



# **Coming Clean and Cleaning Up: Is Voluntary Self-Reporting a Signal of Effective Self-Policing?**

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IS VOLUNTARY SELF-REPORTING A SIGNAL OF EFFECTIVE SELF-POLICING?**

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Abstract

Administrative agencies are increasingly establishing voluntary self-reporting programs both as an investigative tool and to encourage regulated firms to police themselves. Effective self-policing is critical to contemporary regulatory designs that rely heavily on regulated entities to monitor and assure their own regulatory compliance. We investigate whether self-reporting, or the voluntary disclosure of legal violations, can reliably signal effective self-policing efforts that might warrant a reduction in regulatory scrutiny. We find voluntary disclosure to be associated with improvements in regulatory compliance and environmental performance, indicating that self-reporting is associated with effective self-policing. In addition, we find evidence that regulators subsequently reduced scrutiny of voluntary disclosers, which suggests that self-reporting can help regulators economize government enforcement resources and develop cooperative relationships with firms that are committed to self-policing.

Keywords: Self-reporting; Self-policing; Self-regulation; Disclosure; Voluntary Programs; Environmental Regulation; Environmental Performance; Pollution; Audits; Signaling

## COMING CLEAN AND CLEANING UP: IS VOLUNTARY SELF-REPORTING A SIGNAL OF EFFECTIVE SELF-POLICING?

### 1. Introduction

Administrative agencies have established over the past two decades self-reporting programs that mitigate penalties for companies that voluntarily disclose their legal violations. The U.S. Department of Defense-sponsored *Contractor Disclosure Program* reduces penalties for companies that self-report procurement fraud. The Inspector General of the U.S. Health and Human Services Department administers the *Provider Self-Disclosure Protocol*, a leniency program for voluntary disclosers of Medicare and Medicaid violations. The U.S. Department of Justice's *Leniency Program* relaxes sanctions against companies that self-report antitrust violations. The U.S. Environmental Protection Agency-sponsored *Audit Policy* encourages the voluntary disclosure of environmental violations. And the Federal Energy Regulatory Commission recently established guidelines for submitting *Self-Reports* admitting license violations.

These programs provide regulators with valuable information about legal transgressions. The Department of Justice, for instance, characterizes its *Leniency Program* as its “most effective investigative tool,” claiming that “[c]ooperation from leniency applicants has cracked more cartels than all other tools at our disposal combined” (Hammond 2005). Although such enforcement leverage is no doubt part of their appeal, most of these programs have much broader ambitions. They seek to encourage not only self-reporting, but investment in self-policing practices, or internal efforts to monitor employees' activities, that will pay dividends in improved future compliance. The Defense Department's *Contractor Disclosure Program*, for example, is explicitly designed “to encourage self-policing” (U.S. Department of Justice, 1997). The Federal

Energy Regulatory Commission likewise looks beyond the immediate violation reported, observing that “self-reports also detail the steps taken to cure the violation and to prevent any recurrence” (Federal Energy Regulatory Commission, 2007 : 18). Similarly, the stated objectives of the Environmental Protection Agency’s (EPA’s) *Audit Policy* include encouraging “corporate compliance programs that are successful in preventing violations [and] improving environmental performance” and helping to “enhance protection of human health and the environment” (U.S. Environmental Protection Agency, 1995). And the Department of Health and Human Services describes its self-reporting program as seeking to “promote a higher level of ethical and lawful conduct throughout the health care industry” (U.S. Department of Health and Human Services Health Resources and Services Administration, 1998).

Such ambitions appear lofty, especially at a time when the self-regulatory capacities of corporations have been exposed as woefully inadequate in so many arenas. In the wake of major bank failures and a broader financial meltdown, the Securities and Exchange Commission (SEC) eliminated its voluntary supervision program for investment banks, with Chairman Christopher Cox noting: “The last six months have made it abundantly clear that voluntary regulation does not work” (Labaton, 2008: A1). Similarly, EPA recently shut down *Performance Track*, its flagship voluntary program, after media reports charged that the program was nothing more than a “public relations charade” (Shiffman, Sullivan, and Avril, 2009). Academic research likewise suggests a cautious approach to self-regulation, with most studies finding no evidence that self-regulation programs improve regulatory compliance (Ebenshade, 2004; Pirrong, 2000; Pirrong, 1995; Vidovic and Khanna, 2007; Welch, Mazur, and Bretschneider, 2000), and some documenting worse performance by firms purporting to engage in self-regulation than by those not making such claims (King and Lenox, 2000; Rivera, de Leon, and Koerber, 2006).

This article investigates whether the mechanism of coupling voluntary self-reporting with a commitment to self-police can overcome some of the demonstrated limitations of other voluntary regulation approaches. Specifically, we ask whether voluntary disclosure of self-detected compliance violations can reliably signal to regulators effective self-policing efforts, which might warrant a reduction in regulatory scrutiny of the disclosers. These questions lie at the nexus of theories of self-reporting, self-policing, regulatory enforcement, and information disclosure. We extend these bodies of literature by theorizing and empirically testing the relationship between self-reporting and self-policing.

We analyze these questions in the empirical context of the U.S. EPA's *Audit Policy* self-reporting program, which offers penalty mitigation to regulated entities that voluntarily disclose legal violations discovered through systematic self-policing, and in the enforcement context of one of the most widely applicable federal environmental statutes, the U.S. Clean Air Act. Specifically, we ask whether self reporting legal violations is associated with meaningful investments in self-policing, as measured by regulatory compliance and environmental performance outcomes, and whether self-reports can help regulators economize and target regulatory enforcement resources. We find that firms that voluntarily disclosed regulatory violations and committed to self-policing improved their regulatory compliance and environmental performance. Specifically, they subsequently experienced fewer accidental releases of toxic chemicals, and were subsequently cited for fewer regulatory violations by agency inspectors, than a matched set of non-disclosers. These results suggest that on average, self-reporters to the *Audit Policy* program also engaged in effective self-policing. We further find that regulators subsequently reduced their scrutiny over self-reporting facilities, which suggests that regulators viewed these voluntary disclosures as a credible indicator of facilities'

effective self-policing activities. We demonstrate that the agency subsequently reduced both the frequency and probability of inspections of voluntary disclosers relative to inspection rates at similarly situated, non-disclosing facilities. Collectively, our results suggest that self-reporting can be a useful tool for reliably identifying and leveraging the voluntary self-policing efforts of regulated companies.

## **2. Literature Review: Self-Reporting and Self-Policing**

Despite a great deal of economic scholarship on both self-reporting and self-policing, for the most part these issues been addressed separately, and neither has been empirically linked to regulatory outcomes. Several economic models have been developed that illustrate how regulatory schemes can be designed such that voluntary self-reporting enhances enforcement efficiency. Kaplow and Shavell (1994:593), for instance, show that “given any enforcement scheme...without self-reporting, there exists a scheme with self-reporting under which behavior is the same but enforcement costs are lower.” In their model, voluntary self-reporting reduces enforcement costs because regulators need not expend resources to catch those who confess. This extension of Becker’s (1968) probabilistic enforcement model allows regulators to maintain a given level of deterrence while decreasing inspection rates. Various extensions of the Kaplow and Shavel (1994) model demonstrate that self-reporting can also enhance social welfare by eliminating the costs self-reporters might otherwise incur to evade detection of (and resulting punishments for) legal violations (Innes, 2001a, 2001b), and by ensuring that self-reported violations will be remediated (Innes, 1999).

A parallel body of scholarship focuses on the investments firms make in policing themselves. Arlen (1994) and Arlen and Kraakman (1997) generate the key insight that the

deterrence of harmful acts is a function not only of government enforcement efforts, but also of firms' internal efforts to monitor, or self-police, employees' activities. They argue that, although company managers are often better positioned than the government to prevent and detect legal violations perpetrated by their employees, the prevailing structure of corporate criminal liability dampens their incentives to do so, resulting in less-than-optimal deterrence of corporate criminal conduct (Arlen and Kraakman, 1997). This view holds that optimal deterrence depends on properly calibrating the internal and external tiers of enforcement activity. Arlen and Kraakman (1997) suggest that the magnitude and applicability of criminal sanctions should be designed to encourage corporate investments in self-policing. Pfaff and Sanchirico (2000) apply this insight in the regulatory context, noting that self-auditing can be more comprehensive and efficient than periodic regulatory inspections, and arguing that regulators should adjust fines to encourage the practice.

Although this scholarship identifies some important dynamics that underlie self-reporting and self-policing, the two practices remain largely distinct in the literature, and their connection to improving compliance or reducing harm is unclear. For example, Kaplow and Shavell's (1994) foundational model of self-reporting does not address, and its results do not depend on, the existence or effectiveness of self-policing at self-reporting firms. The efficiency gains identified in this model derive solely from the reduction in enforcement costs realized when firms self-report and thus remove themselves from the pool of firms the regulator must investigate. Pfaff and Sanchirico (2000) look beyond self-reporting to the importance of encouraging self-policing by regulated companies, but the enforcement efficiencies they identify are also achieved via regulators' enhanced detection capabilities. They argue that internal investigations associated with self-policing create paper trails that facilitate regulators'

investigations. Accordingly, like Kaplow and Shavell (1994), their models bracket the question of whether self-policing can encourage firms to proactively remedy problems or deter harmful conduct.

Unfortunately, the kinds of enforcement efficiencies theorized in these studies can be difficult to realize in practice. For instance, Short and Toffel (2008) demonstrate that facilities voluntarily self-reported violations and committed to self-policing only after regulators had invested a disproportionate amount of enforcement resources to inspect and prosecute them. For this reason, we investigate whether the internal deterrent effects of self-policing might provide an alternative avenue for economizing enforcement resources while maintaining overall deterrence levels. Arlen (1994) and Arlen and Kraakman (1997) highlight the potential deterrence gains from effective self-policing, but do not address how to identify effective self-policers in a way that would enable agencies to effectively target enforcement resources and leverage these deterrence gains in a complex regulatory scheme. A few studies have investigated how regulatory regimes can optimally leverage self-policing and self-reporting to deter non-compliance and maximize aggregate social welfare (Friesen, 2006; Innes, 2001a). These studies have not, however, considered whether or how self-reporting and self-policing might be related at the firm level. Neither, to our knowledge, have any empirical studies tested whether self-policing does, in fact, deter harmful behavior or how self-policing might be related to self-reporting.

Our study represents a novel attempt to theorize about and test the connections among self-reporting, self-policing, and regulatory compliance and performance outcomes. Understanding these relationships is critically important to the success of contemporary regulatory regimes that increasingly rely on multi-stakeholder and mixed public-private

strategies to achieve regulatory goals. Various characterized as “enforced self-regulation” (Braithwaite, 1982), “meta-regulation” (Parker, 2007) “coregulation” (Freeman, 1997; Gunningham and Sinclair, 1999), “monitored self-regulation” (Estlund, 2005; Meidinger, 2001), and the “regulation of internal self-regulation” (Parker, 2007:207), these approaches judge regulated firms based primarily on the quality of their internal compliance processes rather than solely by their external compliance outcomes (Haines, 2009). But regulators can play a meaningful role in these regulatory arrangements only if they have a basis upon which to judge the largely unobservable self-policing behaviors of regulated entities. In the next sections, we examine whether voluntary disclosures might provide a window on the effectiveness of investments in self-policing.

### **3. Empirical Context: The U.S. EPA Audit Policy**

The EPA’s “Incentives for Self-Policing: Discovery, Correction and Prevention of Violations” (*Audit Policy*) is the empirical setting for our research. On its face, the *Audit Policy* program, launched in 1995, reduces or waives certain penalties for environmental violations that are voluntarily disclosed to the government by regulated entities. We refer to these *Audit Policy* disclosures interchangeably as “voluntary disclosures” or “self-reports.” But EPA’s ambitions for the *Audit Policy* go beyond investigative leverage. Ultimately, the agency’s objective is to encourage the adoption of “corporate compliance programs that are successful in preventing violations [and] improving environmental performance” and helping to “enhance protection of human health and the environment” (U.S. Environmental Protection Agency, 1995:66710-66712).

The program is designed to achieve this by conditioning penalty mitigation on disclosers' representations of their past and future auditing practices. Like other self-reporting policies and more traditional amnesty programs, the *Audit Policy* requires prompt and voluntary disclosure and remediation of the violation.<sup>1</sup> In addition, however, the *Audit Policy* requires that voluntary disclosures arise from the “[s]ystematic discovery of the violation through an environmental audit or the implementation of a compliance management system” and requires self-reporters to make assurances that they will “prevent recurrence of the violation” (U.S. Environmental Protection Agency, 2009). In these ways, the *Audit Policy* program explicitly links self-reporting to self-policing.

Our study interrogates the assumptions that underlie this regulatory design. Specifically, we examine whether self-reporters who represent through the mechanism of the *Audit Policy* that they are self-policing improve their regulatory compliance and performance outcomes, and whether this prompts regulators to re-target their enforcement resources. This significantly extends existing research on the *Audit Policy* that has addressed the types of violations that are voluntarily reported (Pfaff and Sanchirico, 2004), the way the U.S. EPA's adoption of the Audit Policy has altered aggregate enforcement and compliance patterns (Stafford, 2004), and the factors that predict self-reporting (Short and Toffel, 2008; Stretesky and Gabriel, 2005).

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<sup>1</sup> The *Audit Policy* provides the following conditions for full penalty mitigation and an EPA recommendation of no criminal prosecution: “Systematic discovery of the violation through an environmental audit or the implementation of a compliance management system; Voluntary discovery of the violation was not detected as a result of a legally required monitoring, sampling or auditing procedure; Prompt disclosure in writing to EPA within 21 days of discovery...; Independent discovery and disclosure before EPA or another regulator would likely have identified the violation through its own investigation or based on information provided by a third-party; Correction and remediation within 60 calendar days, in most cases, from the date of discovery; Prevent recurrence of the violation; [Violations of] specific terms of an administrative or judicial order or consent agreement [are ineligible]; Cooperation by the disclosing entity is required” (U.S. Environmental Protection Agency, 2009). The *Audit Policy* does not, however, permit the agency to mitigate the “economic benefit” portion of the penalty, which recoups any financial gains the company might have accrued by violating the law.

## 4. Hypotheses

Our empirical analysis investigates two questions. First, we examine whether facilities that “come clean” by self-reporting a violation and commit to self-policing under the conditions of the *Audit Policy* actually “clean up their act” more broadly and improve their regulatory compliance and performance. Although it has been suggested that self-reporting can serve as a proxy for the existence of self-policing at a disclosing firm (Pfaff and Sanchirico, 2000: 200), to our knowledge, no prior studies have examined whether these disclosures convey information about the *effectiveness* of self-policing. We suggest here that facilities self-report regulatory violations to signal to the regulatory agency that they are effective self-policers. Second, we test whether self-reporting and the accompanying commitment to self-police under this regime prompts regulators to adjust their enforcement strategy.

### 4.1 *Assessing Voluntary Disclosers’ Commitment to Self-Police*

Companies have gone to great lengths in recent years to communicate to regulators and the broader public their commitment to police themselves, as by adopting codes of conduct, establishing internal compliance offices, or joining industry- or agency-sponsored voluntary programs (King, Lenox, and Barnett, 2002; King and Toffel, 2009). It is far from clear, however, whether any of these activities reliably signal adopters’ actual self-policing activities. Darnall and Carmin (2005) find little to distinguish one voluntary program from the next, so the mere fact of participation does little to sort companies that are truly committed to self-policing from those merely pretending to be. And, as mentioned earlier, empirical studies have found little evidence that these kinds of self-regulation programs measurably improve participants’ performance (Darnall and Sides, 2008; Lyon and Maxwell, 2007).

These findings are not surprising viewed through the lens of signaling theory. The problem with relying on companies' representations of their own self-regulatory efforts as a signal is that, in most cases, these representations impose little cost on the sender. Because self-regulatory signals like codes of conduct and participation in most government voluntary programs impose minimal costs, they do not reliably communicate good compliance behavior. As Posner (2000:19) notes, "Signals reveal type if only the good types, and not the bad types, can afford to send them, and everyone knows this."

In contrast to other representations of self-regulation, the voluntary disclosures facilities make to the *Audit Policy* are potentially more costly signals. Unlike many other common self-regulation signals, voluntary disclosure of regulatory violations is associated with two kinds of costs. First, there are monetary costs associated with voluntary disclosures that are arguably greater than the expected costs of hiding the violations. These include the cost of implementing and maintaining the systematic, internal monitoring process that is the policy's prerequisite, the cost of remediating the violation, and, often, the cost (albeit discounted) of fines imposed with certainty. As Innes (2001b: 253) notes, "in practice, self-reporting firms have been subject to rather large monetary sanctions." Not every firm will be willing to incur these costs.

Second, revealing a violation to the regulator evokes a risk of incurring greater future costs. Unlike many self-regulation symbols that merely provide a platform for self-promotion, revealing the existence of a legal violation carries the potential to damage as well as to benefit the self-reporter. A facility that is truly a poor complier, and whose minimal compliance efforts are easily detectable under regulatory scrutiny, is much less likely to disclose a violation because doing so risks attracting attention and raising the suspicions of the regulator. Moreover, because no federal audit privilege protects voluntary disclosers, self-reports and the audit materials that

support them can raise a red flag and provide a roadmap for citizen suits. In other words, voluntary disclosures structured this way are arguably “too costly to fake” (Camerer, 1988:S186).

For these reasons, we hypothesize that most companies that self-report violations to the *Audit Policy* will make meaningful investments in self-policing. To the extent that self-reporting facilities effectively implement their commitments to self-police, their environmental compliance and environmental performance should improve in a variety of ways (U.S. Environmental Protection Agency, 2001). As Orts and Murray (1997: 9) note: “First and foremost, environmental auditing informs a company of potential risks of violations and accidents. Better knowledge of these risks encourages prevention.” We hypothesize that voluntary disclosers will both commit fewer environmental regulatory violations and experience fewer environmental accidents than similarly situated, non-disclosing facilities.

#### 4.2 *Assessing the Regulator’s Response to Self-Reports*

To assess the value of these self-reports as a signal of effective self-policing, we look to the response of the signal’s recipient: the regulatory agency. In this section, we investigate how regulators interpret and respond to self-reporters’ representations of effective self-policing. We expect the regulatory agency to respond by reducing scrutiny of self-reporters. First, economic models locate the efficiency gains of self-reporting in the fact that the enforcer need not investigate companies that self-report violations (e.g., Friesen, 2006; Kaplow and Shavell, 1994). Second, many have argued that facilities are motivated to self-report violations to regulators by the prospect that voluntary disclosures will demonstrate cooperation and thus ease regulatory scrutiny (Helland, 1998; Pfaff and Sanchirico, 2000; Short and Toffel, 2008). Finally, an extensive literature on “responsive regulation” suggests that the kind of cooperative regulatory

relationships that underpin mixed public-private regulatory regimes are most likely to develop when regulators engage in a “tit-for-tat” strategy, rewarding the cooperative behavior of regulated facilities and using more punitive enforcement tools only when a regulated facility defects (Ayres and Braithwaite, 1992; Maxwell and Decker, 2006; Scholz, 1984). If, as we hypothesize, voluntary disclosure indicates effective self-policing, inspection holidays would be a key strategy for rewarding voluntary disclosers and encouraging future good behavior.

A number of voluntary regulatory programs are designed with these insights in mind. Programs like the U.S. Department of Agriculture’s *Hazard Analysis and Critical Control Point* and U.S. Occupational Safety and Health Administration’s *Voluntary Protection Program*, for instance, expressly provide that the respective agency will decrease inspection activity at participating firms (Chelius and Stark, 1984). Similarly, when the U.S. EPA launched its *Environmental Leadership Program*, designed to strengthen internal corporate environmental management practices, the agency “promise[d] not to perform routine inspections during the pilot period” (Orts and Murray, 1997:20).

In contrast, the U.S. EPA has been unwilling to commit to providing such a *quid pro quo* for *Audit Policy* disclosers. In fact, it has adopted the formal stance that “[a]uditing does not in any way serve as a substitute for compliance activities, nor does it replace regulatory agency inspections” (Johnson and Frey, 2000:4), and the agency’s Office of Enforcement Policy has noted that irrespective of self-policing efforts, “inspections play a major role in assuring quality and lending credibility to self-monitoring programs” (Wasserman, 1990).<sup>2</sup>

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<sup>2</sup> For example, the U.S. EPA noted in 1997 that “EPA’s longstanding policy is not to agree to limit its non-penalty enforcement authorities as a provision of settlement or otherwise. While EPA may consider such a facility to be a lower inspection priority than a facility that is not known to be auditing, whether and when to conduct an inspection does, and should, remain a matter of Agency discretion” (U.S. Environmental Protection Agency, 1997: vi). Also, the U.S. EPA’s Regional Council noted that “While EPA inspections of self-audited facilities will continue, to the extent that compliance performance is considered in setting inspection priorities, facilities with a good compliance history may be subject to fewer inspections” (Johnson and Frey, 2000: 5).

Although the U.S. EPA acknowledges that the *Audit Policy* can only elicit voluntary disclosures if it avoids the impression that self-reporting will attract *increased* regulatory scrutiny,<sup>3</sup> there are at least two reasons why the agency might not offer an explicit *quid pro quo*. First, informal conversations with U.S. EPA staff revealed that to the extent that voluntary disclosures raised suspicions among some regulators that self-reporting facilities might be concealing other problems,<sup>4</sup> the agency might want to maintain the flexibility to inspect particular disclosers as it sees fit. Second, providing concrete benefits to regulated firms in exchange for implementing compliance measures that are not required by law increases the risk that the program will be subjected to administrative procedural requirements and judicial oversight.<sup>5</sup> An enforcement program that is more coy about how it will target and reward regulated entities can be promulgated through more informal procedures and shielded from both public notice and comment and judicial review (Lobel, 2005; Sparrow, 2000). This approach makes it much more difficult to develop the kinds of productive cooperative relationships that come from regulating responsively (Lobel, 2005). We test whether, despite its equivocation, the U.S. EPA does, in fact, grant inspection holidays to voluntary disclosers, generating greater opportunities for enforcement efficiencies, targeting leverage, and regulatory cooperation.

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<sup>3</sup> In a conversation with one of the authors, a U.S. EPA program administrator noted that “The Agency has to avoid the perception that it is picking on companies who participate in the Audit Policy” (Personal communication, March 16, 2004).

<sup>4</sup> In a conversation with one of the authors, a former U.S. EPA attorney said that the agency tended to regard *Audit Policy* disclosures as a “red flag” that warranted increased scrutiny (Personal communication, June 10, 2004). Our conversations with U.S. EPA inspectors yielded mixed impressions: one inspector said she would be *less* suspicious of firms that self-disclosed, another inspector that he would be *more* suspicious, noting, “if a facility makes a mistake in one area, it is probably making mistakes in other areas” (Personal communication, October 12, 2007).

<sup>5</sup> The Federal Court of Appeals for the District of Columbia recently struck down OSHA’s *Cooperative Compliance Program*, which sought to induce facilities to enter into agreements to police themselves in exchange for reduced scrutiny, stating that non-cooperators would be higher priority targets for inspection. The decision was based, in part, on the fact that the agency sought to use its inspection targeting practices to induce regulated facilities to adopt compliance programs that were not required by law. The court argued that this approach altered the rights and interests of regulated parties and thus required the agency to vet the policy through onerous Administrative Procedure Act notice and comment rulemaking procedures.

## 5. Data and Empirical Methods

### 5.1 *Sample and Measures*

We gathered data on facilities located across the United States that are subject to the U.S. Clean Air Act (CAA), a statute that applies to a wide range of industries and activities that emit air pollutants beyond regulatory thresholds. Our sample period of 1991 through 2003 reflects data availability, as explained below.

**Dependent variables.** Our study employs three dependent variables. The first two are facility-level outcome measures that should be enhanced by effective self-policing: (1) the number of abnormal chemical releases to the environment, and (2) regulatory compliance records. The former is self-reported by facilities, and the latter a third-party assessment recorded by regulatory inspectors. We employ these measures as a form of triangulation. Operationalizing the focal construct in different ways can enhance the reliability of the empirical results (Campbell and Fiske, 1959; Jick, 1979). Here, using both self-reported and inspector-reported outcome measures enables us to triangulate our results.

We measured environmental performance associated with accidental events by gathering from the U.S. EPA's Toxic Release Inventory (TRI) database data on facilities' "abnormal releases" of toxic chemicals. Abnormal releases refer to toxic chemical emissions that result from circumstances outside of routine operations such as tank ruptures, vehicle accidents, or improperly maintained waste pond berms (U.S. Environmental Protection Agency, 2007: 58).<sup>6</sup>

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<sup>6</sup> Specifically, abnormal releases include all TRI chemicals "disposed or released directly into the environment or sent off-site for recycling, energy recovery, treatment, or disposal during the reporting year due to any of the following events: (1) remedial actions; (2) catastrophic events such as earthquakes, fires, or floods; or (3) abnormal events not associated with normal or routine production processes" (U.S. Environmental Protection Agency, 2007: 58). Our calls to New Jersey environmental regulators and the U.S. EPA confirmed that this definition was routinely provided to companies. Calls to several companies that had reported abnormal releases confirmed that they used this definition in deciding what to report as abnormal releases.

Interviews with state and federal regulators as well as company environmental managers suggested that self-policing can improve housekeeping and produce updated management plans, both of which can reduce the severity and frequency of environmental releases associated with abnormal events.<sup>7</sup> For example, ongoing auditing can help managers ensure that equipment is properly maintained and staff members adhere to training schedules, both of which can prevent breakdowns and accidents.<sup>8</sup> We calculated the annual *number of abnormal releases* from the subset of facilities in our sample that were required to report TRI data.<sup>9</sup>

Our second dependent variable is *clean inspection*, a dichotomous variable that refers to a facility's Clean Air Act (CAA) regulatory inspection on a particular date. This variable is coded "1" when the inspection resulted in no compliance violations (i.e., it was "clean"), and "0" when the inspector cited the facility for a violation (i.e., it was "dirty"). This distinction between whether or not inspections resulted in violations has been used in other empirical analyses of regulatory compliance (e.g., Gray and Scholz, 1993; U.S. General Accounting Office, 2001). We obtained data on CAA inspections from 1991 through 2003 from the U.S. EPA's Aerometric Information Retrieval System (AIRS)/AIRS Facility Subsystem database.

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<sup>7</sup> For example, an EPA Regional TRI coordinator told us that "Internal audits would likely set up systems that could prevent or mitigate abnormal releases. They could establish procedures that would prevent or mitigate one-time releases." A regulator from the New Jersey Department of Environmental Protection told us that internal environmental audits could reduce the frequency of abnormal releases.

<sup>8</sup> A regulator at the New Jersey Department of Environmental Protection offered this example: "Above ground storage tanks must have a berm around them that is large enough to contain the substance held in the container should it rupture. Making sure that these are maintained can prevent further discharge into the environment" (Personal communication, March 20, 2008). Similarly, an environmental manager at one of California's largest manufacturing plants told us: "I definitely believe regular audits are necessary to ensure not just regulatory compliance, but also the integrity of a facility's environmental safeguard. The purpose of the audits should be to identify potential mechanical or operating gaps in a system. Once identified, the facility can develop countermeasures or remedial actions to address any findings" (Personal communication, March 20, 2008).

<sup>9</sup> Facilities are required to report TRI data if they have at least 10 employees, operate in a targeted industry (e.g., manufacturers, utilities, mining), and produce or use any of the listed chemicals in quantities greater than particular thresholds (which range from 10 to 25,000 pounds) (U.S. Environmental Protection Agency, 2007:1 and 6).

Our third dependent variable is the annual number of CAA inspections to which each facility has been subjected. We calculated this measure based on data from the AIRS database.<sup>10</sup>

**Independent variables.** Our key explanatory variable is *voluntarily disclosed*, a dichotomous variable coded “1” for a self-reporting facility in the years after it voluntarily disclosed to the *Audit Policy*, and “0” beforehand. For non-disclosers, this variable is always coded “0”. We compiled data on voluntary disclosures associated with the U.S. EPA *Audit Policy* from the U.S. EPA Integrated Compliance Information System (ICIS) database, the U.S. EPA Audit Policy Docket, and lists of facilities that responded to various EPA Compliance Incentive Programs. The U.S. EPA provided these datasets in response to Freedom of Information Act requests.

**Control variables.** Because changes in facility size might affect the number of abnormal releases, we obtained data for two measures of facility size. First, because the number of abnormal releases might increase if production levels increase, we obtained from the TRI database data on facilities’ annual *production ratio* values, that is, the ratio of a facility’s production level in the focal year to the prior year.<sup>11</sup> Because TRI-reporting facilities report production ratios for each chemical each year, we calculated the mean value for each facility-year. We took a conservative approach to avoid undue influence from outliers, top-coding values at the 99.9% percentile for the entire sample distribution of mean production ratios. For our other measure of size, we gathered data on annual facility *employment* from Dun & Bradstreet, using the National Establishment Time-Series (NETS) Database.

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<sup>10</sup> To avoid our results being driven by outliers, we recoded annual inspection tallies beyond 14, the 99.9<sup>th</sup> percentile, to 14 when using this count as a dependent variable. We were even more conservative when using annual inspection tallies as a control variable, taking the additional precaution against large values being overly influential by recoding values beyond 8, the 99<sup>th</sup> percentile, to 8.

<sup>11</sup> As we were unable to find data on actual production levels, we employ production ratios as a proxy.

We gathered data on several factors that might affect facilities' compliance behaviors, and thus the likelihood of regulatory inspections resulting in violations. From the AIRS database, we calculated each facility's *annual number of CAA inspections and violations*. We use 1-year lagged values of these variables in our models.

Finally, we gathered data on several factors that might affect a facility's annual inspection frequency, considering both specific and general deterrence mechanisms (Cohen, 2000).<sup>12</sup> We considered two measures of specific deterrence. First, we calculated the number of *years since the facility was last inspected* for compliance with the CAA, based on data from the AIRS database. Second, we created a dummy variable coded "1" when the facility was cited with at least one *enforcement action* and "0" otherwise, based on data from the U.S. EPA's ICIS database.<sup>13</sup>

We also considered several measures of general deterrence measures. First, we considered the *National Priority Sectors* that the U.S. EPA announced every two years would be targeted as nationwide enforcement priorities. We coded this as a dummy variable based on data from the U.S. EPA website.<sup>14</sup> Second, we created a dummy variable to identify facilities *targeted by US EPA Compliance Incentive Programs* based on data obtained from the agency via Freedom of Information Act requests. These programs explicitly encourage facilities in particular EPA Regions or industries, or that conduct specific regulated activities, to reexamine their compliance status regarding a particular regulatory issue and self-report and correct any

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<sup>12</sup> According to deterrence theory, firms' compliance behavior is influenced by both specific deterrence, "the fear engendered by the prior experience of being inspected, warned or penalized themselves", and general deterrence, "hearing about legal sanctions against others" (Thornton, Gunningham, and Kagan, 2005:263).

<sup>13</sup> Fewer than 2% of facilities with any enforcement actions had more than a single one in a particular year. To avoid our results being driven by these outliers, we created a dummy variable (rather than a count variable) to measure whether a facility had been cited with any enforcement actions.

<sup>14</sup> The U.S. EPA's National Priority sectors can be found at <http://www.epa.gov/compliance/data/planning/shortterm.html>.

violations discovered. Third, we created two annual state-level variables based on AIRS data: the log of total dollar value of Clean Air Act penalties assessed by environmental regulators, and the log of the total number of CAA regulated facilities.

## 5.2 *Abnormal Environmental Releases Model*

To assess the effect on environmental performance of self-reporting a violation and committing to self-police, we estimate the following model:

$$y_{i,t} = f(\beta_1 D_{i,t} + \beta_2 \mathbf{X}_{i,t} + \beta_3 \tau_{i,t} + \beta_4 \lambda_t + \alpha_i, \varepsilon_{i,t}) \quad (1)$$

In this model, the unit of analysis is the facility-year. The dependent variable  $y_{i,t}$  refers to the annual number of abnormal releases by facility  $i$  in year  $t$ . Our key explanatory variable is voluntarily disclosed ( $D_{i,t}$ ), a dummy variable that reflects whether facility  $i$  had self-reported a violation and committed to self-policing under the *Audit Policy* by year  $t$ .

Because changes in facility size and production quantities may affect the number of abnormal releases,  $\mathbf{X}$  includes log employment and log production ratio.<sup>15</sup> We also include a full set of dummies ( $\tau_{i,t}$ ) to control for the number of years before or after the match year (matching is explained below). We also include a full set of year dummies ( $\lambda_t$ ) to control for year-specific factors, such as the emergence of new technologies, that might affect the number of abnormal releases. We include facility-level conditional fixed effects ( $\alpha_i$ ) to control for all unobserved time-invariant factors, such as industry, geographic location, EPA Region and state regulatory authorities, proximity to inspection agencies, and political power of its community, that might influence a facility's abnormal releases.

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<sup>15</sup> Our results were unchanged when we estimated models that omitted these two control variables.

### 5.3 Compliance Record Model

To evaluate the effect on regulatory compliance records of self-reporting a violation and committing to self-policing, we took the approach of other evaluations of self-regulatory programs (Gawande and Bohara, 2005; King and Lenox, 2000; Lenox, 2006). We estimate the following model using the individual inspection as our unit of analysis:

$$y_{i,d} = f(\beta_1 D_{i,d} + \beta_2 \mathbf{X}_{i,t} + \beta_3 I_{i,t} + \beta_4 \boldsymbol{\tau}_{i,t} + \boldsymbol{\alpha}_i, \varepsilon_{i,d}) \quad (2)$$

In contrast to the preceding model, the unit of analysis in this model is a facility's regulatory inspection on a particular date. The dependent variable  $y_{i,d}$  is our clean inspection dichotomous variable that refers to facility  $i$ 's regulatory inspection on date  $d$ . The coefficient on voluntarily disclosed ( $D_{i,t}$ ), coded as above, is our estimate of the change in probability that a facility's inspection was clean following its self-report and accompanying commitment to self-police under the *Audit Policy*, compared to the matched non-disclosing facilities over the same time period.

We control for several factors that can affect a facility's compliance rate. We include in  $\mathbf{X}$  the number of inspections and violations a facility experienced during each of the prior two years, inasmuch as a facility's recent regulatory experience can affect current compliance (Gray and Deily, 1996; Gray and Jones, 1991; Gunningham, Thornton, and Kagan, 2005; Helland, 1998; Magat and Viscusi, 1990; Olson, 1999; Shimshack and Ward, 2005; Weil, 1996).

Because the perceived likelihood of being inspected can affect compliance behavior (Laplante and Rilstone, 1996; Shimshack and Ward, 2005), we also control for the predicted probability of being inspected ( $I$ ) (Earnhart, 2004; Eckert, 2004; Gray and Deily, 1996; Laplante and Rilstone, 1996). We measure a facility's probability of being inspected at least once in a

given year using the predicted value from the inspection model specified below as Equation 3 with a slight variation: we estimate it using a pooled logit model.

We include facility-level conditional fixed effects ( $\alpha$ ) to control for all time-invariant factors, such as the facility’s year of construction, EPA Region and state regulatory authorities, industry, proximity to the regulatory inspector, and the political power and demographic characteristics of its local community, that might affect a facility’s violation rate (Delmas and Toffel, 2008; Gawande and Bohara, 2005; Gray and Deily, 1996; Helland, 1998; Lynch, Stretesky, and Burns, 2004; Shimshack and Ward, 2005). We also include as control variables dummies indicating the number of years before or after the match year ( $\tau$ ).

#### 5.4 *Regulatory Inspection Model*

We estimate the following model to assess the effect on regulatory inspections of self-reporting a violation and committing to self-policing:

$$y_{i,t} = f(\beta_1 D_{i,t} + \beta_2 \mathbf{X}_{i,t} + \beta_3 S_{i,t} + \beta_4 \tau_{i,t} + \beta_5 \lambda_t + \alpha_i, \varepsilon_{i,t}) \quad (3)$$

In this model, the unit of analysis is the facility-year. The dependent variable  $y$  refers to the annual number of CAA inspections to which facility  $i$  has been subjected in year  $t$ . Our key explanatory variable is voluntarily disclosed ( $D_{i,t}$ ), which is coded as described earlier.

We control for many potential determinants of inspections in  $\mathbf{X}$ . Several economic models suggest that regulators can bolster the effectiveness of limited enforcement budgets by targeting inspections based on facilities’ prior compliance records (Friesen, 2003; Harrington, 1988). In addition, the U.S. EPA notes that achieving compliance given its limited resources “is dependent on effective targeting of the most significant public health and environmental risks” (U.S. Environmental Protection Agency, 1999: 20). This means targeting enforcement resources at not only the most pressing problem areas, but also the firms most likely to be creating

problems, “taking into account...compliance/enforcement history” (U.S. Environmental Protection Agency, 1999: 20). Indeed, the U.S. EPA’s policy suggests that facilities found in violation are often targeted for more frequent inspections in the near future (U.S. Environmental Protection Agency, 1990), a relationship supported by empirical evidence (Harrington, 1988; Helland, 1998). We thus include the annual number of CAA violations for which a facility was cited and a dummy variable indicating whether the facility was subjected to an enforcement action, each lagged one and two years.

Because regulators may attempt to ensure that they return to inspect facilities before a certain time lag occurs, we create a series of dummy variables to denote the number of years since the facility was last subjected to a CAA inspection.

We control for regulatory programs that might affect inspection rates by including dummy variables that indicate whether, in a given year, a facility was targeted for heightened inspector scrutiny via a U.S. EPA Compliance Incentive Program or a U.S. EPA National Priority sector. We control for variation in enforcement strategies within states over time by including the log of total penalties environmental regulators assessed and the log of total regulated facilities in each state-year ( $S$ ).

We also include a full set of dummies ( $\tau$ ) to control for the number of years before or after the match year. We include a full set of year dummies ( $\lambda$ ) to control for year-specific factors, such as changes in presidential administrations, Congress, and U.S. EPA leadership, that might affect inspection rates. We include conditional fixed effects ( $\alpha$ ) at the facility level to control for all time-invariant factors, such as EPA Region and state regulatory authorities, year of construction, industry, proximity to the inspection agency, and affluence of the facility’s community, that might influence a facility’s inspection rate (Helland, 1998).

## 5.5 *Matched Sample*

The three models described above employ a difference-in-differences approach whereby we compare changes in the number of abnormal releases, the probability that a regulatory inspection yields no compliance violations, and the number of regulatory inspections among voluntary disclosing facilities relative to those of a matched set of control facilities. This method permits each facility to have its own baseline level for each outcome. To ensure a valid comparison, we developed a matched control group against which to estimate an average treatment effect using a difference-in-differences specification with panel data, a robust technique used in other recent program evaluations (e.g., Blundell and Costa Dias, 2002; Galiani, Gertler, and Schargrodsky, 2005; Greenaway, Gullstrand, and Kneller, 2005; Huttunen, 2007; Qian, 2007; Villalonga, 2004).

The decision to disclose compliance violations to the *Audit Policy* is voluntary, yet the identifying assumption of the difference-in-differences approach we employ is that non-participants and participants would have experienced the same regulatory stringency and pollution behavior changes over time in the absence of program enrollment, after controlling for observables. Unable to identify suitable instrumental variables, we turn to matching, the other main approach to generate unbiased estimates of a program's effect when participation is voluntary (endogenous). Our objective is to compare disclosing facilities to a matched set of non-disclosers that look "similar" to them in the years prior to voluntary disclosure. We do this based on the logic that a matched group of disclosers and non-disclosers that look "similar" before voluntary disclosure occurs would have continued to look similar in the ensuing years had voluntary disclosure not occurred. In developing a matched sample, we seek to replicate a randomized experiment that compares "treated" to "control" facilities that do not differ

systematically from each other at the time the treatment occurs (Shadish, Cook, and Campbell, 2002), or, in our case, when voluntary disclosure occurs. Relying on matched samples has been shown to significantly reduce bias in program evaluation (Blundell and Dias, 2000; Smith and Todd, 2005).

To develop our matched sample, we implement case-control matching based on several factors empirical studies have shown to be associated with facilities' decision to voluntarily disclose to the *Audit Policy* (Short and Toffel, 2008; Stafford, 2007; Stretesky and Gabriel, 2005). We consider for each voluntary discloser its industry (3-digit SIC Code) and annual inspections, violations, and enforcement actions during each of the two years before it disclosed. We include as its matched controls those non-disclosing facilities that match it exactly along all seven dimensions.<sup>16</sup> We refer to the voluntary disclosure year as the “match year” for this “matched group” of facilities. We repeat this process for all voluntary disclosers. We omit from the matched sample any voluntary discloser for which no matches were available, and all non-disclosers that went unmatched. This matching process results in an overall matched sample of 19,986 facilities including 688 that voluntarily disclosed violations. Our analysis includes each matched facility's observations starting two years before its match year through five years after the match year.

We match on the determinants of self-disclosing that have been identified in the prior literature, include in our regressions specifications several factors that affect the outcome variables, and rely on facility-level fixed-effects to control for time-invariant unobservables.

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<sup>16</sup> Specifically, the matching criteria include each of the following facility-level measures: (1) industry as measured by its 3-digit SIC Code; (2) number of CAA inspections one year ago; (3) number of CAA inspections two years ago; (4) number of CAA violations one year ago; (5) number of CAA violations two years ago; (6) number of enforcement actions one year ago; and (7) number of enforcement actions two years ago. For self-disclosing facilities, we use the year a facility self-disclosed to the *Audit Policy* as the year from which to calculate the one- and two-year lags. For non-disclosing facilities, we calculate these factors for all years.

Nonetheless, we cannot rule out the possibility that time-variant unobservables might influence our results. Because we are estimating associational (not causal) models, we do not believe that such differences result in a biased estimate of the association between disclosures and outcomes.<sup>17</sup>

## 6. Results

Descriptive statistics are provided in Table 1. All specifications include facility fixed effects. We report standard errors calculated using block bootstrap, with 500 replications.<sup>18</sup>

### 6.1 *Abnormal Environmental Releases*

We employ conditional fixed effects negative binomial regression to estimate whether facilities' annual number of abnormal releases declined after voluntarily disclosing and committing to self-policing under the *Audit Policy*. The results are presented in Table 2, along with incident rate ratios (IRR) to facilitate interpretation of the negative binomial coefficients. Although we also report results from the entire sample for comparison purposes here and with our other models, we rely on results from the matched sample as the basis of our interpretation

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<sup>17</sup> Additional unobservable differences between our matched set of disclosers and non-disclosers would be of particular concern if we were proposing a causal model theorizing that disclosures themselves improved outcomes, in which case such remaining unobserved differences between disclosers and non-disclosers would be a potential endogeneity issue. Our analysis proposes associational rather than causal models: we posit that reductions in abnormal environmental releases and compliance violations are caused not by self-disclosure, but rather by the (unobserved) internal auditing of which disclosure is an indication. It is precisely the unobservable nature of auditing (i.e., unobservable to the regulator and to us as researchers) that motivates us to examine whether self-disclosures of compliance violations to the *Audit Policy*, which include facilities attesting that they had detected the violation as part of routine and ongoing internal auditing, are associated with subsequent reductions in abnormal environmental releases and compliance violations.

<sup>18</sup> Bertrand, Duflo and Mullainathan (2004) highlight the potential for serial correlation to lead to seriously underestimated standard errors in difference-in-differences specifications. They find that calculating standard errors using block bootstrap provides a reliable solution when the number of groups is large, as it is in our context.

and inferences, noting that in most cases both samples yield similar coefficient estimates and confidence intervals (see Table 5).

The results based on the matched sample (Column 2 of Table 2) indicate that the expected annual number of abnormal releases declined by 20% (IRR=0.80;  $p<0.01$ ) after facilities voluntarily disclosed to the *Audit Policy*, compared to the matched non-disclosers over the same time period. The marginal effect of the post-voluntary-disclosure coefficient (calculated at the mean of all other variables) implies that voluntary disclosers subsequently experienced 0.35 fewer annual abnormal releases, compared to the matched sample of non-disclosers over the same period. To put this number in context, note that facilities in this sample averaged 1.7 abnormal releases in the pre-match period.

Our difference-in-differences approach relies on an identifying assumption that, if they had not committed to self-policing through the *Audit Policy*'s voluntary disclosure vehicle, the trends in outcomes (specifically, the difference in outcomes between the pre- and post-periods) among discloser and non-discloser facilities would have been indistinguishable. Although this assumption cannot be tested, it is more plausible if the two groups had indistinguishable trends during the pre-match period. We assessed whether our results might be confounded by this concern. A t-test indicated that the facilities that were about to voluntarily disclose to the *Audit Policy* and the matched non-disclosing facilities had indistinguishable trends in the number of abnormal releases during the pre-match period, providing no evidence that this potentially confounding factor was present in the matched sample with which our model was estimated.<sup>19</sup>

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<sup>19</sup> Specifically, we calculated the difference between the number of abnormal releases each facility experienced in the match (disclosure) year and the number it experienced two years prior. A t-test indicated that self-disclosers and non-disclosers had indistinguishable pre-trends ( $p=0.37$ ). We employed this “difference” metric rather than a “percent changes” metric because a large proportion of our sample had no abnormal releases in the baseline year and their “percent change” from that period is thus undefined.

## 6.2 Regulatory Compliance Records

We employ conditional fixed effects logistic regression to estimate the probability that a regulatory inspection reveals no violations (i.e., is “clean”). Recall that this model includes a generated regressor: the probability of inspection is a predicted value from a pooled logit model. Thus, the block bootstrap standard errors we report here are calculated by resampling both (1) the logit model that generates the predicted probability of inspection, and (2) the conditional fixed effects model that employs this generated regressor.<sup>20</sup> The results indicate that self-reporting a violation and concomitantly committing to self-policing is associated with improved compliance records. As indicated in Column 2 of Table 3, inspections conducted during the five years subsequent to voluntary disclosure were more than twice as likely as pre-disclosure inspections to be “clean” (OR=2.25; p=0.03), compared to the matched controls over the same time period (Table 3).<sup>21</sup> A t-test confirmed that our results were not confounded by voluntary disclosers having faster improvement trends during the pre-disclosure period than the matched non-disclosers.<sup>22</sup>

Although including *predicted probability of inspection* subjects this model to the possibility of introducing autocorrelation, our results and some additional analysis lead us to believe this is unlikely to be driving our results. First, we note that Table 3 (Column 2) reports the coefficient on predicted probability of inspection to be 0.20 (odds ratio=1.22), a magnitude

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<sup>20</sup> For this model, the bootstrapping procedure was implemented as follows. All observations pertaining to a randomly drawn subsample of facilities are used to estimate the pooled logit regression that predicts the probability of an inspection occurring that year. Predicted values generated for this subsample are used as a generated regressor in the conditional fixed effects logit regression that estimates clean inspections. This procedure is repeated 500 times.

<sup>21</sup> Clustering standard errors by facility yielded results nearly identical to those obtained via block bootstrapping.

<sup>22</sup> We compared self-disclosers’ and non-disclosers’ trends of abnormal releases during the two years prior to the match year. For each facility, we calculated the difference between the proportion of inspections that were “clean” in the match (disclosure) year and two years prior. A t-test provided no evidence that the eventual self-disclosers were improving compliance faster than the matched non-disclosers (p=0.96).

close to zero and not statistically significant (SE=0.57). Also, a one standard deviation change in *predicted probability of inspection* is associated with a mere 1.03 change in the odds ratio,<sup>23</sup> a figure close to a null effect of 1.0. Second, we calculated autocorrelation to equal -0.03, a value close to zero. This low value provides no evidence to suspect that autocorrelation is significantly influencing our results. Third, re-estimating this model omitting *predicted probability of inspection* yielded a coefficient on our focal variable *post voluntary disclosure* that was similar in magnitude and significance to our primary model. Fourth, we re-estimated the model using Arellano-Bond GMM dynamic panel data estimation, which is robust to autocorrelation, to estimate both our primary specification (including *predicted probability of inspection*) and the latter alternative model that omitted *predicted probability of inspection*. These models continued to yield positive, statistically significant coefficients on *post voluntary disclosure*. These additional analyses lead us to believe our results to be robust to the potential for autocorrelation.

Another potential concern with our model is that our results might be contaminated by a regression to the mean. Given our matching technique, our treatment and control facilities share the same underlying compliance violation rate in the years prior to the match (disclosure). If our treatment facilities experienced a random negative shock of experiencing an additional violation in a given year, and this led them to disclose the violation to the *Audit Policy*, then their “improved compliance” in subsequent periods could be interpreted as merely regressing back to the mean. To detect whether mean reversion was driving our results, we estimated an alternative model that estimated annual treatment effects.<sup>24</sup> If our main results were driven by mean

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<sup>23</sup> This change in the odds ratio is calculated as  $\exp(\beta * SD) = \exp(0.20 * 0.16) = 1.03$ , using predicted *probability of inspections* standard deviation of 0.16 per Panel B of Table 1.

<sup>24</sup> Specifically, we replaced the single *post voluntary disclosure* variable (which estimated the average treatment effect) with a series of dummy variables indicating *1st year after voluntary disclosure*, *2nd year after voluntary disclosure*, and so on.

reversion, we would expect only the first (or perhaps first two) of these annual treatment effects to be positive and large. In fact, the results of this alternative model indicate that disclosers continued to show significantly increased likelihood of clean inspections during later years as well. Although performance was significantly better in each post-disclosure year than in the pre-disclosure years, we found no significant difference in improvement between the first post-disclosure year and any other subsequent year. This suggests that mean reversion is unlikely to be driving our results.

### 6.3 *Regulatory Scrutiny*

We employed conditional fixed effects logistic regression to analyze whether the probability of a facility experiencing *any* inspections in a given year declined after voluntarily disclosing a violation and committing to self-policing. We also employed conditional fixed effects negative binomial regression to estimate whether these facilities' annual number of inspections declined. The results for both models are consistent and indicate that regulators granted inspection holidays to facilities that voluntarily disclosed and committed to self-policing (Table 4). The results of the conditional fixed effects logistic model reported in Column 2 of Table 4 indicate that facilities that self-reported subsequently experienced a 25% decline in the probability of facing any inspections compared to the matched controls (OR=0.74;  $p<0.01$ ).

The negative binomial results reported in Column 4 of Table 4 indicate that after facilities voluntarily disclosed, the annual number of inspections declined by 17% (IRR=0.83;  $p<0.01$ ), holding all other variables constant. A t-test confirmed that our results were not confounded by

pre-existing differences in trends between facilities that were about to disclose and matched non-disclosing facilities.<sup>25</sup>

## 7. Conclusion

Our results provide evidence that self-reporting can reliably signal effectively implemented self-policing. Specifically, we demonstrate that facilities that voluntarily disclosed a violation and committed to self-police under the *Audit Policy* improved their environmental performance by (1) reducing their accidental releases of toxic chemicals to the environment, and (2) improving their environmental compliance records, being cited by regulators for fewer violations than similarly situated non-disclosers. We also find that regulators rewarded voluntary disclosers with an “inspection holiday,” suggesting that integrating self-reporting into regulatory design can help regulators economize government enforcement resources and develop cooperative relationships with committed self-policers. We attribute these results to the design of the *Audit Policy*, which explicitly links self-reporting to self-policing.

Our findings contribute in four important ways to a literature that some have criticized for being “noncommittal on the question of whether voluntary disclosure policies are worthwhile complements to conventional enforcement strategies” (Murphy and Stranlund, 2008: 261). First, we demonstrate that facilities that committed to self-police as a part of their voluntary disclosures to the *Audit Policy* did, in fact, internally deter harmful behavior by their employees. These findings support Arlen’s (1994) and Arlen and Kraakman’s (1997) argument that internal self-policing can contribute to overall deterrence in a given enforcement regime, and confirm

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<sup>25</sup> We compared self-disclosers’ and non-disclosers’ inspection trends during the two years prior to the match year. We calculated the difference between the number of inspections each facility experienced in the match (disclosure) year and the number it experienced two years prior. A t-test indicated that two groups had indistinguishable pre-trends ( $p=0.96$ ).

that self-policing can provide enforcement benefits above and beyond those achieved through mere self-reporting.

Second, we demonstrate that, when they are conditioned on a commitment to self-police, self-reports of legal violations can provide a means for regulators to identify facilities that, on average, are deterring harmful behavior on the part of their employees. In a mixed regulatory enforcement scheme, regulators must have a means of recognizing effective self-policers in order to target their resources at, and leverage the deterrent effects of self-policing on, a subset of facilities. Although valid concerns have been raised that companies might self-report violations strategically to game the system (Pfaff and Sanchirico, 2000), our results imply that this concern may be exaggerated, at least within a disclosure scheme designed to impose costly risks on gamers.

Third, and more broadly, our results suggest the importance of addressing signaling issues more explicitly in regulatory design. To date, the literature on self-policing has focused almost exclusively on incentives, discussing how regulators should calibrate rewards and penalties to induce firms to police themselves (e.g., Arlen and Kraakman, 1997; Coglianesi and Nash, 2001; Maxwell and Decker, 2006; Pfaff and Sanchirico, 2000). Agencies also have focused on incentives in designing voluntary programs, going to great lengths to reduce the costs<sup>26</sup> and emphasize the benefits<sup>27</sup> of self-policing. Unfortunately, in their efforts to

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<sup>26</sup> For example, the U.S. EPA promotes its *WasteWise* program by touting the program as being “free, voluntary, flexible,” and makes clear that “The amount of time and money you invest is up to you! You are free to set goals that are the most feasible and cost-effective for your organization,” which includes the possibility of zero investment beyond completing the brief online registration form (U.S. Environmental Protection Agency, EPA WasteWise Program Overview, <http://www.epa.gov/wastewise/about/overview.htm> (updated December 18, 2007; accessed December 31, 2007)).

<sup>27</sup> The U.S. EPA, for instance, provides participants in its *Performance Track* program with “green marketing support” irrespective of the results they ultimately achieve. This includes, according to a Congressional Committee: “motivational posters; camera-ready advertisement “slicks”; press release templates; draft congratulatory letters to be signed by State Governors and other public officials; “tips” for communicating with employees, the public, and the media about *Performance Track*; video and Powerpoint presentations; vehicle signs; flags; and event/conference planning” (Wynn and Stupak Letter, April 13, 2007).

incentivize participation, regulators often strip self-regulatory commitments of any signaling value they might have. Regulators can enhance overall deterrence if they can accurately identify which facilities are behaving cooperatively and effectively policing themselves, and target enforcement resources accordingly. Legal scholarship has begun to explore ways to develop more finely grained sorting and targeting systems along these lines. For instance, Raskolnikov (2009) has proposed a tax compliance system that would identify cooperative and normatively motivated taxpayers by allowing them to opt into an enforcement regime with terms that would be too costly for gamblers to accept. Our findings suggest that self-reporting coupled with a commitment to self-police can serve as a valuable tool for sorting regulated firms by their compliance capacities and motivations.

Finally, we demonstrate that regulators do, in fact, use self-reporting as a heuristic for targeting enforcement resources. Consistent with studies that find reduced regulatory scrutiny for firms that improve toxic pollution levels (Decker, 2005) or self-report Resource Conservation and Recovery Act violations (Stafford, 2007), we show that the U.S. EPA grants an “inspection holiday” to voluntary disclosers. We also demonstrate that these inspection holidays do not dampen the effectiveness of self-policing. By rewarding the firms that improve, while monitoring more intensively those that do not, regulators are engaging in a form of “responsive regulation” (Ayres and Braithwaite, 1992) that has the potential to achieve enforcement efficiencies and develop the self-regulatory capacities of committed self-policers.

Our findings raise a number of interesting questions for future research. Although it suggests the possibility of enforcement efficiencies, our study does not account for the costs of self-policing. A number of studies have raised concerns about the high costs and minimal benefits of internal compliance programs (Krawiec, 2003; Langevoort, 2002). We demonstrate

here that self-policing can have real deterrence benefits, but future research is necessary to determine whether they are worth the cost, or whether similar deterrence levels could be achieved at lower cost through government enforcement. Future research could seek to overcome the data limitations that prevented us from differentiating the severity of compliance violations that were deterred and calculating the attendant avoided social costs.

Future studies should also investigate whether our findings hold up in different regulatory and organizational contexts. It may be that voluntary disclosure has a stronger signaling value in some settings than in others. It would also be valuable to determine whether the signaling value of self-reporting is contingent on a program design that ties it explicitly to a commitment to self-police. Finally, future research could explore other ancillary benefits of voluntary disclosure. For example, prior research has found that voluntarily disclosing environmental liabilities can bolster the credibility of other information such firms release, which reduces their cost of capital and attenuates negative shocks to stock prices when they release bad news (Blacconiere and Patten, 1994; Cormier and Magnan, 2007). Researchers could investigate whether such benefits also accrue to firms that voluntarily disclose regulatory compliance violations.

With regulators continuing to explore alternative approaches to increase compliance at lower cost, further empirical research is needed to examine the efficacy and efficiency of mixed regulatory schemes that combine self-regulation with government enforcement. Regulators eager to engage regulated entities in self-regulation must balance competing needs to design programs that will attract participants, withstand legal and procedural challenges, and effectively bolster compliance. Our results suggest that combining self-reporting with a commitment to self-police can be a valuable tool for helping to achieve this balance.

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**TABLE 1**  
**SUMMARY STATISTICS**

**PANEL A. Sample for abnormal environmental releases analysis (see Table 2)**

	Entire sample N=105,092 facility-years				Matched sample N=30,919 facility-years			
	Mean	SD	Min	Max	Mean	SD	Min	Max
Number of abnormal releases <sup>b</sup>	1.70	3.17	0	17	2.22	3.53	0	17
Post voluntary disclosure (dummy)	0.02	0.13	0	1	0.04	0.19	0	1
Log production ratio <sup>a</sup>	0.71	0.22	0	1.79	0.69	0.22	0	1.79
Log employment	3.78	2.46	0	10.11	3.67	2.34	0	9.90

**PANEL B. Sample for compliance analysis (see Table 3)**

	Entire sample N= 68,411 inspections				Matched sample N=4,174 inspections			
	Mean	SD	Min	Max	Mean	SD	Min	Max
Inspection is “clean” (no violations) (dummy)	0.85	0.35	0	1	0.79	0.41	0	1
Post voluntary disclosure (dummy)	0.02	0.14	0	1	0.08	0.27	0	1
Probability of an inspection (predicted)	0.67	0.13	0.21	0.93	0.64	0.16	0.13	0.90
Number of inspections 1 year ago <sup>a</sup>	2.24	2.42	0	8	1.40	1.84	0	8
Number of violations 1 year ago <sup>a</sup>	0.25	0.60	0	3	0.23	0.57	0	3

**PANEL C. Sample for inspection analysis (see Table 4)**

	Entire sample N=367,776 facility-years				Matched sample N=94,270 facility-years			
	Mean	SD	Min	Max	Mean	SD	Min	Max
Annual number of CAA inspections <sup>b</sup>	0.76	1.01	0	14	0.64	0.79	0	14
Post voluntary disclosure (dummy)	0.01	0.08	0	1	0.02	0.14	0	1
Years since prior inspection <sup>c</sup>	1.94	1.18	1	4	2.08	1.24	1	4
Number of violations 1 year ago <sup>a</sup>	0.03	0.18	0	3	0.02	0.15	0	1
Any enforcement actions 1 year ago (dummy)	0.01	0.10	0	1	0.01	0.11	0	1
Compliance Incentive Program target (dummy)	0.03	0.16	0	1	0.05	0.22	0	1
National Priority sector (dummy)	0.12	0.32	0	1	0.16	0.37	0	1
Log total penalties in the state-year	11.86	4.47	0	17.56	12.44	4.11	0	17.56
Log number of regulated facilities in the state-year	7.25	0.73	1.61	8.29	7.27	0.71	1.61	8.29

In all panels, observations span 1993-2003. Within the matched sample, observations extend from 2 years prior to 5 years after each facility’s match year.

<sup>a</sup> top coded at 99th percentile

<sup>b</sup> top coded at 99.9th percentile

<sup>c</sup> top coded at 4 per year

**TABLE 2**  
**Self-policing is associated with fewer abnormal environmental releases**

Conditional Fixed Effects Negative Binomial Model

*Dependent Variable: Number of abnormal releases*

	(1) Entire sample		(2) Matched sample	
	Coefficients	Incident Rate Ratio	Coefficients	Incident Rate Ratio
Post voluntary disclosure	-0.169** [0.051]	0.84	-0.223** [0.063]	0.80
Log production ratio	0.039+ [0.022]	1.04	0.011 [0.036]	1.01
Log employment	-0.032** [0.008]	0.97	0.003 [0.014]	1.00
Conditional fixed effects at the facility-level	Included		Included	
Year fixed effects (1994-2003)	Included		Included	
Fixed effects for <i>t</i> years before/after match year			Included	
Observations	105,092		30,919	
Facilities	13,082		5,582	
Model Wald Chi-squared	12945.0**		4765.7**	

Block bootstrap standard errors in brackets (500 replications); +  $p < 0.10$ ; \*  $p < 0.05$ ; \*\*  $p < 0.01$ . The model also includes a dummy variable denoting when a missing employment value was recoded to zero. Unit of analysis is the facility-year. The sample includes matched facilities' observations starting 2 years prior to their match year through 5 years after the match year, and includes only facilities that reported data to the EPA's Toxic Release Inventory. The conditional fixed effects negative binomial model drops facilities that have identical annual abnormal release rates throughout the sample period.

**TABLE 3**  
**Self-policing is associated with fewer compliance violations**

Conditional Fixed Effects Logistic Regression Model

*Dependent Variable: Clean Inspection (dummy)*

	(1)		(2)	
	Entire sample Coefficients	Odds Ratio	Matched sample Coefficients	Odds Ratio
Post voluntary disclosure	0.431*	1.54	0.813*	2.25
	[0.188]		[0.380]	
Probability of an inspection (predicted)	0.035	1.04	0.202	1.22
	[0.120]		[0.573]	
Number of inspections 1 year ago	0.005	1.01	-0.122†	0.88
	[0.016]		[0.075]	
Number of inspections 2 years ago	-0.021	0.98	-0.153	0.86
	[0.015]		[0.082]	
Number of violations 1 year ago	0.295**	1.34	1.195**	3.30
	[0.036]		[0.235]	
Number of violations 2 years ago	0.378**	1.46	1.247**	3.48
	[0.031]		[0.234]	
Year fixed effects (1994-2003)	Included			
Fixed effects for <i>t</i> years before/after match year			Included	
Observations (Inspections)	68,411		4,174	
Facilities	6,752		713	

Block bootstrap standard errors in brackets (500 replications); †  $p < 0.10$ ; \*  $p < 0.05$ ; \*\*  $p < 0.01$ . Unit of analysis is a facility's inspection. The dependent variable is coded "1" when an inspection resulted in no cited violations and "0" when at least one violation was cited. The model includes conditional fixed effects at the facility-level and includes facilities' observations spanning 2 years prior to the match year through 5 years after the match year. Conditional fixed effects logistic regression models drop facilities with no variation in the dependent variable throughout the sample period, including those facilities in our sample that maintained uniform (perfect) compliance records during our sample period.

**TABLE 4**  
**Self-policing is associated with fewer inspections**  
**and lower probability of being inspected**

	Any annual inspections Conditional Fixed Effects Logistic				Number of annual inspections Conditional Fixed Effects Negative Binomial			
	(1)		(2)		(3)		(4)	
	Entire sample Coefficients	Odds Ratio	Matched sample Coefficients	Odds Ratio	Entire sample Coefficients	Incident Rate Ratios	Matched sample Coefficients	Incident Rate Ratios
Post voluntary disclosure	-0.256** [0.082]	0.77	-0.303** [0.114]	0.74	-0.053 [0.037]	0.95	-0.185** [0.052]	0.83
2 years since last inspection	0.170** [0.011]	1.19	0.372** [0.023]	1.45	0.032** [0.005]	1.03	0.126** [0.011]	1.13
3 years since last inspection	0.282** [0.016]	1.33	0.537** [0.031]	1.71	0.064** [0.009]	1.07	0.231** [0.018]	1.26
4 or more years since last inspection	0.654** [0.015]	1.92	1.177** [0.032]	3.25	0.256** [0.011]	1.29	0.618** [0.018]	1.86
Number of violations 1 year ago	0.225** [0.028]	1.25	0.157* [0.071]	1.17	0.058** [0.010]	1.06	0.026 [0.027]	1.03
Number of violations 2 years ago	0.071** [0.027]	1.07	0.035 [0.067]	1.04	0.005 [0.011]	1.01	-0.010 [0.027]	0.99
Any enforcement actions 1 year ago	-0.079+ [0.046]	0.92	-0.142 [0.087]	0.87	-0.004 [0.020]	1.00	0.002 [0.035]	1.00
Any enforcement actions 2 years ago	-0.150** [0.046]	0.86	-0.185+ [0.099]	0.83	-0.016 [0.022]	0.98	-0.024 [0.046]	0.98
Compliance Incentive Program target	0.045 [0.027]	1.05	0.049 [0.045]	1.05	0.022+ [0.013]	1.02	0.049* [0.020]	1.05
National Priority sector	0.014 [0.018]	1.01	0.282** [0.034]	1.33	0.031** [0.009]	1.03	0.102** [0.020]	1.11
Log total CAA penalties in the state-year	0.021** [0.003]	1.02	0.013 [0.008]	1.01	0.002 [0.002]	1.002	-0.012** [0.004]	0.99
Log number of CAA-regulated facilities in the state-year	1.675** [0.044]	5.34	1.390** [0.102]	4.02	0.791** [0.021]	2.21	0.623** [0.042]	1.87
Facility-level conditional fixed effects	Included		Included		Included		Included	
Fixed effects for <i>t</i> years before/after match year			Included				Included	
Year fixed effects (1994-2003)	Included				Included		Included	
Observations	328,032		82,287		367,776		94,270	
Facilities	42,270		13,673		48,972		16,078	
Wald chi-squared	6560.4**		2637.9**		4616.5**		4733.2**	

Block bootstrap standard errors in brackets (500 replications); +  $p < 0.10$ ; \*  $p < 0.05$ ; \*\*  $p < 0.01$ . Unit of analysis is the facility-year. The sample includes matched facilities' observations starting 2 years prior to their match year through 5 years after the match year. The conditional fixed effects negative binomial model drops facilities that have identical annual inspection rates throughout the sample period. The conditional fixed effects logistic model drops facilities for which annual inspection rates are either always positive or always zero throughout the sample period.

**Table 5. Comparing results: Entire sample to Matched sample**

*Post voluntary disclosure* coefficients, with 95% confidence intervals in brackets

Dependent variable:	Number of abnormal releases (Table 3)	Clean Inspection (Table 4)	Any annual inspections (Table 5)	Number of annual inspections (Table 5)
Matched sample	-0.22 [-0.35, -0.10]	0.81 [0.07, 1.56]	-0.30 [-0.53, -0.08]	-0.18 [-0.29, -0.08]
Entire sample	-0.17 [-0.27, 0.07]	0.43 [0.06, 0.80]	-0.26 [-0.42, -0.10]	-0.05 [-0.13, 0.02]