spirit of Evans et al. (2018), we were able to gather data on plant ownership from the EPA's Facility Registry System (FRS) and replicate our analysis for a subset of facilities with credible ownership information. *Appendix C* describes the data generating process and *Appendix Table 5* presents results. First, we note an economically and statistically insignificant coefficient on an indicator for 'plant owned by a known multiple-plant firm' in our first stage regressions. As such, observed penalties conditional on a violation for facilities owned by single-plant firms are statistically indistinguishable from observed penalties conditional on a violation for facilities owned by known multi-plant firms. Second, second-stage analyses where we restrict the sample to only plants owned by single-plant firms document somewhat larger but statistically indistinguishable relationships between violations and expected penalties (relative to our main results for both infrequent and frequent violators). As such, we find no evidence that spillovers in compliance behavior within multi-plant firms drive any of our main results. We find no evidence that facilities owned by larger multi-plant parent companies are more responsive to fines, perhaps because they wish to curry favor via regulatory dealing across compliance domains or to protect their public reputation. Results are at least weakly consistent with a conjecture that facilities owned by smaller single-plant parent companies may be more responsive to fines because they have fewer resources to contest or pay penalties.[31]

[31] We thank a helpful referee for suggesting this exercise and potential hypotheses. We note that similar second-stage analyses for facilities owned by multi-plant firms were underpowered due to small sample sizes. Coefficients for

6. Discussion and conclusion

This paper's point of departure is that sanctions are costly to regulators. Direct investigation, negotiation, and court costs are substantial. Indirect political economic costs and pressures are also important. Our primary conceptual contribution is to show that a regulator motivated to achieve high compliance given costly sanctions and limited resources must consider two effects when optimizing the treatment of violations committed by frequent vs. infrequent violators: an enforcement response effect and a sanction cost effect. Casual intuition – and the preceding literature – largely overlooks the second mechanism: it is relatively cheap to maintain a given expected threat against infrequent violators because the regulator need not back up threats with costly sanctions very often for this group. Violations by infrequent violators may simply be inexpensive to prevent.

Our main empirical contribution is a stark illustration that harsher treatment of violations by frequent violators – as is standard practice at regulatory agencies around the world – can be counterproductive if the goal is to maximize compliance. In our industrial Clean Water Act setting, we find that the ratio of marginal costs to marginal benefits (the 'buck per bang') is more than *ten* times higher for enforcement resources directed towards violations by frequent violators. This is due to a sanction cost effect, not because infrequent violators are marginally more responsive to the threat of punishment. More generally, we believe that with suitable adjustments for specific institutional contexts our basic empirical apparatus can be used in other regulatory settings by agencies considering the optimal treatment of frequent and infrequent violators. In any setting where compliance is reasonably observed through self-reporting, continuous monitoring, etc.,

facilities owned by multi-plant firms were smaller in magnitude but remained statistically similar to both main results and results for single-plant firms.

regulators have ready access to average penalties and violation rates, and deterrence effects can be estimated by in-house statisticians or outside researchers.

We note several limitations. First, to keep our work focused, this paper considers a case where the costs of observing violations are low, as in regulatory settings with self-reporting or continuous monitoring. We do not consider the costs of inspection and detection in the analysis. These costs are important in many settings, but a key point is that our main mechanism remains applicable. Previous research that accounts for costly monitoring but not costly enforcement overlooks a crucial feature of economically optimal regulatory behavior. Second, we have limited direct information on enforcement costs. We are unable to identify empirically the specific global optimum, but as noted above, we are able to identify and bound the direction of an optimum relative to existing practice. We acknowledge that more complete information on administrative, legal, and political costs of regulatory punishment is an important area for future research. Third, due to data limitations, we are unable to explore the heterogeneity of our results across detailed facility-level characteristics.

The above issues notwithstanding, notable policy implications arise from our work. CWA agencies currently punish violations by frequent violators far more severely than violations by infrequent violators; on the margin, this behavior actually lowers total compliance. Common proposals to compensate for declining overall water and air pollution enforcement budgets with an even greater emphasis on frequent violators may make matters worse.[32] Our methods apply to similar policy questions in other regulatory settings. Consider, as one illustrative example, that the

[32] Draft EPA strategic plans regularly reference "fiscal constraints" and have called for as much as 40 to 50 percent reductions in enforcement activities. Proposals include recommendations to maintain regulatory threats by focusing greater attention towards frequent violators (USEPA 2008, 2013).

Department of Commerce's Office of the Inspector General proposed that NOAA fisheries should entirely eliminate sanctions for first-time and infrequent violators (USDOC 2010).

This research is intended to be positive rather than normative. Regulators may have objectives beyond maximizing total compliance, including potentially important fairness and ethical considerations. Our results are predicated on existing institutions, practices, and the observed range of variation in the data. We are nowhere suggesting equal or higher penalties for violations by infrequent violators. Instead, this research simply serves to highlight that especially lenient treatment of infrequent violators relative to especially harsh treatment of frequent violators involves efficiency trade-offs that may sacrifice desired outcomes. Regulators are acutely aware that sanctions are costly, but the full implications of those costs for the relative treatment of frequent and infrequent violators have not yet have entered the regulatory or scholarly dialogue.

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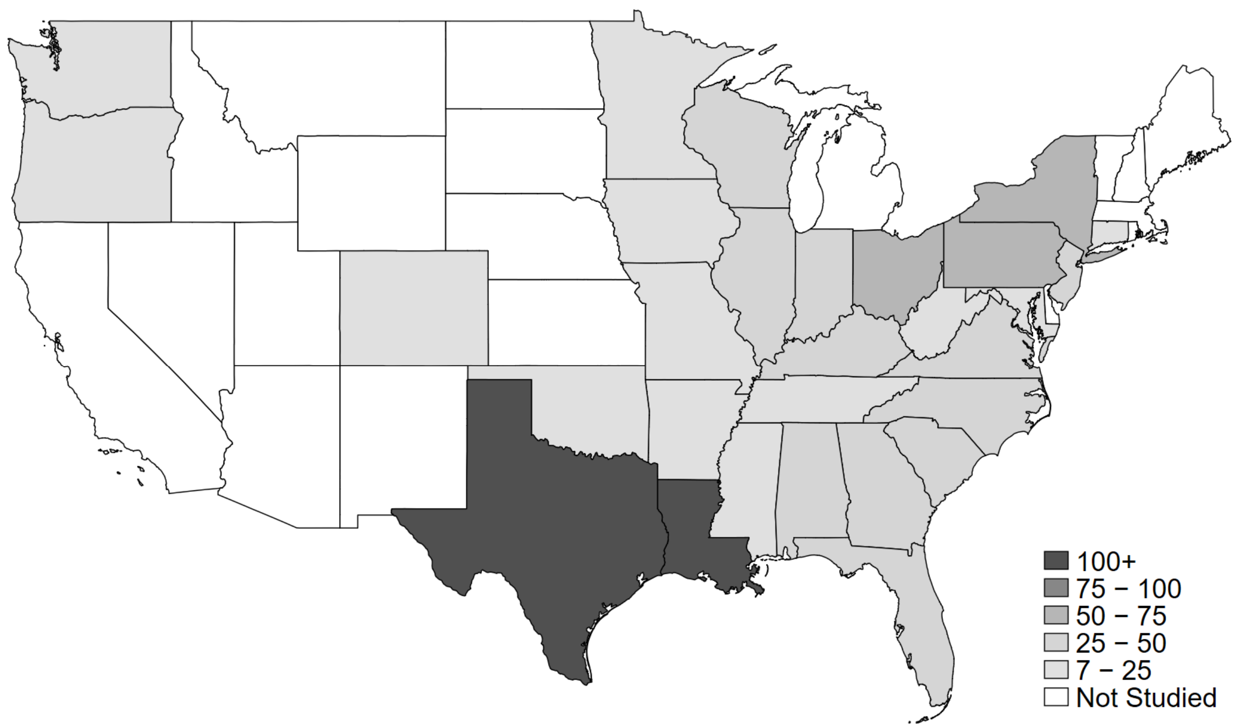
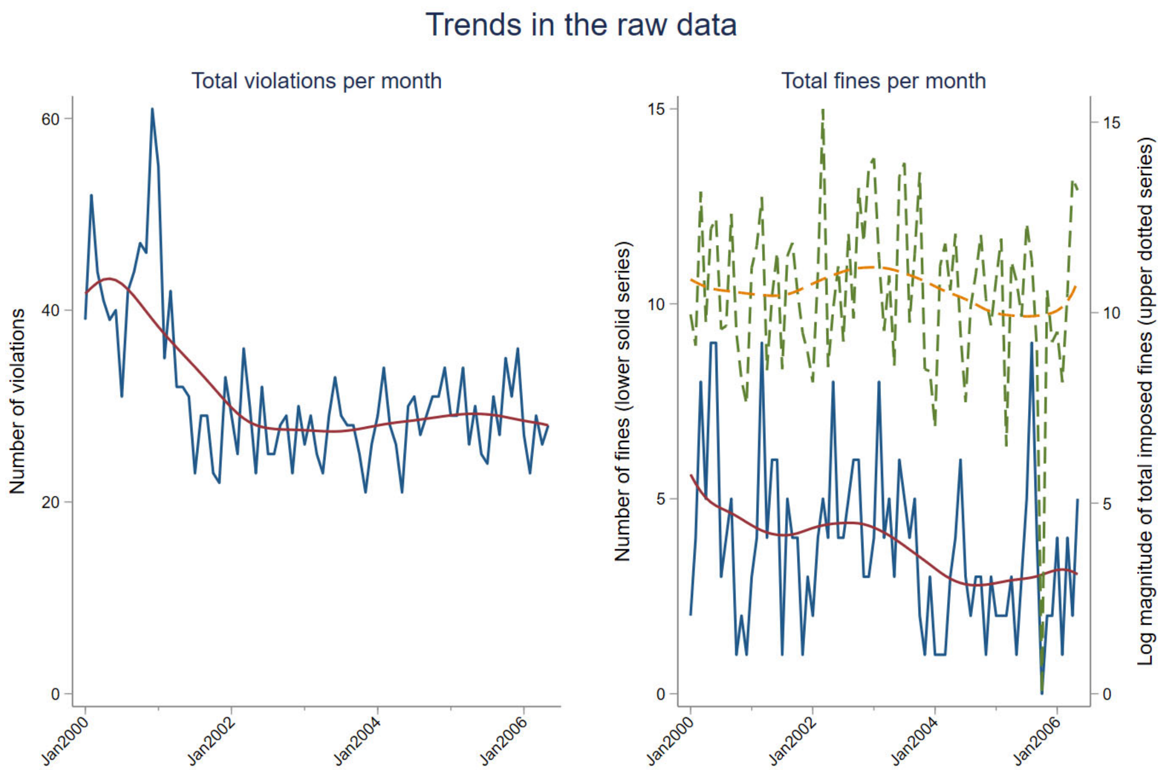


Figure 1. Number of sample facilities per state. Our 1001 sample CWA establishments are predominantly located in the eastern half of the United States and the industry-intensive states of TX, CO, WA, and OR. For inclusion, sample states must have 7 or more CWA major industrial facilities and compliance/enforcement data spanning 1999 to 2006.



Summary Statistics

	All Facilities	Infrequent Violators	Frequent Violators
Facilities	1001	786	215
States	29	29	29
Violations	2410	247	2,163
Admin. Fines	341	197	144
Fine median	\$5,000	\$2,300	\$10,000
Fine IQR	\$1,300 – \$18,800	\$1,000 – \$10,000	\$3,000 – \$33,000

Figure 2. Summary Statistics for violations and fines data. Violations, number of fines, and log-scale fine magnitudes display seasonality and generally trend downward. All series also display idiosyncratic variation. The uptick in violations during early 2001 will not be apparent in regression year dummies, suggesting these data points will be explained by features of our model. 1,001 facilities in 29 states recorded 2,410 significant noncompliance effluent violations and received 341 monetary fines with a median penalty of \$5,000. Infrequent violators have far fewer violations and fewer fines per facility over the sample period. As measured by the median and interquartile range, on average infrequent violators are subject to lower monetary penalties than frequent violators.

Figure 3. Average fines per violation across infrequent and frequent violators. The top row compares average fine per violation across types. Many violations by both types are not fined, so the bottom row presents the same comparison conditional on a positive fine. In each row, the three graphs represent different definitions of 'infrequent' vs. 'frequent' violators. Two key points emerge. First, frequent violators are punished more severely for equivalent violations. Second, modestly different definitions of frequent violator have little impact on the overall patterns observed.

TABLE 1. Main econometric estimates

	FIRST STAGE: PENALTY PREDICTION EQ.		SECOND STAGE: INFREQUENT VIOLATORS				SECOND STAGE: FREQUENT VIOLATORS			
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]
Dependent Var.	Fines	Fines	Violation	Violation	Violation	Violation	Violation	Violation	Violation	Violation
Sample Restriction	Months w/ Violations	Months w/ Violations	All Months	All Months	All Months	All Months	All Months	All Months	All Months	All Months
Specification	OLS	OLS	Linear Probability	Linear Probability	Probit	Probit	Linear Probability	Linear Probability	Probit	Probit
Fines per plant on others 1-12 months ago	2.657** (0.417)	2.634** (0.379)	n/a	n/a	n/a	n/a	n/a	n/a	n/a	n/a
Penalty Proxy	n/a	n/a	-0.00059** (0.00014)	-0.00057** (0.00014)	-0.00076** (0.0002)	-0.00073** (0.00018)	-0.0115** (0.0050)	-0.0112** (0.0046)	-0.0136** (0.0068)	-0.0133** (0.0063)
Frequent Violator	1.041** (0.417)	1.122** (0.422)	n/a	n/a	n/a	n/a	n/a	n/a	n/a	n/a
Rainfall	0.333 (0.332)	0.353 (0.297)	0.0020** (0.0008)	0.0023** (0.0008)	0.0018** (0.0005)	0.0020** (0.0005)	0.00037 (0.0153)	0.0052 (0.0151)	0.0055 (0.0164)	0.0070 (0.0163)
Unemployment	-	-0.550** (0.266)	-	-0.0006 (0.0004)	-	-0.0007 (0.0004)	-	-0.0248** (0.0113)	-	-0.0257** (0.0113)
Income	-	-0.151** (0.037)	-	-0.0001 (0.0001)	-	-0.0001 (0.0001)	-	-0.0002 (0.0002)	-	-0.0003 (0.0001)
% Vote Repub	-	-7.475 (4.701)	-	-0.0134** (0.0051)	-	-0.0140** (0.0045)	-	-0.1853 (0.1455)	-	-0.2001 (0.1520)
Year Fes	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Season FEs	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Industry FEs	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	2,410	2,410	60,522	60,522	60,522	60,522	16,555	16,555	16,555	16,555
# Facilities	328	328	786	786	786	786	215	215	215	215
BaselineViol	n/a	n/a	0.0041	0.0041	0.0041	0.0041	0.1307	0.1307	0.1307	0.1307

NOTES: Clustered standard errors in parentheses. *, ** indicate significant at 10, 5 percent levels. Probit estimates are marginal effects. All penalty variables use a logarithmic transformation of fines plus one. In first-stage regressions, the dependent variable is log fines attributed to the given violation plus one. In first-stage regressions, the key regressor is the regulator reputation effect proxy constructed by summing logged fines plus one in the past year over all other facilities in the same state, scaled by the number of facilities. In second-stage regressions, the key dependent variable is a violation by facility j in period t and the key explanatory variable is the predicted penalty proxy for facility j in period t. All results are robust to using inverse hyperbolic sine transformations.

APPENDIX A. Selected U.S. Regulatory Agency Enforcement Guidelines

U.S. Department of Commerce (USDOC). 2010. Office of the Inspector General. Final Report: Review of NOAA Fisheries Enforcement Programs and Operations. Report No. OIG-19887-2.

U.S. Department of Labor. 2013. Occupational Safety and Health Administration. Severe Violator Enforcement Program: White Paper.

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U.S. Federal Energy Regulatory Commission (USFERC). 2008. Revised Policy Statement on Enforcement. Docket No. PL08-3-000.

U.S. Health and Human Services (USHHS). 2011. Food and Drug Administration. Regulatory Procedures Manual.

U.S. Securities and Exchange Commission (USSEC). 2013. Division of Enforcement. Office of Chief Counsel. Enforcement Manual. October 9, 2013.

Appendix B: Dynamic considerations

A key assumption in this paper is that type is fixed and known, such that a one period model captures the essential economic tensions. This assumption aimed to illustrate our main point as sharply and clearly as possible, but we acknowledge its potential to influence our lessons for economics and policy.

Nevertheless, we show below that our analysis can be generalized to relax the assumption of fixed and known types and to allow the firm to strategically choose the frequency of violations in order to qualify for a different penalty size. The core insight of this type of model is that facility compliance decisions depend not only on the current penalty but also on the difference in future penalties caused by being an offender now. We also consider the empirical relevance of dynamic considerations for our empirical CWA case study below. We show empirically that our key findings are robust.

To formalize a simple model with dynamic elements, consider a regulator that uses an index h_t of violation history, which determines the expected penalty levied, $P(h_t)$, given a violation v. One natural specification is h_t as a weighted average of the previous period's violation index and current compliance:

$$(1)\ h_t = (1-\delta)h_{t-1} + \delta v_{t-1},$$

or equivalently

$$(2)\ h_t - h_{t-1} = \delta(v_{t-1} - h_{t-1}),$$

where δ determines how quickly older violations lose influence in determining current penalties.

Putting this in a continuous time context for simplicity of presentation,[1] we have the firm's objective function as maximizing the present value of profit net of expected sanctions:

$$(3)\ \int_0^T e^{-rt}\left[\pi\big(v(t)\big) - v(t)P\big(h(t)\big)\right]dt$$

through choice of violations *v(t)* as the control variable subject to evolution of the state equation as

$$(4)\ h = \delta(v - h).$$

The current value Hamiltonian is:

$$(5)\ \pi(v) - vP(h) + \mu\delta(v - h).$$

[1] The same essential points obtain in a discrete time dynamic programming setting, but with a longer derivation.

The optimality condition for the control is:

$$(6)\ \pi'(v) = P(h) - \mu\delta,$$

and the costate equation is

$$(7)\ (r+\delta)\mu - \dot{\mu} = -vP'(h).$$

At steady state, the the costate equation reduces to

$$(8)\ \mu = -vP'(h)/\,(r+\delta),$$

and the steady state optimality condition is therefore:

$$(9)\ \pi'(v) = P(h) + \frac{\delta}{(\delta+r)}\, vP'(h).$$

If $\frac{\delta}{(\delta+r)} = 0$, the final term in (9) vanishes and we are left with the one period optimality condition $\pi'(v) = P(h)$. This occurs in the limit as either $r \rightarrow \infty$ (i.e., a myopic agent) or $\delta \rightarrow 0$ (i.e., the compliance history index updates very slowly). Of course, the final term also vanishes if the penalty schedule does not depend on violation history (i.e., if $P'(h) = 0$).

Outside these cases, the final term in (9) will distort behavior away from the static optimum for the firm, in the same direction as a one-shot increase in the penalty. Intuitively, dynamically optimizing firms may take into account the difference $P'(h)$ in penalties across compliance types in addition to expected penalty $P(h)$. A natural question is: how large might the resulting distortion be? We note that $\frac{\delta}{(\delta+r)} \leq 1$ and, in our context, $v \leq 1$. Also, for $P'(h)$ to grow at least proportionally to $P(h)$, the relationship between P and h must be highly convex (at least exponential).

For these reasons, the conceptual framework suggests one might practically expect the impact of the dynamic term to be no greater than the static term in many contexts, and perhaps substantially less. Indeed, our working assumption in the initial presentation can be interpreted in this framework as the dynamic term $P'(h)$ being of negligible importance. Ultimately, however, the question is an empirical one. We investigate these empirical implications in the paper.

Appendix C: Plant ownership data

As a robustness check, we examine whether facilities owned by multi-plant firms have different compliance response patterns. Our source of information for this check is the EPA Facility Registry Service (FRS) National Organizations file. As is widely understood, these data are incomplete and quality varies (Evans et al. 2018). Most notably, the data source largely provides snapshots in time for facility information; historical data are limited or unreliable.

Nevertheless, we downloaded FRS data in June 2022 for all CWA facilities with an NPDES (National Pollution Discharge Elimination System) permit. We then matched NDPES identifiers in our compliance dataset of 1001 industrial facilities with the NDPES identifiers in the FRS. We were able to match just over 85 per cent of our sample facilities to an FRS NPDES identifier. Sources of missing matches would include permit number changes over time, or potentially facility closures after the conclusion of our sample period.

FRS parent company data offer no standardized parent names or identifiers. As such, for the 857 of 1001 facilities in our sample with matching NDPES numbers in FRS, we manually grouped parent companies based on name stems and then visual inspection of names. Some facilities had multiple parents matching (with no reliable date data), because the large industrial facilities in our dataset commonly change hands over time through direct sales and indirectly through mergers. The manual matching process requires choices. In cases of ambiguity, research assistants used Google searches to try to identify the appropriate parent company for our sample period by examining EPA and other regulatory documentation online. When multiple parent options continued to present themselves, we heuristically matched a facility to the parent with the most facilities in our dataset as the most likely.

Following this matching process, out of 1001 facilities, 144 did not match to FRS data. 488 facilities matched to parent companies owning and operating multiple facilities in our dataset and 369 were associated with parent companies owning and operating a single facility in our dataset. Using the process described above, Dupont, International Paper, Koch (including Georgia Pacific), Exxon Mobil, Westrock, and Mosaic each owned and operated more than 10 facilities in our dataset. Shell, Occidental, and Dow each owned and operated at least several facilities in our dataset.

APPENDIX TABLE 1.
Robustness to including facilities' recent violation history in first-stages regressions

	FIRST STAGE: PENALTY PREDICTION EQ.		**FIRST STAGE: PENALTY PREDICTION EQ**	
	[1]	[2]	[3]	[4]
Dependent Var.	Fines	Fines	Fines	Fines
Sample Restriction	Months w/ Violations	Months w/ Violations	Months w/ Violations	Months w/ Violations
Specification	OLS	OLS	OLS	OLS
Fines per plant on others 1-12 months ago	2.657** (0.417)	2.634** (0.379)	2.675** (0.417)	2.654** (0.379)
Fines on this plant 1-12 months ago	**n/a**	**n/a**	**0.0265 (0.048)**	**0.0296 (0.046)**
Frequent Violator	1.041** (0.417)	1.122** (0.422)	.0932** (0.374)	1.001** (0.385)
Rainfall	0.333 (0.332)	0.353 (0.297)	0.374 (0.363)	0.398 (0.333)
Unemployment	-	-0.550** (0.266)	-	-0.558** (0.270)
Income	-	-0.151** (0.037)	-	-0.152** (0.038)
% Vote Repub	-	-7.475 (4.701)	-	-7.482 (4.799)
Year Fes	YES	YES	YES	YES
Season FEs	YES	YES	YES	YES
Industry FEs	YES	YES	YES	YES
Observations	2,410	2,410	2,410	2,410
# Facilities	328	328	328	328

NOTES: Clustered standard errors in parentheses. *, ** indicate significant at 10, 5 percent levels.

APPENDIX TABLE 2. ROBUSTNESS TO DYNAMIC TERMS

	SECOND STAGE: INFREQUENT VIOLATORS *MAIN RESULTS* (without dynamic term, as reported in Table 1)		**SECOND STAGE: INFREQUENT VIOLATORS *ROBUSTNESS RESULTS* (with dynamic term)**		**SECOND STAGE: FREQUENT VIOLATORS *MAIN RESULTS* (without dynamic term, as reported in Table 1)**		**SECOND STAGE: FREQUENT VIOLATORS *ROBUSTNESS RESULTS* (with dynamic term)**	
	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]
Dependent Var.	Violation	Violation	Violation	Violation	Violation	Violation	Violation	Violation
Sample Restriction	All Months	All Months	All Months	All Months	All Months	All Months	All Months	All Months
Specification	Linear Probability	Linear Probability	Linear Probability	Linear Probability	Linear Probability	Linear Probability	Linear Probability	Linear Probability
Penalty Proxy	-0.00059** (0.00014)	-0.00057** (0.00014)	-0.00094** (0.0002)	-0.00088** (0.00026)	-0.0115** (0.0050)	-0.0112** (0.0046)	-0.0128** (0.0048)	-0.0110** (0.0044)
Proxy for P´	n/a	n/a	0.0030 (0.0020)	0.0027 (0.0019)	n/a	n/a	0.0045 (0.0502)	-0.0130 (0.0499)
Rainfall	0.0020** (0.0008)	0.0023** (0.0008)	0.0020** (0.0007)	0.0023** (0.0007)	0.00037 (0.0153)	0.0052 (0.0151)	0.0055 (0.0164)	0.0044 (0.0153)
Comm. Chars.	NO	YES	NO	YES	NO	YES	NO	YES
Year FEs	YES	YES	YES	YES	YES	YES	YES	YES
Season FEs	YES	YES	YES	YES	YES	YES	YES	YES
Industry FEs	YES	YES	YES	YES	YES	YES	YES	YES
Observations	60,522	60,522	60,522	60,522	16,555	16,555	16,555	16,555
# Facilities	786	786	786	786	215	215	215	215
BaselineViol	0.0041	0.0041	0.0041	0.0041	0.1307	0.1307	0.1307	0.1307

NOTES: Clustered standard errors in parentheses. *, ** indicate significant at 10, 5 percent level.

APPENDIX TABLE 3. Robustness to Fixed Effect Specifications

	INFREQUENT VIOLATORS			FREQUENT VIOLATORS		
	[1]	[2]	[3]	[4]	[5]	[6]
Dependent Var.	Violation	Violation	Violation	Violation	Violation	Violation
Fixed Effects	No state or facility FEs	State Fes	Facility FEs	No state or facility FEs	State FEs	Facility FEs
Penalty Proxy	-0.00059**	-0.00071**	-0.00067**	-0.0115**	-0.0065**	-0.0050
	(0.00014)	(0.00015)	(0.00015)	(0.0050)	(0.0041)	(0.0046)
Rainfall	0.0020*	0.0023**	0.0020**	0.00037	0.00088	0.0379**
	(0.0008)	(0.0007)	(0.0006)	(0.0153)	(0.0158)	(0.0115)
Year FEs	YES	YES	YES	YES	YES	YES
Season FEs	YES	YES	YES	YES	YES	YES
Industry FEs	YES	YES	NESTED	YES	YES	NESTED
State FEs	NO	YES	NESTED	NO	YES	NESTED
Facility FEs	NO	NO	YES	NO	NO	YES
Observations	60,522	60,522	60,522	16,555	16,555	16,555
# Facilities	786	786	786	215	215	215
BaselineViol	0.00408	0.00408	0.00408	0.1307	0.1307	0.1307

NOTES: Standard errors in specifications [1], [3], [4], and [6] clustered at the facility level. Standard errors in specifications [2] and [5] clustered at the state level. *, ** indicate significant at 10, 5 percent levels.

APPENDIX TABLE 4. Robustness to definitions of frequent and infrequent violators.

	INFREQUENT VIOLATORS				FREQUENT VIOLATORS			
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Dependent Var.	Violation	Violation	Violation	Violation	Violation	Violation	Violation	Violation
Frequent Violator Definition	3 or more violations	4 or more violations	5 or more violations	Violation in the past 12 months	3 or more violations	4 or more violations	5 or more violations	Violation in the past 12 months
Penalty Proxy	-0.00041**	-0.00059**	-0.00048**	-0.00058**	-0.0095*	-0.0115**	-0.0158**	-0.0180*
	(0.00015)	(0.00014)	(0.00018)	(0.00011)	(0.0047)	(0.0050)	(0.0061)	(0.0103)
Rainfall	0.0019**	0.0020*	0.0036**	0.0032**	0.00017	0.00037	0.00046	-0.0031
	(0.0008)	(0.0008)	(0.0012)	(0.0014)	(0.0128)	(0.0153)	(0.0204)	(0.0279)
Year Fes	YES	YES	YES	YES	YES	YES	YES	YES
Season Fes	YES	YES	YES	YES	YES	YES	YES	YES
Industry Fes	YES	YES	YES	YES	YES	YES	YES	YES
Observations	58,135	60,522	64,141	68,744	18,942	16,555	12,936	8,333
# Facilities	755	786	833	n/a[a]	246	215	168	n/a[a]
BaselineViol	0.00292	0.00408	0.00635	0.00532	0.1183	0.1307	0.1548	0.2453

NOTES: Clustered standard errors in parentheses. *, ** indicate significant at 10, 5 percent levels. [a] Infrequent violators and frequent violator facility numbers are not constant for the violator classification that allows switching.

APPENDIX TABLE 5.
Robustness to Ownership considerations

	FIRST STAGE: PENALTY PREDICTION EQ.		SECOND STAGE: INFREQUENT VIOLATORS		SECOND STAGE: FREQUENT VIOLATORS	
	[1]	[2]	[3]	[4]	[5]	[6]
Dependent Var.	Fines	Fines	Violation	Violation	Violation	Violation
Restriction	Original Sample	**Sample of firms w/ ownership info**	Original Analysis	**Single-Plant Firms Only**	Original Analysis	**Single-Plant Firms Only**
Fines per plant on others 1-12 months ago	2.657** (0.417)	2.885** (0.396)	n/a	n/a	n/a	n/a
Penalty Proxy	n/a	n/a	-0.00059** (0.00014)	-0.00080** (0.00023)	-0.0115** (0.0050)	-0.0177** (0.0063)
Owned by Multiple Plant Firm?	n/a	0.0022 (0.330)				
Frequent Violator	1.041** (0.417)	1.113** (0.468)				
Rainfall	0.333 (0.332)	0.279 (0.396)	0.0020** (0.0008)	0.0032** (0.0014)	0.00037 (0.0153)	0.0267 (0.0182)
Year Fes	YES	YES	YES	YES	YES	YES
Season FEs	YES	YES	YES	YES	YES	YES
Industry FEs	YES	YES	YES	YES	YES	YES
Observations	2,410	2,155	60,522	20,559	16,555	7,854
# Facilities	328	285	786	267	215	102

NOTES: Clustered standard errors in parentheses. *, ** indicate significant at 10, 5 percent levels.