



Coming Clean and Cleaning Up: Does Voluntary Self-Reporting Indicate Effective Self-Policing?

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Coming Clean and Cleaning Up: Does Voluntary Self-Reporting Indicate Effective Self-Policing?*

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Administrative agencies are increasingly establishing voluntary self-reporting programs both as an investigative tool and to encourage regulated firms to commit to policing themselves. We investigate whether self-reporting can reliably indicate effective self-policing efforts that might provide opportunities for enforcement efficiencies. We find that regulators used self-reports of legal violations as a heuristic for identifying firms that are effectively policing their own operations, shifting enforcement resources away from voluntary disclosers. We also find that firms that voluntarily disclosed regulatory violations and committed to self-policing improved their regulatory compliance and environmental performance, suggesting that the enforcement relief they received was warranted. 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.

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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's (EPA) *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 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: 66706, 66712). 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: 58399).

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, the 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: A1). 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 indicate 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 EPA's *Audit Policy*, a 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 the U.S. Clean Air Act, one of the most widely applicable federal environmental statutes. First, we ask whether regulators actually use self-reports of legal violations as a heuristic for identifying firms that are effectively policing their own operations, shifting enforcement resources away from voluntary disclosers. We then investigate whether such enforcement relief would be warranted, based on self-reporters' subsequent regulatory compliance and environmental performance. We find that regulators reduced their scrutiny over self-reporting facilities, which suggests that regulators used these voluntary disclosures as an indicator that facilities were engaging in self-policing activities. The agency subsequently reduced both the frequency and probability of inspections of voluntary disclosers relative to inspection rates at similarly situated, non-disclosing facilities. We further demonstrate that firms that voluntarily disclosed regulatory violations and committed to self-policing improved their regulatory compliance and environmental performance. Specifically, they were subsequently cited for fewer

regulatory violations by agency inspectors and subsequently experienced fewer accidental releases of toxic chemicals than a matched set of non-disclosers. These results suggest that, on average, self-reporters to the *Audit Policy* also engaged in effective self-policing. 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, these issues generally have 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 Shavell (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; Innes 2001b) and by ensuring that self-reported violations will be remediated (Innes 1999).

A parallel body of scholarship focuses on how firm-level self-policing can contribute to overall deterrence levels. 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 their employees' activities, or to "self-police." 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 avoid the question of whether self-policing can encourage firms to proactively remedy problems or deter harmful conduct.

Unfortunately, the kinds of enforcement efficiencies theorized for self-reporting can be difficult to realize in practice. 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, potentially cannibalizing any *ex post* gains that might be realized. Moreover, even *ex post* gains can be elusive in a complex regulatory state, where legal violations are often chronic and result from either technological or compliance management deficiencies. Merely reporting such lapses does not necessarily mean the problem has been remedied, so regulators cannot automatically remove self-reporters from the pool of firms to be monitored, as the police can do, for instance, when a suspect confesses to murder in the criminal context. For these reasons, it is important to investigate the internal deterrent effects of self-policing as an alternative mechanism for economizing enforcement resources while maintaining overall deterrence levels in the regulatory context.

Leveraging deterrence gains from firm-level self-policing efforts has been challenging both because there is wide variation in firms’ motivations and capacities to police themselves and because regulators cannot readily observe self-policing behavior to assess it for themselves. While some research on voluntary regulation has suggested that firms can reduce the harmful effects of their activities by adopting internal compliance management practices (Innes and Sam 2008; Khanna and Damon 1999; Sam, Khanna and Innes 2009), other studies suggest that, on average, purportedly self-regulating firms do not measurably improve their performance

(Alberini and Segerson 2002; Darnall and Sides 2008; Ebenshade 2004; Koehler 2007; Lyon and Maxwell 2007; Pirrong 2000; Pirrong 1995; Rivera, de Leon and Koerber 2006; Vidovic and Khanna 2007; Welch, Mazur and Bretschneider 2000) and sometimes actually perform worse than their non-self-regulating counterparts (King and Lenox 2000; Rivera, de Leon and Koerber 2006). Because firms' internal self-policing practices are not readily observable, regulators may not be able to distinguish effective from ineffective self-policers. This has made it exceedingly difficult for regulators to leverage the effective self-policing achieved by some firms toward broader enforcement efficiencies.

In this article, we theorize and test whether and how self-reporting might serve as an indication of effective self-policing—as evidenced by improved regulatory compliance and performance outcomes—that would create possibilities either for enhanced deterrence or the economization of enforcement resources. To our knowledge, this is a novel undertaking. The phenomenon of self-reporting is virtually absent from the literature on voluntary regulation. 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 target enforcement resources effectively and leverage these deterrence gains in a complex regulatory scheme. Pfaff and Sanchirico (2000) suggest that self-reporting can serve as a proxy for self-policing, but they neither theorize nor test whether this self-reporting conveys information about the *effectiveness* of self-policing. 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 within particular organizations. 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.

3. Empirical Context: The 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 the 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.¹ In addition, however, the *Audit Policy* requires that voluntary disclosures arise from the "[s]ystematic discovery of the violation through an environmental

¹ 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.

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.²

Our study investigates the assumptions that underlie this regulatory design. Specifically, we examine (a) whether claims of self-policing made in connection with *Audit Policy* disclosures prompt regulators to re-target their enforcement resources; and (b) whether self-reporters who claim to be self-policing actually improve their regulatory compliance and performance outcomes. 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 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).

4. Hypotheses

Our empirical analysis investigates two questions. First, we examine whether regulators use voluntary disclosures as a heuristic for targeting their enforcement resources. We suggest that regulators will shift inspection resources away from self-reporting facilities in the belief that these facilities are effectively policing themselves. Second, we test whether there is any warrant for doing so. Specifically, we investigate whether facilities that “come clean” by self-reporting a

² By 1999, the EPA had received Audit Policy disclosures of compliance violations pertaining to nearly 2000 facilities, and while EPA was “encouraged by the growing trend in corporate-wide disclosures under our Audit Policy” (U.S. Environmental Protection Agency 1999a: 2), it is important to consider this figure in the context of traditional enforcement mechanisms. For example, in 1999, EPA and state environmental regulators conducted more than 500,000 compliance inspections, issued more than 40,000 notices of violations, and filed nearly 12,000 formal administrative actions (Brown and Green 2001; U.S. Environmental Protection Agency 2002).

violation and commit to self-policing under the conditions of the *Audit Policy* actually “clean up their act” more broadly and improve their overall regulatory compliance and performance. We suggest here that, under certain conditions, voluntary disclosures can reliably indicate effective self-policing practices.

4.1 *Assessing the Regulator’s Response to Self-Reports*

In this section, we investigate how regulators respond to self-reports. For a number of reasons, we expect regulators to reduce their 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 (Friesen 2006; Kaplow and Shavell 1994). Even in the regulatory context, where violations are often ongoing, regulators may be eager to reallocate limited enforcement resources away from self-reporters and toward seemingly less cooperative facilities, and/or to reduce agency costs by inspecting fewer facilities. Second, 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). Inspection relief is a key strategy for rewarding voluntary disclosers, building trust and responsibility, and encouraging cooperative behavior. Indeed, 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), and some have argued that regulators should credibly commit to easing

regulatory scrutiny in order to incentivize self-regulatory behavior (Ayres and Braithwaite 1992; Maxwell and Decker 2006).

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

By contrast, the 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: 17).³

Although the EPA acknowledges that the *Audit Policy* can only elicit voluntary disclosures if it avoids the impression that self-reporting will attract *increased* regulatory scrutiny,⁴ there are at least two reasons why the agency might not offer an explicit *quid pro quo*.

³ For example, the 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 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).

⁴ In a conversation with one of the authors, an EPA program administrator noted that "[t]he Agency has to avoid the perception that it is picking on companies who participate in the Audit Policy" (Personal communication, March 16, 2004).

First, structured interviews and informal conversations with EPA staff revealed that the agency might want to maintain the flexibility to inspect particular disclosers as it sees fit, because many regulators see voluntary disclosures as red flags indicating that self-reporting facilities might be concealing bigger problems.⁵ 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 heightened administrative procedural requirements and judicial oversight.⁶ 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 both from public notice and comment and from judicial review (Lobel 2005; Sparrow 2000). We test whether, despite its equivocation, the EPA does, in fact, grant inspection relief to voluntary disclosers, generating greater opportunities for enforcement efficiencies, targeting leverage, and regulatory cooperation.

4.2 *Assessing Voluntary Disclosers' Commitment to Self-Police*

We next examine whether there is any warrant for granting enforcement relief to self-reporters, based on their subsequent compliance performance. We suggest that, in this context, there is reason to believe that self-reporting will be a reliable indicator of effective firm-level self-policing that would justify inspection relief, because the disclosure of legal violations is a

⁵ In a conversation with one of the authors, a former 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 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). See also discussion, *infra*, at p. 16.

⁶ 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.

risky and potentially costly act, especially for firms that cannot live up to the associated self-policing commitments.

Companies have gone to great lengths in recent years to communicate to regulators and the broader public their commitment to police themselves 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 identifies firms that have actually implemented effective internal self-policing practices (Darnall and Carmin 2005). Outward indicators of self-regulation typically impose minimal costs on the firms that adopt them (Darnall and Sides 2008; Lyon and Maxwell 2007). Voluntary program design has focused on reducing costs to *attract* participants, but less attention has been paid to the incentives necessary to promote meaningful corporate harm reduction. Consequently, the fact that a firm adopts some form of voluntary regulation reveals little about whether it is likely to follow through with the associated commitments. Indeed, some have argued that self-regulation has been primarily a vehicle for self-promotion (Bruno 1992; Krut and Gleckman 1998; Lyon and Maxwell forthcoming; Rhodes 2007) and regulation avoidance (King and Lenox 2000; Maxwell, Lyon and Hackett 2000). Most self-regulatory programs have therefore been poor proxies for good compliance behavior.

However, voluntary regulation programs have been consistently associated with meaningful self-policing under circumstances where they impose costs on those that fail to implement their commitment to internally deterring harmful behavior. These costs have been built into self-regulatory schemes in different ways; for instance, by requiring third-party certifications of self-policing quality and denying certification to firms that fail to maintain robust self-policing practices (Potoski and Prakash 2005; Toffel 2006; Weil 2005) and by

imposing more stringent government regulation on those that fail to regulate themselves effectively (Innes and Sam 2008; Maxwell, Lyon and Hackett 2000).

We argue that the *Audit Policy* similarly makes representations of self-policing more credible by making them more costly, especially for those who would misrepresent their self-policing intentions or capacity. Unlike many other common indicators of self-policing, the voluntary disclosure of regulatory violations is associated with three 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 illegal actions has the potential to damage a firm's reputation, both with the regulator and with the public. As mentioned above, regulators sometimes view voluntary disclosures as "red flags," indicating more serious wrongdoing. And firms' voluntary disclosures are publicly available, so business partners, customers, and shareholders may become aware of wrongdoing that the firm otherwise could have concealed. Finally, disclosing a violation to the regulator creates a risk of greater future litigation costs. Because no federal audit privilege protects voluntary disclosers, self-reports and the audit materials that support them can invite citizen suits that would impose costs far beyond those directly associated with voluntary disclosure, especially where private attorneys general suspect that the discloser has not cleaned up its act.

These risks are much greater for firms that would game the system than for firms that wish to communicate, through their disclosures, a genuine commitment to self-policing. 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 regulators, private attorneys general, and the general public. Our interviews with regulators suggest that they are particularly wary of being duped by self-reporters. One inspector reported that voluntary disclosures can raise a “red flag,” but “[o]nly if they self-disclose and they’re not meeting the requirements of their self disclosure” (Interview Transcript 4, 2009). Another noted that the agency may punish firms more harshly when they misrepresent their self-policing efforts. “If we go to a place that has voluntary programs and we find violations, we’re more likely to take enforcement. If we find violations, it’s almost like they’re being dishonest in the voluntary program” (Interview Transcript 1, 2009). In this way, self-reporting stands in stark contrast to the many self-regulation symbols that merely provide a platform for self-promotion in that the voluntary disclosure of legal violations is arguably “too costly to fake” (Camerer 1988: S186).

For these reasons, we hypothesize that, on average, companies that self-report violations to the *Audit Policy* will make meaningful investments in self-policing to deter harmful conduct by their employees. 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.

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. First, we measure regulatory scrutiny as the annual number of CAA inspections to which each facility has been subjected. We calculated this measure based on data from the Aerometric Information Retrieval System (AIRS) database.⁷

We use two dependent variables to assess the extent to which facilities are honoring their commitment to self-police. Specifically, we examine regulatory inspection records as recorded by a third-party, and abnormal releases of toxic chemicals that facilities are required to self-report to the regulator. Relying on these different metrics enables us to triangulate our results, and enhance the reliability of our empirical results (Campbell and Fiske 1959; Jick 1979).

A key facility-level metric that should be enhanced by effective self-policing is the facility's regulatory compliance record, a third-party assessment recorded by regulatory inspectors. To measure environmental compliance, we created *clean inspection*, a dichotomous variable that refers to a facility's CAA regulatory inspection on a particular date. This variable is coded "1" when the inspection resulted in no compliance violations ("clean") and "0" when the

⁷ To avoid our results being driven by outliers, we recoded annual inspection tallies beyond 14, the 99.9th 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 99th percentile, to 8.

inspector cited the facility for a violation (“dirty”). This distinction between whether or not inspections resulted in violations has been used in other empirical analyses of regulatory compliance (Gray and Scholz 1993; U.S. General Accounting Office 2001). We obtained data on CAA inspections from 1991 through 2003 from the EPA’s AIRS/AIRS Facility Subsystem (AFS) database.

Effective self-policing should also reduce the occurrences of abnormal operational events such as accidental releases of pollutants to the environment. In contrast to regulatory inspections which are recorded by a third party, certain facilities face regulatory requirements to self-report accidental toxic chemicals releases to the EPA under penalty for misreporting or not reporting incidents. We obtained data on these abnormal releases of toxic chemicals from the EPA’s Toxic Release Inventory (TRI) database. In this context, abnormal releases refer to toxic chemical emissions that result from circumstances outside of routine operations; for example, tank ruptures, vehicle accidents, or improperly maintained waste-pond berms (U.S. Environmental Protection Agency 2007: 58).⁸ Interviews with state and federal regulators as well as company environmental managers suggested that self-policing can result in improved housekeeping and updated management plans, both of which can reduce the severity and frequency of environmental releases associated with such abnormal or accidental events.⁹ For example, ongoing auditing can help managers ensure that equipment is properly maintained and

⁸ 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 to the 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.

⁹ 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.

that staff members adhere to training schedules, both of which can prevent breakdowns and accidents.¹⁰ We calculated the annual *number of abnormal releases* from the subset of facilities in our sample that were required to report TRI data.¹¹

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 EPA *Audit Policy* from the EPA Integrated Compliance Information System (ICIS) database, the EPA Audit Policy Docket, and lists of facilities that responded to various EPA Compliance Incentive Programs. The EPA provided these datasets in response to Freedom of Information Act requests.

Control variables. We obtained data for two measures of facility size. We gathered Dun & Bradstreet data on annual facility *employment* from the National Establishment Time-Series (NETS) Database. Because the number of abnormal releases might increase if production levels increase, we also obtained data, from the TRI database, on facilities’ annual *production ratio* values; that is, the ratio of a facility’s production level in the focal year to the prior year.¹² 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

¹⁰ 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).

¹¹ Facilities are required to report TRI data if they have at least 10 employees, operate in a targeted industry (such as 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, 6).

¹² As we were unable to find data on actual production levels, we employ production ratios as a proxy.

undue influence from outliers, top-coding values at the 99.9th percentile for the entire sample distribution of mean production ratios.

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 one-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).¹³ 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 EPA's ICIS database.¹⁴

We also obtained several measures of general deterrence. Every two years, the EPA announces National Priority sectors to be targeted as nationwide enforcement priorities. We coded *National Priority sector* as a dummy variable based on data from the EPA website.¹⁵ Second, we created a dummy variable to identify facilities that were EPA *Compliance Incentive Program targets* based on data obtained from the agency via Freedom of Information Act requests. These programs explicitly encourage facilities in particular EPA Regions or industries

¹³ 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).

¹⁴ 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.

¹⁵ The EPA's National Priority sectors can be found at <http://www.epa.gov/compliance/data/planning/shortterm.html>.

or that conduct specific regulated activities to reexamine their compliance status regarding a particular regulatory issue and to self-report and correct any violations discovered. Third, we created two annual state-level variables pertaining to CAA enforcement based on AIRS data: *total CAA penalties assessed* (in dollars) by environmental regulators and *total number of CAA regulated facilities*.

5.2 Regulatory Scrutiny Model

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

$$y_{i,t} = f(\beta_1 \mathbf{D}_{i,t} + \beta_2 \mathbf{X}_{i,t} + \beta_3 \mathbf{S}_{i,t} + \beta_4 \boldsymbol{\tau}_{i,t} + \beta_5 \boldsymbol{\lambda}_t + \boldsymbol{\alpha}_i, \varepsilon_{i,t}) \quad (1)$$

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

We control for many potential determinants of inspections in $\mathbf{X}_{i,t}$. 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 EPA notes that, given its limited resources, achieving compliance “is dependent on effective targeting of the most significant public health and environmental risks” (U.S. Environmental Protection Agency 1999b: 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 1999b: 20). Indeed, EPA 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 include a series of dummy variables to denote the number of years since the facility was last subjected to a CAA inspection. Specifically, we include three dummy variables to denote 2, 3, and 4 and more years since a facility's last 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 an EPA Compliance Incentive Program or an EPA National Priority sector. We control for variation in enforcement strategies within states over time by including the log of total penalties assessed by environmental regulators and the log of total regulated facilities in each state-year ($S_{i,t}$).

We 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 (λ_t) to control for year-specific factors, such as changes in presidential administration, Congress, and EPA leadership, that might affect inspection rates. We include conditional fixed effects (α_i) 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).

5.3 Regulatory Compliance Record Model

To evaluate the effect that self-reporting a violation and committing to self-policing has on regulatory compliance records, we pursued 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:

$$y_{i,d} = f(\beta_1 \mathbf{D}_{i,d} + \beta_2 \mathbf{X}_{i,t} + \beta_3 \mathbf{I}_{i,t} + \beta_4 \lambda_t + \beta_5 \tau_{i,t} + \alpha_i, \varepsilon_{i,d}) \quad (2)$$

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* ($\mathbf{D}_{i,d}$) is our estimate of the change in the 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 probability for 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 $\mathbf{X}_{i,t}$ the number of inspections and violations a facility experienced during each of the prior two years because a facility's recent regulatory experience can affect its subsequent 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 also affect compliance behavior (Laplante and Rilstone 1996; Shimshack and Ward 2005), we control for the predicted probability of being inspected ($\mathbf{I}_{i,t}$) (Earnhart 2004; Eckert 2004; Gray and Deily 1996; Laplante and Rilstone 1996). We estimate a facility's probability of being inspected at least once in a

given year using the predicted value from the inspection model specified above as Equation (1) but estimated using pooled logistic regression.¹⁶

We include facility-level conditional fixed effects (α_i) 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). As with Equation (1), we also include as control variables a full set of dummies indicating the number of years before or after the match year ($\tau_{i,t}$) and a full set of dummies to denote years (λ_t).

5.4 *Abnormal Environmental Releases Model*

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

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

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* ($\mathbf{D}_{i,t}$).

Because changes in facility size and production quantities may affect the number of abnormal releases, $\mathbf{X}_{i,t}$ includes log *employment* and log *production ratio*.¹⁷ We also include a full set of dummies ($\tau_{i,t}$) to control for the number of years before or after the match year. We

¹⁶ We use pooled logistic regression here to generate predicted probabilities because the conditional fixed-effects logit model does not yield predicted probabilities that incorporate the fixed effects.

¹⁷ Our results were unchanged when we estimated models that omitted these two control variables.

include a full set of year dummies (λ_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 (α_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.

5.5 *Matched Sample*

The three models described above employ a difference-in-differences approach whereby we compare changes in the number of regulatory inspections, the probability that a regulatory inspection yields no compliance violations, and the number of abnormal releases among self-reporting (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 (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, compliance, and pollution behavioral trends 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 generating 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” and “control” facilities that do not differ systematically from each other at the time the treatment occurs (Shadish, Cook and Campbell 2002), which in our case is when voluntary disclosure occurs. Relying on matched samples has been shown to significantly reduce bias in program evaluation (Blundell and Costa Dias 2000; Smith and Todd 2005).

To develop our matched sample, we implement case-control matching based on several factors which 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 its 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.¹⁸ 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.

¹⁸ 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 in the prior (calendar) year; (3) number of CAA inspections in the year two years prior; (4) number of CAA violations in the prior year; (5) number of CAA violations in the year two years prior; (6) a dummy coded “1” if the facility faced any enforcement actions in the prior year; and (7) a dummy coded “1” if the facility faced any enforcement actions in the year two years prior. 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.

Our matching process resulted in our discarding many facilities for which matches could not be identified. This substantially increased the proportion of disclosing facilities (treatments) in our matched sample because matching led us to discard many more potential controls than treatments. Matching resulted 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.

In our analysis, we focused on facilities' initial disclosure and considered a facility to be a treatment facility in that year and in all subsequent years. We believe this to be a reasonable way to consider treatment in our context, since 70% of the facilities that voluntarily disclosed to the *Audit Policy* did so only once and, of those that disclosed more than once, the vast majority (approximately 90%) did so only twice, with repeat disclosures averaging only 1.4 years apart.

Although 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, we cannot rule out the possibility that time-variant unobservables might influence our results. However, our models that predict organizational outcomes are associational, not causal: the reductions in compliance violations and abnormal environmental releases are caused not by the self-disclosures that we model, but rather by the unobserved self-policing (internal auditing) for which we take the disclosures to be an indication. Unobservable differences should therefore not generate biased estimates of the association between disclosures and these outcomes. In contrast, our regulatory scrutiny model is a causal model that predicts that regulators will respond directly to *Audit Policy* disclosures under the assumption that they indicate internal auditing. Consequently, as with any evaluation that draws causal inferences based on difference-

in-differences regression on a sample matched on observables, our regulatory scrutiny results should be interpreted with the caveat of the identifying assumption that time-variant unobservables are not correlated with both disclosing and regulatory scrutiny.

6. Results

Descriptive statistics are provided in Table 1. We report the results of our regressions that predict outcomes recorded by the regulator (inspection frequency and clean inspections), followed by the results of our regressions that predict self-reported outcomes (abnormal releases) in Tables 2-4. All specifications include facility fixed effects, with standard errors calculated using block bootstrap with 500 replications.¹⁹ Although we also report results from the entire sample for comparison purposes, we rely on results from the matched sample as the basis of our interpretation and inferences, noting that in most cases both samples yield similar coefficient estimates and confidence intervals (see Table 5).

6.1 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 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

¹⁹ 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.

inspection holidays to facilities that voluntarily disclosed and committed to self-policing (Table 2).

The results of the conditional fixed-effects logistic model reported in Column 2 of Table 2 indicate that facilities that self-reported subsequently experienced a 26% decline in the probability of facing any inspections compared to the matched controls ($\beta=-0.30$; $p<0.01$; $OR=0.74$). To put this result into context, this refers to a seven-percentage-point decrease in the probability of an inspection from the 37% baseline to 30%.²⁰

The negative binomial results reported in Column 4 of Table 2 indicate that after facilities voluntarily disclosed, the annual number of inspections declined by 17% ($\beta=-0.19$; $p<0.01$; $IRR=0.83$). This effect corresponds to a decrease in the number of annual inspections during the two-year period prior to the match year from the 0.50 baseline to 0.42 (calculated as $IRR \times$ baseline rate, or 0.83×0.50). In other words, on average, 100 disclosing facilities would be subjected to a total of 40 fewer inspections over a five-year period (calculated as a 0.08 decrease in inspections per facility-year \times 100 facilities \times 5 years).

6.1.1. Robustness Tests

Our conditional fixed-effects logistic regression results were nearly identical when, instead of using block bootstrap, we clustered standard errors by facility. Our conditional fixed-effects negative binomial regression results were nearly identical to results from our estimated this model using a conditional fixed-effects Poisson regression with robust standard errors clustered by facility.

²⁰ The 37% figure refers to the mean of the dependent variable in this matched sample during the two years prior to the match year (0.37). The 30% figure corresponds to the probability (p) is calculated based on odds (Ω) and odds ratio (OR) as follows. First, convert the baseline probability of 0.37 to a baseline odds as $\Omega=p/(1-p) = 0.37 / (1-0.37) = 0.5873$. Second, multiply the result by the odds ratio estimate as $\Omega \times OR = 0.5873 \times 0.74 = 0.4346$. Third, convert the result to a probability as $\Omega = p/(1-p) = 0.4346 = p(1-p)$. Thus $p=0.30$ or 30%.

Our difference-in-differences approach relies on an identifying assumption that 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 if discloser facilities had not committed to self-policing through the *Audit Policy*'s voluntary disclosure vehicle. Although this assumption cannot be tested, it is more plausible if the two groups had indistinguishable trends during the pre-period. 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.²¹

To assess the risk that mean reversion might be driving our results, we estimated an annual treatment effects models. Specifically, we replaced the single *voluntarily disclosed* variable (which estimated the average treatment effect) with a series of dummy variables indicating the first year after voluntary disclosure, the second year after voluntary disclosure, and so on. If our main results were driven by mean reversion, we would expect only the first (or perhaps the first two) of these annual treatment effects to be negative and large. The results of these annual treatment effect models, reported in Table A1 in the Appendix, provide no evidence that our results were driven largely by changes in inspector behavior during the first post-disclosure year. Indeed, the results of both of these models indicate that the inspectors' scrutiny declined over time, suggesting that mean reversion is unlikely to be driving our results.

To assess the extent to which our analysis might be biased by substantive differences in observables during the pre-match period between the matched treatment and control facilities, we calculated standardized differences (Rosenbaum and Rubin 1985) for all of the covariates,

²¹ 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 the two groups had indistinguishable pre-trends ($p=0.96$).

regardless of whether or not they were part of the matching regime.²² The absolute values of the standardized differences were below the 20% threshold indicated by Rosenbaum and Rubin (1985) for five of the seven covariates. The two covariates whose standardized differences exceeded the 20% threshold were the dichotomous variables *Compliance Incentive Program target* and *National Priority sector*. To assess the extent to which these imbalances might be biasing our results, we split the matched sample into two subsamples: (a) those treatment and control facilities that had been Compliance Incentive Program targets and/or in National Priority sectors during this pre-match period and (b) those treatment and control facilities that had been neither Compliance Incentive Program targets nor in National Priority sectors during this pre-match period. We re-estimated the model on these two subsamples, the results of which continued to indicate statistically significant reductions in regulatory scrutiny ($\beta = -0.12$, $p = 0.086$ for subsample (a); $\beta = -0.24$, $p < 0.01$ for subsample (b)). The substantial overlap of the confidence intervals for these two estimates²³ implies they are statistically indistinguishable, indicating little cause for concern that differences in observables are biasing our main results.

6.2 Regulatory Compliance Records

We employ conditional fixed-effects logistic regression to estimate the probability that a regulatory inspection is “clean”; that is, it reveals no violations. Recall that this model includes a generated regressor because the probability of inspection is a predicted value from a pooled logit

²² We calculated standardized differences pursuant to Rosenbaum and Rubin (1985) as follows:

$$\text{Standardized Difference}(X) = 100 \times \frac{\bar{X}_{Tm} - \bar{X}_{Cm}}{\sqrt{\frac{V_{Tr}(X) + V_{Cr}(X)}{2}}}$$

where \bar{X}_{Tm} and \bar{X}_{Cm} represent the sample means of covariate X for the matched treatment facilities and the matched control facilities, respectively, during the two years prior to the match (treatment) year. $V_{Tr}(X)$ and $V_{Cr}(X)$ represent the variance of covariate X for the entire reservoir of treatment and control facilities (facilities in the entire sampling frame regardless of whether or not they were in the matched sample).

²³ The 95% confidence interval was (-0.254, 0.017) for sample (a) and (-0.351, -0.125) for subsample (b).

model. Thus, the block bootstrap standard errors we report here are calculated as a result of a sequential two-step estimation process (Cameron and Trivedi 2005: 200; Cameron and Trivedi 2009: 427) that we implemented as follows. All observations pertaining to a randomly drawn subsample of facilities were used to estimate the pooled logit regression that predicts the probability of an inspection occurring that year. Predicted values generated for this subsample were used as a generated regressor in the conditional fixed-effects logistic regression that estimates clean inspections. This procedure was repeated 500 times to generate our results.

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” ($\beta=0.73$; $p=0.075$; $OR=2.08$), compared to the matched controls over the same time period (Table 3). This effect corresponds to a 7.4-percentage-point increase in the probability of a clean inspection from the 84.6% baseline to 92.0%.²⁴

6.2.1. Robustness Tests

Clustering standard errors by facility yielded results nearly identical to those obtained via block bootstrapping, resulting in a more precise estimate (clustered $SE=0.38$ and $p=0.054$, compared to block bootstrap $SE=0.41$ and $p=0.075$). A t-test confirmed that our results were not confounded by voluntary disclosers having faster improvement trends during the pre-disclosure

²⁴ The 84.6% baseline is the mean of the dependent variable in this matched sample during the two years prior to the match year (0.846). The 92.0% figure refers to the probability (p) calculated based on odds (Ω) and odds ratio (OR) as follows. First, convert the baseline probability of 0.846 to a baseline odds as $\Omega=p/(1-p) = 0.846 / (1-0.846) = 5.4935$. Second, multiply the result by the odds ratio estimate as $\Omega \times OR = 5.4935 \times 2.08 = 11.4265$. Third, convert the result to a probability as $\Omega = p/(1-p) = 11.4265 = p(1-p)$. Thus $p=11.4265 / 12.4265 = 0.9195$ or 92.0 %.

period than the matched non-disclosers.²⁵

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 0.090 (odds ratio=1.09), a magnitude close to zero and not statistically significant. Also, a one-standard-deviation change in *predicted probability of inspection* is associated with a very minor change in the odds of a clean inspection of a magnitude very close to a null effect of 1.0.²⁶ 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 the model omitting *predicted probability of inspection* yielded a coefficient on our focal variable *voluntarily disclosed* that was similar in magnitude and significance to our primary model. Fourth, we re-estimated the model using Arellano-Bond general methods of moments (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*. Each of these alternative models yielded positive, statistically significant coefficients on *voluntarily disclosed*, which supports our primary results. These additional analyses lead us to believe our results are robust to potential autocorrelation.

To assess the robustness of our results to estimation techniques, we re-estimated the model employing a two-stage panel data instrumental variables model to simultaneously estimate

²⁵ 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 more quickly than the matched non-disclosers (p=0.96).

²⁶ This "very minor change" is calculated as $\exp(\beta * SD) = \exp(0.09 * 0.15) = 1.01$, using the predicted *probability of inspections* standard deviation of 0.15 per Panel B of Table 1.

the “probability of an inspection” as a first stage and “clean inspection” model as a second stage. This model, designed to accommodate continuous rather than our dichotomous variables, also yielded a positive, statistically significant coefficient on *voluntarily disclosed*.

Another potential concern with our model is that our results might be confounded by 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 apparent improvement in compliance in the subsequent year could be interpreted as merely regressing back to the mean. In contrast to this scenario, the results of an alternative model with annual treatment effects, reported in Table A2 in the Appendix, indicate that disclosers show significantly increased likelihood of clean inspections during later post-disclosure years, which suggests that mean reversion is unlikely to be driving our results.

As above, we assessed the extent to which our analysis might be biased by substantive differences in observables during the pre-match period between the matched treatment and control facilities by calculating standardized differences for all covariates in the model. For only one of the covariates, lagged inspections, did the absolute value of the standardized difference exceed the 20% threshold. To test whether this imbalance might be biasing our regression results, we trimmed the matched sample to include only matched treatments and controls with annual inspection rates during the two-year period prior to the match year that were within the 95th percentile of the common support; that is, 0, 1, or 2 annual inspections the prior year. The absolute value of the standardized differences for all covariates in this trimmed sample fell within than the 20% threshold. The results of our regulatory compliance model when estimated

for this trimmed sample continued to indicate a statistically significant increase ($\beta=0.83$, $p=0.03$, $OR=2.30$) in the probability of a clean inspection following disclosure, with a coefficient magnitude very similar our main results. The substantial overlap of the 95% confidence intervals on *voluntarily disclosed* between this estimate (0.07, 1.60) and that of our main result (-0.01, 1.48) implies that the two estimates are statistically indistinguishable, indicating little cause for concern that differences in observables are biasing our main results.

In light of our earlier finding that regulators reduced the frequency with which they inspected self-disclosing facilities, it is possible that when inspectors return to these facilities they might inspect less intensively, which could contribute to our finding a reduction in compliance violations among self-disclosers. However, our interviews with regulatory inspectors provide evidence against a systematic bias in favor of (or against) facilities that disclosed to the *Audit Policy*. Some inspectors reported that they did not know whether the facilities they inspected were voluntary disclosers and those inspectors who did know said that this knowledge had no impact on the way they conducted their inspections. As one inspector noted, “We look at everything and it makes no difference one way or the other” (Interview Transcript 5, 2009). An inspector with relatively broad experience inspecting voluntary disclosing facilities stated his belief that self-policing produced mixed results that necessitated ongoing scrutiny. Discussing the quality of internal compliance auditing conducted by voluntary disclosing facilities, he said: “It really varies. I’ve seen companies that took it to heart, but it didn’t affect how we inspect them, and I’ve seen companies where they say ‘We’re part of all these programs,’ and found a lot of violations” (Interview Transcript 1, 2009). In addition, EPA takes structural precautions against inspection bias, strictly segregating its office for voluntary programs from its field inspection operations to avoid any actual or apparent conflict of interest. Thus, although we

cannot rule out the possibility that some inspectors might inspect self-disclosers less intensely and we highlight this issue to provide context when interpreting our results, our qualitative evidence suggests that such a reduction in intensity is neither prevalent nor systematic.

6.3 *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 based on the matched sample (Column 2 of Table 4) indicate that the expected annual number of abnormal releases declined by 20% ($\beta=-0.22$; $p<0.01$; $IRR=0.80$) after facilities voluntarily disclosed to the *Audit Policy*, compared to the matched non-disclosers over the same time period. This corresponds to a decline in abnormal releases per year from the 1.2 baseline to 0.96.²⁷ This 0.24 decline per facility-year is equivalent to 120 fewer abnormal releases amongst 100 disclosing facilities over a five-year period (calculated as 0.24 releases per facility-year \times 100 facilities \times 5 years).

6.3.1. Robustness Tests

Our results were very similar when the model was re-estimated using conditional fixed effects Poisson regression with robust standard errors clustered by facility. In addition, 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

²⁷ The 1.2 baseline is the matched sample average during the two years prior to the match year. The 0.96 figure is calculated as $IRR \times$ baseline rate, or 0.80×1.2 .

releases during the pre-match period, providing no evidence that this factor confounded our results.²⁸

To assess the potential concern that our results might be driven by regression to the mean, we estimated an annual treatment effects model. The results, reported in Table A3 in the Appendix, indicate that reductions in abnormal releases during the first post-disclosure year were statistically indistinguishable from reductions in subsequent years, which suggests that mean reversion is unlikely to be driving our results.

We also assessed the extent to which our analysis might be biased by substantive differences in observables during the pre-match period. In this estimation sample, the absolute value of the standardized difference for all covariates fell within the 20% threshold, indicating little cause for concern that differences in observables are biasing our main results (Rosenbaum and Rubin 1985).

7. Conclusion

Our results provide evidence that self-reporting can reliably indicate effectively implemented self-policing and that regulators are, in fact, using self-reporting to identify firms that are meaningfully monitoring their own operations. Specifically, we find that regulators rewarded voluntary disclosers with inspection relief, suggesting that integrating self-reporting into regulatory design can help regulators economize government enforcement resources and develop cooperative relationships with committed self-policers. We also demonstrate that

²⁸ Specifically, we calculated the difference between the number of abnormal releases each facility experienced in the match (disclosure) year and the number it experienced in the two prior years. 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.

facilities that voluntarily disclosed a violation and committed to self-police under the *Audit Policy* improved their environmental performance by (1) improving their environmental compliance records, being cited by regulators for fewer violations than similarly situated non-disclosers, and (2) reducing their accidental releases of toxic chemicals to the environment. 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 find evidence that suggests 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 EPA grants inspection relief to voluntary disclosers. It is important to note that, while they inspect self-reporters at reduced rates, regulators do continue to monitor these facilities. This suggests an attempt on the part of the agency to blend responsive and deterrence-based enforcement tools.

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

Third, we demonstrate that, when self-reports of legal violations are conditioned on a commitment to self-police, they 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 leverage the deterrent effects achieved at these facilities and reallocate enforcement resources toward facilities that are not deterring harmful conduct internally. 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.

Finally, 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 (Arlen and Kraakman 1997; Coglianesi and Nash 2001; Maxwell and Decker 2006; Pfaff and Sanchirico 2000). Agencies, too, have focused on incentives in designing voluntary programs, going to great lengths to reduce the costs²⁹ and emphasize the benefits³⁰ of self-policing. Unfortunately, in their efforts to incentivize participation, regulators often strip self-regulatory commitments of any informational value they

²⁹ For example, the EPA promotes its *WasteWise* program as “free, voluntary, flexible” and makes clear that “[t]he 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)).

³⁰ The 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” (Letter from the Hon. Albert R. Wynn, Chairman, House Subcommittee on Environment and Hazardous Materials, and the Hon. Bart Stupak, Chairman, House Subcommittee on Oversight and Investigations, to the Hon. Stephen L. Johnson, Adm’r, EPA (April 13, 2007)).

might have. Regulators can enhance overall deterrence only if they can identify accurately which facilities are policing themselves effectively and can 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 gamers to accept. Our findings suggest that self-reporting coupled with a commitment to self-police can serve as a valuable starting point for thinking about how to sort regulated firms by their compliance capacities and motivations.

Our findings raise a number of interesting questions for future research. Although our study suggests the possibility of enforcement efficiencies, it does not account for the costs of self-policing or the nature of enforcement efficiencies achieved. 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. It is also important to determine how enforcement resources shifted away from voluntary disclosers are being reallocated and whether self-policing is contributing to enhanced overall deterrence levels or to reduced enforcement costs.

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 increasing 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 inspection analysis in Table 2

	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 inspections ^a	0.76	1.01	0	14	0.64	0.79	0	14
Voluntarily disclosed (dummy)	0.01	0.08	0	1	0.02	0.14	0	1
Years since prior inspection ^b	1.94	1.18	1	4	2.08	1.24	1	4
Number of violations 1 year ago ^c	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

PANEL B. Sample for compliance analysis in 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
Voluntarily disclosed (dummy)	0.02	0.14	0	1	0.08	0.27	0	1
Probability of an inspection this year (predicted)	0.67	0.13	0.21	0.93	0.61	0.15	0.13	0.89
Number of inspections 1 year ago ^c	2.24	2.42	0	8	1.40	1.84	0	8
Number of violations 1 year ago ^c	0.25	0.60	0	3	0.23	0.57	0	3

PANEL C. Sample for abnormal environmental releases analysis in Table 4

	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 ^a	1.70	3.17	0	17	2.22	3.53	0	17
Voluntarily disclosed (dummy)	0.02	0.13	0	1	0.04	0.19	0	1
Log production ratio ^c	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

Note. 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.

^a top-coded at 99.9th percentile

^b top-coded at 4 per year

^c top-coded at 99th percentile

TABLE 2
Self-policing is associated with fewer inspections and lower probability of being inspected

Dependent variable:	Conditional fixed-effects logistic regression				Conditional fixed-effects negative binomial regression			
	<i>Any annual inspections</i>				<i>Number of annual inspections</i>			
	(1)		(2)		(3)		(4)	
	Entire sample		Matched sample		Entire sample		Matched sample	
	Coefficients	Odds ratio	Coefficients	Odds ratio	Coefficients	Incident rate ratios	Coefficients	Incident rate ratios
Voluntarily disclosed	-0.256**	0.77	-0.303**	0.74	-0.053	0.95	-0.185**	0.83
	[0.082]		[0.114]		[0.037]		[0.052]	
2 years since last inspection	0.170**	1.19	0.372**	1.45	0.032**	1.03	0.126**	1.13
	[0.011]		[0.023]		[0.005]		[0.011]	
3 years since last inspection	0.282**	1.33	0.537**	1.71	0.064**	1.07	0.231**	1.26
	[0.016]		[0.031]		[0.009]		[0.018]	
4 or more years since last inspection	0.654**	1.92	1.177**	3.25	0.256**	1.29	0.618**	1.86
	[0.015]		[0.032]		[0.011]		[0.018]	
Number of violations 1 year ago	0.225**	1.25	0.157*	1.17	0.058**	1.06	0.026	1.03
	[0.028]		[0.071]		[0.010]		[0.027]	
Number of violations 2 years ago	0.071**	1.07	0.035	1.04	0.005	1.01	-0.010	0.99
	[0.027]		[0.067]		[0.011]		[0.027]	
Any enforcement actions 1 year ago	-0.079+	0.92	-0.142	0.87	-0.004	1.00	0.002	1.00
	[0.046]		[0.087]		[0.020]		[0.035]	
Any enforcement actions 2 years ago	-0.150**	0.86	-0.185+	0.83	-0.016	0.98	-0.024	0.98
	[0.046]		[0.099]		[0.022]		[0.046]	
Compliance Incentive Program target	0.045	1.05	0.049	1.05	0.022+	1.02	0.049*	1.05
	[0.027]		[0.045]		[0.013]		[0.020]	
National Priority sector	0.014	1.01	0.282**	1.33	0.031**	1.03	0.102**	1.11
	[0.018]		[0.034]		[0.009]		[0.020]	
Log total CAA penalties in the state-year	0.021**	1.02	0.013	1.01	0.002	1.002	-0.012**	0.99
	[0.003]		[0.008]		[0.002]		[0.004]	
Log number of CAA-regulated facilities in the state-year	1.675**	5.34	1.390**	4.02	0.791**	2.21	0.623**	1.87
	[0.044]		[0.102]		[0.021]		[0.042]	
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	5688.7**		2782.4**		4616.5**		4733.2**	

Note. Block bootstrap standard errors in brackets (500 replications). Unit of analysis is the facility-year. Models 2 and 4 are estimated on the matched sample and include matched facilities' observations starting 2 years prior to their match year through 5 years after the match year. The conditional fixed-effects logistic regressions (Models 1 and 2) omit facilities for which annual inspection rates are either always positive or always zero throughout the sample period. The conditional fixed-effects negative binomial regressions (Models 3 and 4) omit facilities that have identical annual inspection rates throughout the sample period.

+ p<0.10; * p<0.05; ** p<0.01.

TABLE 3
Self-policing is associated with fewer compliance violations

Conditional fixed-effects logistic regression
Dependent variable: Clean inspection

	(1)		(2)	
	Entire sample		Matched sample	
	Coefficients	Odds ratio	Coefficients	Odds ratio
Voluntarily disclosed	0.730+	2.0757	0.733+	2.08
	[0.376]		[0.412]	
Probability of an inspection this year (predicted)	0.107	1.1124	0.090	1.09
	[0.387]		[0.598]	
Number of inspections 1 year ago	-0.194**	.82383	-0.200**	0.82
	[0.043]		[0.076]	
Number of inspections 2 years ago	-0.134**	.8749	-0.134	0.87
	[0.046]		[0.083]	
Number of violations 1 year ago	1.089**	2.9709	1.182**	3.26
	[0.123]		[0.224]	
Number of violations 2 years ago	1.129**	3.09395	1.194**	3.30
	[0.120]		[0.229]	
Facility-level conditional fixed-effects	Included		Included	
Year fixed effects (1994-2003)	Included		Included	
Fixed effects for <i>t</i> years before/after match year			Included ^a	
Observations (Inspections)	11,326		4,174	
Facilities	1,772		713	
Wald Chi squared	257.0***		154.5***	

Note. Block bootstrap standard errors in brackets (500 replications). 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. Model 2 is estimated on the matched sample and includes facilities' observations spanning 2 years prior to the match year through 5 years after the match year. The conditional fixed-effects logistic regression models omit facilities with no variation in the dependent variable throughout the sample period, including facilities in our sample that maintained uniform (perfect) compliance records during our sample period. *Predicted probability of an inspection this year* is the predicted value from the inspection model specified in Equation (1) estimated with pooled logistic regression.

+ $p < 0.10$; * $p < 0.05$; ** $p < 0.01$.

^a To overcome collinearity with the year fixed effects that prevented the model from converging, we merged the dichotomous dummy variables *4 years after match year* and *5 years after match year* into a single dichotomous variable.

TABLE 4
Self-policing is associated with fewer abnormal environmental releases

Conditional fixed-effects negative binomial regression
Dependent variable: Number of abnormal releases

	(1)		(2)	
	Entire sample		Matched sample	
	Coefficients	Incident rate ratio	Coefficients	Incident rate ratio
Voluntarily disclosed	-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
Facility-level conditional fixed-effects	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**	

Note. Block bootstrap standard errors in brackets (500 replications). The unit of analysis is the facility-year. These models also include a dummy variable denoting when a missing employment value was recoded to zero. These models are estimated only on those facilities that reported data to the EPA's Toxic Release Inventory. Model 2 is estimated on the matched sample and 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 regression models omit facilities that have identical annual abnormal release rates throughout the sample period.

+ p<0.10; * p<0.05; ** p<0.01.

Table 5. Comparing results: Entire sample to matched sample

Voluntarily disclosed coefficients, with 95% confidence intervals in brackets

Dependent variable:	Any annual inspections	Number of annual inspections	Clean inspection	Number of abnormal releases
Entire sample	Table 2, Model 1 -0.26 [-0.42, -0.10]	Table 2, Model 3 -0.05 [-0.13, 0.02]	Table 3, Model 1 0.73 [-0.01, 1.47]	Table 2, Model 1 -0.17 [-0.27, 0.07]
Matched sample	Table 2, Model 2 -0.30 [-0.53, -0.08]	Table 2, Model 4 -0.18 [-0.29, -0.08]	Table 3, Model 2 0.73 [-0.07, 1.54]	Table 4, Model 2 -0.22 [-0.35, -0.10]

APPENDIX

Table A1. Inspection frequency annual treatment effect models

Estimation technique: Dependent variable:	(1) Conditional fixed-effects logistic regression <i>Any annual inspections</i> Matched sample		(2) Conditional fixed-effects negative binomial regression <i>Number of annual inspections</i> Matched sample	
	Coefficients	Odds ratio	Coefficients	Incident rate ratios
T1 1st year after voluntarily disclosed	0.081 [0.115]	1.08	-0.039 [0.058]	0.96
T2 2nd year after voluntarily disclosed	-0.191 [0.145]	0.83	-0.107+ [0.064]	0.90
T3 3rd year after voluntarily disclosed	-0.609** [0.165]	0.54	-0.232** [0.068]	0.79
T45 4th and 5th year after voluntarily disclosed	-0.854** [0.209]	0.43	-0.393** [0.076]	0.68
2 years since last inspection	0.372** [0.022]	1.45	0.126** [0.013]	1.13
3 years since last inspection	0.539** [0.029]	1.71	0.232** [0.020]	1.26
4 or more years since last inspection	1.180** [0.029]	3.25	0.619** [0.018]	1.86
Number of violations 1 year ago	0.159* [0.065]	1.17	0.026 [0.026]	1.03
Number of violations 2 years ago	0.031 [0.063]	1.03	-0.010 [0.028]	0.99
Any enforcement actions 1 year ago	-0.230** [0.082]	0.79	-0.036 [0.039]	0.96
Any enforcement actions 2 years ago	-0.177* [0.089]	0.84	-0.033 [0.042]	0.97
Compliance Incentive Program target	0.051 [0.040]	1.05	0.050* [0.021]	1.05
National Priority sector	0.280** [0.033]	1.32	0.101** [0.017]	1.11
Log total CAA penalties in the state-year	0.013+ [0.008]	1.01	-0.012** [0.005]	0.99
Log number of CAA-regulated facilities in the state-year	1.385** [0.095]	4.00	0.624** [0.046]	1.87
Facility-level conditional fixed effects	Included		Included	
Fixed effects for <i>t</i> years before/after match year	Included		Included	
Year fixed effects (1994-2003)			Included	
Observations	82,287		94,270	
Facilities	13,673		16,078	
Wald chi-squared	3058.7**		4134.7**	
Wald tests of indistinguishable coefficients:	T1 = T2: $\chi^2=3.26+$ T1 = T3: $\chi^2=16.97**$ T1 = T45: $\chi^2=18.15**$		T1 = T2: $\chi^2=0.89$ T1 = T3: $\chi^2=6.25*$ T1 = T45: $\chi^2=17.62**$	

Note. Brackets contain standard errors, clustered by facility in Model 1. See notes to Table 2. Compare results to inspection frequency average treatment effect models reported in Columns 2 and 4 of Table 2.
+ p<0.10; * p<0.05; ** p<0.01.

Table A2. Regulatory compliance annual treatment effect model

Conditional fixed-effects logistic regression
Dependent variable: Clean inspection

		Matched sample	
		Coefficient	Odds ratio
T1	1st year after voluntarily disclosed	0.248 [0.347]	1.29
T2	2nd year after voluntarily disclosed	0.459 [0.457]	1.59
T3	3rd year after voluntarily disclosed	1.541* [0.754]	4.70
T45	4th and 5th year after voluntarily disclosed	2.766** [1.002]	15.98
	Probability of an inspection (predicted)	0.242 [0.584]	1.29
	Number of inspections 1 year ago	-0.231** [0.075]	0.79
	Number of inspections 2 years ago	-0.122 [0.076]	0.88
	Number of violations 1 year ago	1.201** [0.203]	3.32
	Number of violations 2 years ago	1.208** [0.203]	3.34
	Facility-level conditional fixed-effects	Included	
	Fixed effects for <i>t</i> years before/after match year	Included ^a	
	Fixed effects for two-year periods (1994/5-2002/3)	Included	
Observations (inspections)		4174	
Facilities		713	
Wald chi-squared		158.9**	
Wald tests of indistinguishable coefficients:		T1 = T2: $\chi^2=0.23$	
		T1 = T3: $\chi^2=2.92^+$	
		T1 = T45: $\chi^2=6.47^*$	

Note. Brackets contain standard errors clustered by facility. See notes to Table 3. Compare results to regulatory compliance average treatment effect model reported in Column 2 of Table 3.

+ p<0.10; * p<0.05; ** p<0.01.

^a To overcome collinearity with the year fixed effects that prevented the model from converging, we merged the dichotomous dummy variables *4 years after match year* and *5 years after match year* into a single dichotomous variable.

Table A3. Abnormal release annual treatment effect model

Conditional fixed-effects negative binomial regression

Dependent Variable: Number of abnormal releases

	Matched sample	
	Coefficients	Incident rate ratio
T1 1st year after voluntarily disclosed	-0.208** [0.060]	0.81
T2 2nd year after voluntarily disclosed	-0.199** [0.061]	0.82
T3 3rd year after voluntarily disclosed	-0.274** [0.068]	0.76
T45 4th and 5th year after voluntarily disclosed	-0.261** [0.07]	0.77
Log production ratio	0.011 [0.033]	1.01
Log employment	0.002 [0.010]	1.00
Facility-level conditional fixed-effects	Included	
Year fixed effects (1994-2003)	Included	
Fixed effects for <i>t</i> years before/after match year	Included	
Observations	30,919	
Facilities	5,582	
Model Wald Chi-squared	9811.7**	
Wald tests of indistinguishable coefficients:	T1 = T2: $\chi^2=0.01$	
	T1 = T3: $\chi^2=0.76$	
	T1 = T45: $\chi^2=0.39$	

Note. Brackets contain standard errors. See notes to Table 4. Compare results to abnormal release average treatment effect model reported in Column 2 of Table 4.

+ p<0.10; * p<0.05; ** p<0.01.