Does mandatory labeling of outfall points influence pollution and compliance? Evidence from a natural experiment in Ohio

Xian Liu and Jay P. Shimshack*
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Xian Liu
Indiana University Purdue University
Indianapolis, IN 46202
xl46@iupui.edu
317-274-8709

Jay P. Shimshack
University of Virginia
Charlottesville, VA 22904
jay.shimshack@virginia.edu
434-243-7318

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ABSTRACT

This is the first paper to show that compulsory public labeling of pollution sources influences facilities' environmental performance and compliance. Using extensive regulatory compliance data and quasi-experimental methods that exploit policy institutions for identification, we document that an Ohio program that required visible signs at all water pollution outfalls had large impacts on pollution discharges and violations. Treated facilities' violations fell about one-third and overall discharges fell about eight percent relative to a counterfactual. Heterogeneity explorations are suggestive of several traditional and behavioral economic mechanisms.

KEYWORDS: enforcement, monitoring, information, disclosure, signs, notifications

1. Introduction

Enforcement and compliance under public law is costly and controversial, and scholars and policy-makers have debated related issues since at least Bentham (1789). However, nearly all of the existing evidence in the regulatory context explores the causes and consequences of traditional inspection and sanctioning tools.[1] This research explores a more novel policy instrument to encourage regulatory compliance: the use of information disclosure as regulatory leverage.

This is the first paper to study the pollution and compliance impacts of mandatory pollution outfall labeling. We assess an innovative Ohio EPA program that required water polluters to post signs containing permit numbers and contact information at each and every discharge point. We study this discharge labeling program for three reasons. First, standard theory presumes that disclosure without novel information or specific performance data should have no effect on subsequent compliance and regulatory performance. In contrast, legal scholars, behavioral economists, and policymakers regularly argue that similar seemingly inconsequential information programs can significantly influence real world outcomes.[2] Second, the Ohio signage program is an unusual transparency program, as it operates within a formal regulatory framework and serves to leverage and complement traditional monitoring and enforcement instruments rather than replace traditional monitoring and enforcement.[3] Third, the pollution outfall labeling program typifies a key pillar of the "next generation compliance" movement. The "Next Gen" initiative is a proposed regulatory strategy to use innovative enforcement tools to increase regulatory compliance at lower public cost. Although the US EPA has been a leading advocate for these tools,

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[1] See Cohen (1999); Polinsky and Shavell (2000); Shavell (2004); Gray and Shimshack (2011); Leeth (2012).

[2] See Sunstein (1999); Weil et al. (2006); Dranove and Jin (2010); and Loewenstein et al. (2014) for broad discussions of transparency policies.

[3] As discussed in detail later, in this sense our program is similar to the subset of the "name and shame / proclaim" literature where regulators publicly identify particularly good or bad regulatory performers. Nevertheless, the "disclosure as enforcement leverage" program studied in this paper remains different in that only permit data and contact information is included. Performance data is not disclosed.

the program mirrors current and proposed programs across many regulatory agencies.[4] EPA asserts one of its priority question under the environmental next gen initiative is, “whether requiring dischargers to post signs or other public notices where they discharge helps drive compliance” (Hindin 2015).

Despite the scholarly and public policy implications of mandatory outfall signage programs, we know little about their impacts. The lack of evidence stems from at least two challenges. First, these types of programs are relatively new and data are scarce. We investigate a unique yet well-documented policy implemented in June 2011 in Ohio. We combine program information with extensive facility-level Clean Water Act (CWA) pollution, compliance, and enforcement data from Ohio and several control states between 2009 and 2013. We merge in weather and zip-code level community characteristic information. Second, statistical identification can be challenging. Outfall labeling and other innovative pollution control programs may be implemented in conjunction with other policy initiatives, instigated in response to changing compliance levels, or driven by unobservable factors that may also directly influence performance and compliance. We exploit a natural experiment to isolate causal impacts.

Our naïve baseline research design is a standard difference-in-differences (DID) intent-to-treat approach using the program state (Ohio) to define treatment group and the program start date (June 2011) to define ‘pre’ and ‘post’ time periods. Intuitively, we compare changes in pollution and compliance before and after June 2011 for facilities in the treatment state after netting out changes over the same time periods for facilities in control states. Although we verify parallel trends assumptions before the policy is implemented, a possible concern is that differential longer-

[4] EPA recently proposed to reduce traditional inspections by as much as one-third, at least partially because innovative monitoring and enforcement might achieve a greater compliance “bang per buck” (USEPA 2013). See Giles (2013) and Hindin and Silberman (2016) for a discussion of environmental “next gen” features, motivations, and mechanisms.

term trends in pollution and compliance across states may be unrelated to the signage program. We therefore use a second approach that exploits a key administrative quirk of the program: sign requirements were only legally binding *at the facility-level* after Clean Water Act (CWA) discharge permits were renewed or amended. CWA permits are renewed on schedules that are plausibly exogenous from the facility perspective.[5] We use a DID strategy where treatment group is defined by program state but treatment periods are now defined by exogenous CWA permit status changes occurring after the program was in effect. Intuitively, we examine the effect of permit status changes for facilities in Ohio after the program start date, after netting out effects of permit status changes for facilities in control states after June 2011. Since a final concern is that the effects of permit status changes themselves might somehow differ between Ohio and control states, we also use a third triple difference (difference-in-difference-in-differences) approach. Our triple difference design explores the effects of permit status changes after June 2011 in Ohio, after netting out the effects of permit status changes after June 2011 in control states and after netting out the effects of permit status changes in Ohio (the treatment state itself) before June 2011 (before program implementation). To our knowledge, these latter two identification strategies are new to the regulatory enforcement and compliance literature.

Despite different sources of statistical identification, all three research designs suggest that the outfall signage program had significant and large effects on facilities' subsequent water pollution and compliance outcomes.[6] Relative to a counterfactual, treated facilities' pollution discharges fell about eight percent and their violations fell about one-third in response to the program. Results are robust to placebo tests, sensitivity explorations, and treatment definitions.

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[5] We discuss and explore the exogeneity of permit renewals in more detail later in the paper.
[6] In truth, these effects are surprisingly large relative to the authors' priors.

We believe our analysis makes at least three contributions. First, we provide the first causal assessment of the impact of mandatory outfall signage programs on pollution and compliance outcomes. High frequency data, institutional administrative quirks, and quasi-experimental methods allow us to credibly isolate the effects of information disclosure from alternative explanations. Second, we show that at least one "next generation compliance tool" can impact performance and enhance compliance at unusually low direct incremental costs. Direct implementation costs for the program appear to be less than *$600* per facility. Third, our results provide additional evidence that enforcement policy interventions impact prosocial behaviors and regulatory compliance outcomes. Although a growing literature explores the effects of enforcement in environmental, health, and safety settings, we validate and assign empirical magnitudes to a new and widely accessible instrument in the public enforcement of law toolkit.[7]

We are unable to completely and causally isolate the economic mechanisms driving our results. Nevertheless, empirical explorations suggest several broader lessons about regulatory compliance motivations. We find that signage impacts emerge quickly at the intent to treat stage, are driven by wastewater treatment plants rather than industrial facilities, and robustly appear in highly educated local areas only. Although there are other plausible explanations, we believe these results are most consistent with signage programs: (1) influencing regulated entities' expected costs and benefits of prosocial behavior via accountability to community members, watchdog groups, and local politics; (2) serving reminder and reassurance functions to decision-makers that prosocial behaviors matter and that antisocial behaviors may be detected; and (3) increasing attention to prosocial behaviors through the psychology of objective self-awareness and feelings

[7] For the literature sharing this goal, see for example Cohen (1987), Magat and Viscusi (1990), Gray and Deily (1996), Helland (1998), Stafford (2002), Earnhart (2004b), Shimshack and Ward (2005, 2008), and Telle (2013). See Gray and Shimshack (2011) for a review.

of being watched. We believe our mechanistic explorations represent a fourth paper contribution, as the literature credibly providing behavioral economic explanations for firm compliance with pollution regulation is limited.

This paper proceeds as follows. Section 2 describes the Ohio signage program in detail and reviews the state of knowledge related to "if" and "how" one might expect a signage program to influence prosocial behaviors. Section 3 begins by reviewing the context, emphasizing institutional details necessary to understand research strategies that exploit institutional quirks for identification. The remainder of Section 3 explains our data sources and Section 4 reviews our research strategies and econometric details. We present our main results in Section 5. Section 6 offers further empirical explorations, and we discuss possible interpretations for economic mechanisms in some detail. Section 7 concludes.

2. The Ohio Outfall Signage Program

Background

The Clean Water Act (CWA) requires all point sources discharging into the nation's waterways to obtain National Pollution Discharge Elimination System (NPDES) permits. These water pollution allowances are typically issued and managed by the state where the facility is located. Ohio administrative code OAC 3745-33-08, issued June 7 2011, officially revised the general conditions governing the NPDES permitting and oversight program for the state of Ohio. Requirements simply reiterated all previous requirements (previously confirmed in December of 2006), with the key exception of new provision (A)(12) that stipulated:

- All NPDES permittees shall "post and maintain permanent signs" at "each outfall."
- "The sign shall include, at a minimum, the name of the permittee, the permit number, and the outfall number identified in the permit." A contact telephone number is also required.

- “The information shall be printed in letters not less than two inches high” and “the sign shall be a minimum of two feet by two feet and the bottom of the sign shall be a minimum of three feet above the ground.”

By law and in practice, all industries were treated similarly by the signage requirements and Ohio regulators did not prioritize signage compliance for specific industries or locations.

An important feature of the program was that the new permit conditions, and thus the sign posting requirements, were not immediately binding at the facility-level after the change in administrative code in June 2011. For existing facilities, updated permit conditions only became formal requirements at the facility-level when the specific facility’s NPDES water pollution permit was legally modified or renewed after June 2011. In other words, neither Ohio EPA nor the federal EPA had any formal standing to require or enforce signage conditions until the language appeared in the plant’s NPDES permit. The practical implication was that facilities operating with continuing and effective NPDES permits after June 2011 were not required to post signs until their permit was renewed. Regulatory inspections or enforcement actions at these facilities could not address the presence, absence, or quality of signage until permit renewal. As discussed in more detail later, the specific timing of permit renewals is plausibly exogenous from the facility perspective.

Appendix 1 provides examples of typical industrial and municipal wastewater discharge points, with and without signs. In some cases, the specific points where industrial or municipal wastewater enters a river, stream, lake, reservoir, creek, or other surface water are easily associated with the source facility. However, it is common for discharge points to be distant from the source facility. Because of this distance and because outfall points are typically at the end of a pipeline or tunnel passing through private and public lands, *a typical observer would often not know the source of a given discharge,* even by sight. A typical large facility may have several outfall points.

The common disconnect between an outfall's location and its source at least partly motivated the signage program. Ohio EPA administrative documents suggest that the intended purpose of the signage program was to inform the public about sources and character of specific outfalls or discharges. The program was neither designed nor necessarily intended to address compliance concerns, but rather to use public permit identifiers and contact information to remove facility anonymity from discharge pipes, to help the public report problems, and to highlight pollution sources to those recreating or conducting third-party river surveys on the waterway.

Linking signage to pollution and compliance outcomes: Evidence from the literature

Could an outfall signage program influence pollution and compliance? If so, how? The literature provides guidance. Sunstein (1999), Weil et al. (2006), and Loewenstein et al. (2014) provide broad inter-disciplinary reviews of related scholarship. Studies paint a fairly pessimistic view of disclosure rules in settings like corporate finance, campaign finance, medical malpractice, and conflict of interest. Similarly, broad-based hazard advisories like warnings about mercury in fish and radon in homes, air quality alerts, and homeland security threat levels often affect behavior, but rarely in ways fully consistent with policy objectives. Quasi-regulatory product labeling programs like nutrition labeling and cigarette warnings, as well as quasi-regulatory performance registries like toxic releases inventories, have somewhat more favorable effectiveness records on average. Even in these settings, though, the effects of disclosure appear nuanced.[8]

[8] For support of the broad assertions in this paragraph, see the previously cited reviews as well as Hamilton (1972); Hannan et al. (1994); Hamilton (1995); Konar and Cohen (1997); Viscusi (1997); Mathios (2000); Hofstetter et al. (2002); Bui (2005); Greenstone et al. (2006); Dafny and Dranove (2008); Dranove and Jin (2010); Shimshack and Ward (2010); Bae et al. (2010); Darden and McCarthy (2015); and others.

The subset of the disclosure literature exploring “name and shame” or “name and proclaim” programs, where regulators publicly identify good or bad performers under some law, is more favorable to program effectiveness. In this context, disclosure serves to leverage and complement rather than substitute for more formal regulation. Jin and Leslie (2003, 2009) found that health regulators’ hygiene inspection grade cards in Los Angeles improved restaurant hygiene and reduced foodborne illness hospitalizations. Foulon et al. (2004) and Bennear and Olmstead (2009) found that public disclosure of poor regulatory performance significantly reduced water pollution emissions and drinking water quality violations. Evans (2016) showed that disclosure of an EPA Clean Air Watch list reduced listed facilities’ air pollution violations by roughly 10 to 20 percent.

The literature on disclosure as a complement to formal regulation perhaps suggests that a signage program could influence pollution and compliance. Yet, our program differs markedly from the name and shame literature in that no performance information is conveyed; in our context, only permit numbers and contact information is required. Further, the literature generally agrees on key features of effective transparency policies: (a) mandatory rather than voluntary disclosure, (b) simple and standardized information, (c) information harnessing communication technologies and key intermediaries, (d) salient information provided where and when decision-making occurs, and (e) information offering clear actions paths with simple and specific information on how to respond (Weil et al. 2006; Loewenstein et al. 2014). The Ohio signage program may satisfy conditions (a)-(c), but is unlikely to satisfy (d) and (e).

The ultimate policy impact of an Ohio signage program on pollution and compliance, then, seems an open empirical question. A related economic question is, ‘to the extent that signage influences pollution and compliance, how might it do so?’ First, signage might directly affect regulated entities’ expected costs and benefits of pollution and compliance, in the broad spirit of the Becker (1968)'s rational criminal actor model. Outfall signage requirements may be perceived by the regulated community as a signal that the regulator intends to increase its attention towards water pollution and compliance issues. Similarly, activist and community pressure plays a significant role in agents' prosocial behavior and regulatory compliance choices (e.g. Earnhart 2004a; Eesley and Lenox 2006; Innes and Sam 2008; Kitzmueller and Shimshack 2012).[9] Signage might be especially likely to influence citizen complaints and activist actions, and the evidence suggests citizen watchdog groups are especially active and influential in water pollution contexts (Grant and Grooms 2012; Langpap and Shimshack 2010).

Second, signage may serve to leverage one or more economic psychology mechanisms influencing individual or organizational decision-making.[10] Signage may serve reminder and reassurance functions, prompting decision-makers to believe that prosocial behaviors "matter" and that antisocial behaviors may be detected (Thornton et al. 2005; Hindin and Silberman 2016). Sustained benefits from a hospital handwashing signage program were attributed to posters reminding workers that handwashing was an organizational commitment with important benefits for employees, patients, and the broader community (Pittet et al. 2000). Reminder and reassurance functions may operate overtly or subconsciously or both, and the underlying salience issues may be driven by infrequent shocks or repeated cues or both (Lowry and Joslyn 2014).

Similarly, signage may increase attention to standards or prosocial behaviors through the psychology of objective self-awareness (Duval and Wicklund 1973; Wicklund 1975; Mazar et al. 2008). Subtle cues of being watched, like images of eyes, significantly increase prosocial

[9] Investor or employee pressure on management may also influence prosocial behaviors, but the evidence here is less compelling (Kitzmueller and Shimshack 2012).

[10] Although the closely related economic psychology literature has often emphasized individual choices, individual-level and group-level behavioral realties have marked impacts on managerial and organizational decision-making outcomes as well (Bazerman and Moore 2008).

behaviors in laboratory and in real-world settings (Hayley and Fessler 2005; Bateson et al. 2006). The idea is that increased perceptions of being watched may be cognitively salient even when understated in practice. Antisocial behaviors may threaten the decision-makers' external prosocial reputations or self-conceptions as an honest individuals or organizations (Mazar et al. 2008). For example, signage increases payments in honor-based payment systems, with the likely mechanism that signs serve as moral reminders reducing the possibility that actors are able to deceive themselves that they are "doing the right thing" while simultaneously committing socially harmful acts (Pruckner and Sausgruber 2013).

3. Context and Data

Clean Water Act Regulation and Permitting

In order to understand our evaluation of Ohio's mandatory signage program, it useful to briefly review key features of its Clean Water Act setting.[11] Congress drafts major environmental legislation like the CWA, and these major national statutes dictate overall regulatory structure. Nevertheless, the overwhelming majority of permitting, inspection, and enforcement activities under the CWA (and most other statutes) are delegated to states or more local authorities. When states or local agencies are given primary oversight responsibility, dubbed "primacy," they control essentially all day-to-day oversight. Primary authorities provide activity metrics to federal and regional EPA offices, and these regional and federal agencies review state operations and may conduct their own monitoring, enforcement, and permitting activities when local agencies are perceived as insufficiently rigorous. Federal EPA technically retains the right to revoke state or local primacy, but such revocations have not occurred under the Clean Water Act.[12] The key point

[11] See Shimshack (2014) for a more detailed review of institutions governing pollution monitoring and enforcement.
[12] In rare cases, states have declined or returned primacy, typically for resource or political economy reasons.

is that states are the practical regulatory unit, but regional and national offices can provide modest oversight pressure.

All point sources discharging into the nation's waterways are required to obtain CWA permits.[13] Section 402 of the Act authorizes the National Pollution Discharge Elimination System (NPDES) program, which governs the permitting and regulatory processes for industrial and municipal water polluters. Permits are issued by the authority with primacy (i.e. henceforth "state," as states are the authority in our empirical application). Permits set contaminant-specific numeric average and maximum limits, which may or may not vary by season. Such limits may be expressed as concentrations or quantities per unit time, although both quantities and concentrations are designed to produce similar compliance implications. Many permits include both quantity and concentration limits. Time periods used for compliance determination vary across pollutants and permits, but the most common and the most enforcement-relevant time period for conventional water pollution is the month.[14] Numerical pollution discharge limits are primarily based on facility design size, industry, and technology, although biophysical and water quality conditions of the receiving waters may play a role for limits in some circumstances.

According to statutes, NPDES permits are intended to be issued and renewed on fixed five year review cycles. Within a given permit cycle, pollution limits remain constant. In historical practice, most pollution limits remain very similar from permit to permit for a given contaminant. It is useful to note that, from the perspective of the plant at any given time, the timing of the typical NPDES permit cycle is exogenous. It is reasonably common in practice for NPDES permit renewals to take longer than the scheduled five years due to administrative backlogs, resource

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[13] See USEPA (2012) for an overview of the NPDES program.

[14] For example, a facility may be required to meet limits such as "a 30-day average of total suspended solids must not exceed 30 milligrams per liter", or "any 7-day average over a given 30-day period of total suspended solids must not exceed 60 milligrams per liter."

constraints, and personnel time. Facilities making timely application, however, have their permits automatically continued or interim permits issued under 'administrative continuation' legal provisions. In these cases, the exact timing of the NPDES permit cycle may be shorter or longer than five years (depending on whether the extension is handled as the same permit or an interim permit), but the ultimate timing of the permit status change is not a facility decision and not influenced by facility behavior. In rare cases, permit renewals may be triggered ahead of the five year schedule by substantial modifications to the facility or substantial concerns about the quality of the receiving waters. Also infrequently, permits may be held up at the typical 5-year renewable stage due to an ongoing enforcement concern.[15]

Compliance with the terms of NPDES permits is overseen through a traditional monitoring and enforcement process. The primary form of pollution monitoring for large facilities regulated under the CWA is self-reporting. Major facilities are required to submit discharge monitoring reports to regulators each and every month. Researchers and policy-makers typically assume that self-reported discharges are first-order accurate.[16] On-site regulator inspections help confirm the accuracy of these self-reports and the self-reporting process, and these inspections verify appropriate maintenance and operation of abatement equipment. Regulator evaluations may or may not involve regulator sampling of effluent. Violations, either self-disclosed or uncovered during regulator inspections, are subject to enforcement actions ranging from phone calls and warning letters to formal administrative sanctions.[17]

[15] These very rare cases of endogenous permit cycles could, at least in theory, pose a threat to some but not all of our later empirical strategies. We provide evidence that these cases are unlikely to influence results.

[16] We discuss and validate this assertion in later robustness sections.

[17] As a general rule, CWA guidelines stipulate that penalty frequency and severity is a function of gain, harm, ability to pay, compliance history, fairness, intent, and the strength of the legal evidence. In practice, however, authorities more often pursue the minimum sanction thought to achieve a return to compliance and some longer-term deterrence objective (Shimshack 2014). Thus, although more severe penalties are available, formal sanctions almost always to take the form of administrative sanctions. The sanctions are typically issued by administrative law judges and are increasingly accompanied by monetary penalties. Although severe violations may be referred to state attorneys

3b. Data

Our specific data sources are the Environmental Protection Agency's Integrated Compliance Information System and the Permit Compliance System. These databases track monthly facility-level self-reported discharges, permitted pollution limitations, permit information, facility characteristics, inspections, and enforcement actions under the Clean Water Act (CWA). Our analysis period is the 60 months spanning January 2009 to December 2013, i.e. two and one-half years before the signage program implementation date and two and one-half years following the program signage implementation date.

We focus on large facilities in Ohio, the treatment state, and in the five other states that make up EPA region 5 (IL, IN, MI, MN, WI). Appendix 2 provides a map of EPA regions. Although states have the primary responsibility for monitoring, enforcing, and overseeing pollution and compliance under the Clean Water Act, the region 5 sample restriction ensures the most comparable umbrella EPA oversight pressures between treatment and control facilities. We consider large "CWA majors" only, i.e. those entities that discharge more than 1 million gallons of wastewater or more per day or have unusual potential to generate harm. Major facilities are required to report pollution and compliance outcomes each and every month; in contrast, smaller facilities are typically not required to report monthly and the states are not necessarily required to input their data regularly into central EPA databases.

We focus on pollution and compliance outcomes for the conventional water pollutants biochemical oxygen demand (BOD) (EPA parameters 00310 or 80082) and total suspended solids (TSS) (EPA parameter 00530). BOD and TSS are the water pollutants most consistently measured,

general, civil authorities, or the Department of Justice for civil or criminal prosecutions, such referrals are extremely rare for non-catastrophic CWA discharges. For example, criminal sanctions are reserved only for intentionally operating without permit, intentionally falsifying records, and violations with truly extraordinary harm (i.e. Exxon Valdez, Deepwater Horizon). See Uhlmann (2009).

tracked, and reported across a large number of industries and facilities. BOD and TSS discharges are also correlated with other conventional pollutants, toxics, and other contaminants and therefore serve as indicator pollutants. Violations are coded as 1 if discharges for BOD or TSS or both exceed permitted allowable effluent limitations. For analysis purposes, following the literature, we scale pollution numerical discharges to obtain ratios of actual to permitted discharges, which can be thought of as discharges as a percent of the standard. Since some plants may have multiple outfalls, our final plant-level unit of observation is the maximum discharge ratio for each pollutant across outfalls.[18]

We augment pollution, compliance, permitting, and enforcement data with socio-demographic, political economy, and weather variables at the zip code-level. We extract socio-economic and political economy variables from the 2000 census, as provided by the Longitudinal Tract Database. We focus on zip-code level characteristics since facilities may impact communities beyond their immediate neighborhood. We compute characteristics as spatially-weighted means of individual census tract data following Logan et al. (2012). We also merge in spatially-weighted temperature and precipitation data at the zip-code level from the US Historical Climatology Network. Water pollution is often influenced by variability in temperature and especially precipitation, as outdoor abatement activities and technologies are influenced by the volume of wastewater, temperature, acidity, the number and composition of microorganisms, light, nutrients, and other factors. Details of the weather data construction are provided in Appendix 3.

Despite exceptionally rich pollution, compliance, permitting, enforcement, and program institutions data, we do not directly observe sign installations. It is our understanding that signage

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

compliance was essentially complete. Confirming signage is a simple and transparent part of regulator inspections, and conversations with Ohio regulators assured us that signage was actually regularly verified in practice. To our knowledge, no enforcement actions were issued for noncompliance with signage requirements, despite regulator assurances that enforcement would have been imposed for missing or noncompliance signs.

3c. Final sample

Our sample consists of 978 major facilities across all six region 5 states, as shown in Table 1. 236 sample facilities are located in Ohio and 742 sample facilities are located in control states. Wastewater treatment (SIC code 4952) is the most common industry, and roughly 80 percent of facilities are wastewater treatment plants. Other common industries include pulp, paper, inorganic chemicals, organic chemicals, petroleum refining, and steel. Together, the industries in our sample generate the overwhelming majority of point source water pollution in the U.S.

Table 2 provides several summary statistics for all sample facilities, for Ohio facilities, and for control state facilities. In an average month, around 2 percent of facilities were in violation for BOD or TSS. 411 facilities violated their monthly BOD or TSS standards 1,286 times during our sample period. The average violation was more than two times the permitted limit, and dozens of violations were more than 10 times permitted limits. Mean discharges for TSS pollution were about 33 percent of limits and the 25th and 95th percentiles were approximately 15 and 78 percent of the limits, respectively. Mean discharges for BOD pollution were about 27 percent of limits and the 25th and 95th percentiles were approximately 13 and 65 percent of the limits, respectively. Both BOD and TSS pollution discharges were very variable, across facilities and across time for the average facility. Table 2 also illustrates that, in any given month, about 12 percent of facilities received a regulator inspection and about 1 percent of facilities received a monetary penalty.

Relative to national average, our facilities were located in zip codes where unemployment was relatively high, per capita income and educational attainment was relatively low, and manufacturing as a share of employment was relatively high.

Notably, Table 2 indicates that Ohio facilities and other region five facilities were relatively comparable during our sample period. None of the summary statistics in the Ohio column (2) are statistically different from the summary statistics in the rest-of-region-five column (3). This suggests that treatment and controls defined by state exhibit reasonable balance on observables.

4. Methods

Our goal is to assess the causal impact of the Ohio signage program on pollution and compliance. Since we have a panel dataset, is it perhaps tempting to simply compare Ohio facilities' pre-program outcomes with Ohio facilities' post-program outcomes. However, we cannot necessarily attribute changes in pollution and compliance after the signage requirement to the program alone. For example, outfall labeling may be implemented in conjunction with other policy initiatives, instigated in response to changing compliance levels, or driven by unobservable factors that may also directly influence pollution and compliance. One natural way to isolate casual effects of the signage program would be to examine outcome differences over time between an experimental group of facilities randomly assigned to display signs and a control group of facilities randomly assigned to a 'no signage' condition. While this is not possible ex-post, our quasi-experimental methods mimic this general structure.

Our baseline research design is a standard "intent to treat" difference-in-difference strategy. We define treatment group by location in Ohio, the program state. The control group contains facilities in the other EPA region 5 states. We define treatment time periods by the program implementation date of June 2011. Pre-treatment periods are those before June 2011 and post-treatment periods are June 2011 or later. The standard intuition and identifying assumptions apply. Conditional on the covariates, we assume that before vs. after changes in pollution and compliance in control states represent expected changes for outcomes in the treatment state (Ohio), had the treatment not occurred.

Intuitively, our baseline methods explore how outcomes vary before vs. after June 2011 for facilities in Ohio, after netting out how outcomes vary before vs. after June 2011 for facilities in control states. More formally, for facility *i* in month *t* of season *s* and year *y*, our baseline specification is:

$$(1) \qquad y_{it} = \alpha_i + \beta_1 POST_t + \beta_2 (POST_t \times OHIO_i) + X_{it}\Gamma + \lambda_y + \mu_s + \varepsilon_{it} \,,$$

where y_{it} is pollution discharges at facility *i* month *t* or an indicator function for a violation at facility *i* in month *t*, $1[violation_{it}]$. Facility-level fixed effects α_i capture all relatively time invariant facility characteristics including industry, subindustry, production capacity, geography, and local area socio-demographic and political economic factors. Facility fixed effects also allow differences in time invariant pollution and compliance outcomes across states, i.e. α_i subsumes a treatment group dummy ($OHIO_i$). $POST_t$ indicates post-treatment periods. X_{it} is a vector of controls including weather variables and lagged monitoring and enforcement activities actions.[19] λ_y and μ_s are year and season dummies. The two coefficients of interests are β_1 and β_2; β_1 captures before vs. after June 2011 changes in control states and β_2 is program effect (i.e. before vs. after changes in the treatment state after netting out before vs. after changes in the control states).

One natural concern with the standard intent to treat difference-in-differences approach is that factors unrelated to the treatment may have caused differential changes over time between

[19] Lagged enforcement and monitoring and variables may be endogenous in the presence of serial correlation, but they are included as controls and we are not interested in their causal interpretation.

facilities in the program state and facilities in control states. To address this concern, we use additional research designs that exploit a key administrative quirk of the program: sign requirements were only legally binding at the facility-level after Clean Water Act (CWA) discharge permits were renewed or amended. CWA permits are renewed on schedules that are plausibly exogenous from the facility perspective.[20] Thus, our second approach to treatment effect identification replicates the former analysis but now requires permits status changes to trigger treatment status.

Intuitively, we explore how outcomes vary before vs. after post-June 2011 permit changes for facilities in Ohio, after netting out how outcomes vary before vs. after post-June 2011 permit status changes for facilities in control states. More formally, for facility *i* in month *t* of season *s* and year *y*, our baseline specification is:

$$(2)\ y_{it} = \alpha_i + \delta_1 PERMIT\Delta\_POST_{it} + \delta_2 (PERMIT\Delta\_POST_{it} \times OHIO_i) + X_{it}\Gamma + \lambda_y + \theta POST_t + \mu_s + \varepsilon_{it}\,,$$

where $PERMIT\Delta\_POST_{it}$ indicates a permit status change at facility i in some period t after June 2011. Other variables are defined as above.

In the approach represented by (2), it is no longer a threat to identification if pollution and compliance outcomes trend differently before vs. after June 2011 between Ohio and control states. However, identifying assumptions may still be violated if the effect of permit renewals post June 2011 somehow differs between Ohio and control states. To address this latter concern, we use a third research design that attempts to net out the effects of permit renewals absent the program. This approach has the advantage of exploiting changes over time in control states as well as pre-treatment changes in the treatment state itself to construct counterfactuals. The disadvantage is that

[20] We discuss and explore the exogeneity of permit renewals elsewhere in the paper, and we explore possible violations of the exogeneity assertion in sensitivity analysis.

this approach asks a lot of the data.

Intuitively, we explore how outcomes vary before vs. after a permit status change post June 2011 for facilities in Ohio, after netting out how outcomes vary before vs. after a permit status change post June 2011 for facilities in control states, and after netting out how outcomes vary before vs. after a permit status change prior to June 2011 in Ohio itself. More formally, for facility *i* in month *t* of season *s* and year *y*, our baseline specification is:

(3)
$$\begin{aligned} y_{it} = \alpha_i + \phi_1 POST_t + \phi_2 (POST_t \times OHIO_i) + \phi_3 PERMIT\Delta_{it} + \phi_4 (PERMIT\Delta_{it} \times OHIO_i) + \\ \phi_5 PERMIT\Delta\_POST_{it} + \phi_6 (PERMIT\Delta\_POST_{it} \times OHIO_i) + X_{it}\Gamma + \lambda_y + \mu_s + \varepsilon_{it} \end{aligned} ,$$

where $PERMIT\Delta_{it}$ represents a permit status change at facility *i* in any period *t*. Other variables are defined as above. ϕ_6 is the coefficient of interest, and represents the difference-in-difference-in-differences estimate when treatments are jointly defined by Ohio, post-implementation period, and permit status change.

In analyses that follow, we cluster standard errors to allow for arbitrary correlations at the facility-level. Although addressing the Bertrand et al. (2004) concerns with difference-in-difference estimators ideally involves clustering at the state-level, we have data from only six states and thus a small clusters problem. We are in the process of implementing the Cameron, Gelbach, and Miller (2008) wild bootstrap approach that allows us to overcome the small clusters problem while clustering at the state-level. This work is not yet complete. Reassuringly, however, patterns of statistical significance remain identical in all finished investigations.

5. Results

Baseline DID. Table 3 presents our baseline results. Results in the first row suggest that violations and pollution change little or may modestly decline over time in control states. The difference-in-difference coefficients in row two are all negative and significant at conventional levels for conventional pollution violations and for the pollutant BOD. Interpreting column (1)-

(3) results, we find that, relative to a counterfactual, violations for conventional water pollutants BOD and TSS fell roughly 1.2 to 1.5 percentage points in Ohio after June 2011. These violation results are practically large; this effects is roughly one-third of baseline violation levels. Interpreting columns (4)-(6) and (7)-(9), we find that BOD discharges fell around 1.6 percentage points (6 percent of baseline levels) and TSS discharges fell around 3.0 to 5.3 percentage points (10 percent of baseline levels) after June 2011 in Ohio relative to a counterfactual.

Heterogeneity. Table 3 results appear to be driven by (largely municipal) wastewater treatment plants in high education areas only. Table 4 replicates our baseline DID regressions for industrial facilities and for wastewater treatment plants. Columns (1) – (3) indicate that pollution and violations for Ohio industrial facilities remained unchanged relative to a counterfactual after June 2011. In contrast, columns (4) – (6) show that, relative to a counterfactual, wastewater treatment plants' violations for conventional water pollutants fell roughly 1.8 percentage points (around half of baseline levels), BOD discharges fell around 1.8 percentage points (7 percent of baseline levels), and TSS discharges fell around 4.2 percentage points (12 percent of baseline levels) after June 2011 in Ohio relative to a counterfactual.

Table 5 replicates our baseline DID regressions but now includes interactions with local area socio-demographic variables. Column (1) row (2) indicates that water pollution violations remain relatively unchanged relative to a counterfactual after June 2011 in below median education areas of Ohio. In contrast, column (1) row (4) suggests that violations for conventional water pollutants BOD and TSS fell markedly in high education areas of Ohio after June 2011. Magnitudes are consistent with Table 3 results. Columns (2) – (4) suggest that income, race, and percent employment in manufacturing did not influence compliance changes after June 2011 in Ohio relative to a counterfactual. Baseline diff-in-diff coefficients in row 2 are still significant with magnitudes similar to comparable estimates in Table 3, but interaction effects in row 4 are insignificant and generally small.

DID with treatments defined by post-June 2011 permit renewals. A natural concern with the results in Tables 3-5 is that perhaps factors unrelated to Ohio's signage program caused differential changes over time between Ohio and other Region 5 facilities. We therefore replicated the baseline analyses but now define treatment by the effects of exogenous permit status changes after June 2011. Table 6 presents results. Results in the first row again suggest that violation and pollution perhaps decline modestly over time in control states. Results in the second row suggest that violations may increase after permit renewals post-June 2011 in control states and pollution remains relatively unchanged on average. Key program effects in row 3 remain extremely similar to results in Tables 3 and 4. Relative to a counterfactual, violations fell roughly 1.5 to 2.5 percentage points in Ohio after June 2011. Relative to a counterfactual, BOD and TSS discharges fell around 2 to 4 percentage points (5-10 percent of baseline levels) in Ohio after June 2011.

Triple differences. One remaining concern with the preceding analyses is that perhaps the effects of permit renewals after June 2011 somehow differ between Ohio and control states. We therefore replicated our analyses with a triple difference approach where treatment is simultaneously defined by state, permit status change, and post-June 2011. Table 7 presents the results. Results in row 2 confirm earlier insights; conventional water pollution violations and discharges fall markedly in Ohio after June 2011 relative to a counterfactual. Results in row 3 suggest that permit renewals do not systematically affect pollution in control states before June 2011 on average. Results in row 5 suggest that permit renewals after June 2011 may have modestly increased pollution violations and TSS discharges in control states.

Results in Table 7 row 6 are the diff-in-diff-in-diff coefficients. Estimates are typically noisy and the first three columns show no results in the full sample, as expected from earlier investigations. Column 5, however, indicates that permit renewals in Ohio after June 2011 were associated with reductions in BOD pollution of similar magnitude as discussed in Tables 3-6. These results are identified after sweeping out the effects of permit renewals after June 2011 in control states and after sweeping out the effects of permit renewals in Ohio itself before June 2011. Conventional water pollution violations and TSS pollution, however, appear unchanged relative to this demanding counterfactual. We are working to understand these results in more detail, but one explanation is that the program's main effects on violations occurred at the intent to treat stage (captured by row 2 coefficients).

5b. Robustness

Program treatment effects are generally robust to three different research designs, each with a different source of identification. Nevertheless, we consider several robustness issues. First, we explore the standard parallel trends identifying assumption. We statistically compare trends in pollution and compliance between the treatment state (OH) and control states (IL, IN, MN, WI, MI) for periods preceding June 2011, and we are unable to reject the equivalence of pre-treatment trends in outcomes. Second, and towards similar ends, we perform a falsification exercise where we replicate our analysis after moving the signage data ahead in time by precisely one year or two years. If unobserved facility-level confounders, seasonality, or longer-term trends drive results, this sensitivity check would likely produce false program effects because the simulated signage requirements would occur in the same places, start during the same time of year, and take effect during an even earlier part of the sample period relative to the actual requirements. Reassuringly, we do not find evidence of reduced pollution or enhanced compliance after simulated programs; we find these effects only after actual program implementation.

Second, we explored the possibility that confounding policies or programs may have been implemented in the treatment state around or after June 2011 but not in control states. We spoke with dozens of state and federal EPA personnel, and we were unable to identify significant policy changes in Ohio but not neighboring states. Ohio's high profile electronic discharge report system mandate was imposed and (at least close to) fully implemented before the start of our sample period (Gray et al. 2012). Ohio did change its wastewater treatment 'plant operator of record' rules during our sample period, but these changes are extremely unlikely to impact major facilities since they are about minimum staffing levels nearly always exceeded by large facilities. Moreover, neither of the above initiatives were associated with permit renewals after June 2011. Conversations with regulators did suggest that Ohio made small changes to its enforcement priorities beginning in 2012, but we observe and directly control for monitoring and enforcement pressures in our analysis.

Third, we investigated the possibility that the nature and type permit status changes may have been meaningfully different across states and/or across time within Ohio. Although we have limited information on the exact content of the permits, we do observe the crucial pollution limits directly. Pollution limits do not change with the vast majority of permit renewals or reissues. We find no significant difference in the size and direction of pollution limit changes between Ohio and control states. We find no significant difference in the size and direction of pollution limit changes in Ohio before June 2011 relative to limit changes in Ohio after June 2011.

5c. Self-reporting

To be fully precise, our results indicate that the mandatory outfall signage program impacted *self-reported* pollution and compliance outcomes. These results are unambiguously consistent with the signage program meaningfully affecting facilities' incentives regarding pollution and compliance outcomes. However, a natural question is whether we are detecting actual reductions in pollution and violations or whether we are detecting greater misreporting about pollution and violations. More generally, a natural question with all self-reported data is whether plants strategically misreport discharges.

We believe systematic misreporting is unlikely in our context for institutional reasons. Theory suggests that well-designed self-reporting regimes will be incentive compatible if penalties for intentional misreporting are large relative to penalties for act-based violations, and if penalties for intentional misreporting are borne by both principles and agents (Cohen 1992, Kaplow and Shavell 1994). These conditions are met in the Clean Water Act context. Sanctions for intentional misreporting are severe, and may include incarceration for both employees and managers (Uhlmann 2009). In contrast, penalties for typical violations of permitted pollution limits are relatively modest and do not involve incarceration (Shimshack 2014). Moreover, independent government reviews and a growing empirical literature fail to reject the accuracy of major industrial facilities' CWA self-reports (U.S. EPA 1999; Laplante and Rilstone 1996; Shimshack and Ward 2005; Chakraborti and Shimshack 2012).

Nevertheless, we explore the issues of self-reporting empirically. The ideal test of strategic misreporting would compare self-reported discharges to objectively measured actual discharges. Unfortunately, not even CWA regulators conduct such direct checks. However, following Laplante and Rilstone (1996), Shimshack and Ward (2005), and Chakraborti and Shimshack (2012), it seems reasonable to suspect that plants report more accurately in the presence of a regulatory inspector. If plants underreport in the absence of an inspector, but report accurately in the presence of an inspector, then one might expect a positive correlation between reported pollution and contemporaneous inspections (after controlling for other pollution determinants and regulatory targeting factors). We regressed our pollution measures on contemporaneous inspections and the full slate of explanatory variables including lagged enforcement, and we did not find the hypothesized positive relationship between reported pollution and contemporaneous inspections. Violation equation point estimates were small and t-statistics were below 0.5. Pollution equation point estimates were negative and insignificant.

A final concern is that perhaps strategic misreporting occurs only when plants perceive their costs of reported violations are unusually harsh. The concern most germane to our paper is that incentives to misreport become unusually great once the program takes effect. Thus, we reinvestigated the relationship between reported pollution and contemporaneous inspections, as above, but only for Ohio and only after June 2011. We continued to find no statistically positive difference between reported pollution when an inspector was present and when an inspector was absent. Violation equation point estimates were small and t-statistics remained below 0.5. Pollution equation point estimates were negative and insignificant.

6. Discussion

To our knowledge, this is the first paper to analyze the impact of mandatory pollution point source signage programs on pollution and compliance. We develop and implement novel empirical research designs that may help isolate program effects in compliance settings with regulatory permitting. We find that signage induced statistically significant and large reductions in conventional water pollution and violations. In particular, we find that total suspended solids (TSS) and biochemical oxygen demand (BOD) discharges fell five to 10 percent on average and conventional water pollution violations fell more than 30 percent in response to the program.

These results are perhaps surprising in light of the existing literature on information disclosure. The Ohio signage program provided no performance information; only permitting and contact information are provided on the signs. Yet, the effects of the signage program on pollution and compliance appeared immediately and strongly at the intent-to-treat stage. Heterogeneity, however, was pronounced. We found program effects for (municipally-owned) wastewater treatment plants only. We found program impacts more consistently for BOD discharges rather than for TSS discharges, perhaps reinforcing of the wastewater treatment plant result since BOD is more variable and more binding in regulation that TSS for wastewater plants (Shimshack and Ward 2008). We found program effects in high education communities only.

Although we are unable to precisely and full isolate causal mechanisms, we believe our main analyses and heterogeneity explorations are most consistent with the signage program: (1) Impacting regulated entities' expected costs and benefits via accountability to community members, watchdog groups, and local politics; (2) Serving reminder and reassurance functions to decision-makers that prosocial behaviors matter and antisocial behavior may be detected; and (3) Increasing attention to prosocial behaviors through the psychology of objective self-awareness and feelings of being watched.

Assessing the full welfare effects of the Ohio signage program is beyond the scope of this study. For perspective, however, (and with facility-level compliance costs notwithstanding) the direct incremental costs of the signage program are extremely low. A back of the envelope calculation estimates those costs are roughly $600 or less per facility.[21] Yet, our retrospective

[21] Since signs are intended to be durable outdoors, visibly salient, and at least two feet by two feet in size, one might assume that materials, fabrication, and installation costs are similar to common roadway speed limit signs. We thus

analysis suggest that discharge signage programs has measurable and meaningful effects on pollution and compliance. As such, a natural policy lesson from a regulator's perspective is that signage programs have the potential to generate an unusually large average regulatory "bang per buck."[22] In the CWA pollution context alone, if the many states not pursuing outfall signage programs were to do so, the social benefits would be potentially large.

Program benefits are perhaps surprising, and especially given that the signage mandate is far from the ideal "textbook" transparency program. Although the program involves mandatory disclosure, simple and standardized information, and possibilities to leverage technology and intermediaries, a more effective program might involve more salient information, information provision where and when targeted audiences make decisions, simple and specific information on how to respond to the disclosure, and especially a clear and concise action path from disclosure to regulatory outcomes of interest. Another cautionary policy note is that pronounced heterogeneity results suggesting greater program effectiveness in high education areas points towards a previously unexplored channel for environmental policy to generate yet more disparities across subpopulations. Complementary interventions may be necessary in low education / low socioeconomic status areas.

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assume that outfall signs cost $200 each. The average facility in our dataset has three or fewer discharge points, so we conservatively assume $600 in signage costs.

[22] In this sense, our results are in the spirit of Jin and Leslie (2003, 2009) and Evans (2015).

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Table 1 Facility Distribution by States

States	Number of major facilities	Observations
Control facilities		
Illinois	258	15,480
Indiana	166	9,960
Michigan	130	7,800
Minnesota	70	4,200
Wisconsin	118	7,080
Treatment facilities		
Ohio	236	14,160
Total facilities	978	58,680

Note: This table presents facility distribution for EPA region 5 NPDES major wastewater permittees.

Table 2 Ohio facilities vs. other Region 5 facilities

	Region 5	OH	IL, IN, MI, MN, WI
2-digit SIC = 49xx	82%	77%	82%
BOD or TSS violation	0.021	0.028	0.019
TSS (relative to limit)	0.33	0.34	0.33
BOD (relative to limit)	0.27	0.28	0.27
Inspection this month	0.12	0.09	0.12
Penalty this month	0.01	0.01	0.01
Unemployment	9.41	9.84	9.25
Per Capita Income	25466	24089	25964
% people Bachelor+	22.4	19.5	23.4
% people in manufacturing	17.2	17.7	17.0
% renter occupied	27.4	29.0	27.8
% white	88.4	89.2	88.1

Table 3: Impact of Signage, Using Program Start Date to Define Treatments

	Violation Dummy			BOD Ratio			TSS Ratio		
VARIABLES	1	2	3	4	5	6	7	8	9
PostJune2011	-0.008***	-0.005	-0.002	-0.016***	-0.006	-0.003	-0.029	-0.071	0.005
	(0.002)	(0.003)	(0.003)	(0.004)	(0.006)	(0.006)	(0.017)	(0.061)	(0.018)
PostJune2011 x OH	-0.012***	-0.012***	-0.015***	-0.015*	-0.015*	-0.017**	-0.030	-0.030	-0.053***
	(0.006)	(0.006)	(0.006)	(0.008)	(0.008)	(0.008)	(0.023)	(0.023)	(0.019)
Facility FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Season FEs	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Other Controls	No	No	Yes	No	No	Yes	No	No	Yes
Facilities	978	978	973	797	797	797	964	964	964
Observations	58,530	58,530	58,234	47,558	47,558	47,558	57,580	57,580	57,580

Notes: PostJune2011 equals to 1 if the month is after June 2011. Other controls are regulatory deterrence and weather variables described in Table 2. Linear probability model is used for violation dummy equation. Clustered standard errors in parentheses. Asterisks denote statistical significance: *** p<0.01, ** p<0.05, * p<0.1

Table 4: Diff-in-diff Impact of Signage, Across Sectors

	Industrial Facilities			Wastewater Treatment Plants		
VARIABLES	Viol	BOD	TSS	Viol	BOD	TSS
PostJune2011	-0.002	-0.009	0.051	-0.002	-0.001	-0.008
	(0.006)	(0.022)	(0.069)	(0.004)	(0.007)	(0.022)
PostJune2011 x OH	-0.005	-0.023	-0.085	-0.018**	-0.018**	-0.042**
	(0.008)	(0.027)	(0.053)	(0.007)	(0.009)	(0.007)
Facility FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	No	Yes	Yes	No	Yes	Yes
Season FEs	No	Yes	Yes	No	Yes	Yes
Other Controls	No	No	Yes	No	No	Yes
Facilities	213	102	212	753	684	740
Observations	12,752	6,083	12,651	45,062	40,822	44,214

Notes: PostJune2011 equals to 1 if the month is after June 2011. Other controls are regulatory deterrence and weather variables described in Table 2. Linear probability model is used for violation dummy equation. Clustered standard errors in parentheses. Asterisks denote statistical significance: *** p<0.01, ** p<0.05, * p<0.1

Table 5: Diff-in-diff Impact of Signage, Demographic Interactions

	DEP VAR: Violations			
VARIABLES	Education	Income	Race	Manuf.
Post	-0.005	-0.001	0.001	0.001
	(0.004)	(0.004)	(0.004)	(0.004)
Post x OH	-0.005	-0.015*	-0.022***	-0.021**
	(0.008)	(0.008)	(0.008)	(0.009)
Post x Hi Education	0.007			
	(0.004)			
Post x Hi Education x OH	-0.020*			
	(0.011)			
Post x Hi Income		0.003		
		(0.004)		
Post x Hi Income x OH		0.001		
		(0.011)		
Post x Hi White			-0.004	
			(0.004)	
Post x Hi White x OH			0.015	
			(0.011)	
Post x Hi Manufacturing				-0.005
				(0.004)
Post x Hi Manufacturing x OH				0.012
				(0.011)
Facility FEs	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes
Season FEs	Yes	Yes	Yes	Yes
Other Controls	Yes	Yes	Yes	Yes
Facilities	973	973	973	973
Observations	58,234	58,234	58,234	58,234

Notes: Post equals to 1 if the month is after June 2011. Other controls are regulatory deterrence and weather variables described in Table 2. Linear probability model is used for violation dummy equation. Clustered standard errors in parentheses. Asterisks denote statistical significance: *** p<0.01, ** p<0.05, * p<0.1

Table 6: Diff-in-diff Impact of Signage, Using Post-Program Permit Renewals to Define Treatments

	Full sample			Wastewater Plants in Hi Educ Areas		
VARIABLES	Viol	BOD	TSS	Viol	BOD	TSS
Post	-0.006*	-0.006	-0.005	-0.005	0.004	-0.014
	(0.003)	(0.006)	(0.017)	(0.005)	(0.008)	(0.011)
Post-Renewal	0.012***	0.008	-0.001	0.016***	0.009	0.029*
	(0.003)	(0.007)	(0.018)	(0.006)	(0.010)	(0.015)
Post-Renewal x OH	-0.015**	-0.020**	-0.032**	-0.025***	-0.032**	-0.044**
	(0.006)	(0.009)	(0.016)	(0.009)	(0.015)	(0.021)
Facility FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Season FEs	Yes	Yes	Yes	Yes	Yes	Yes
Other Controls	Yes	Yes	Yes	Yes	Yes	Yes
Facilities	973	793	959	412	372	403
Observations	58,234	47,324	57,285	24,650	22,188	24,072

Notes: Post equals to 1 if the month is after June 2011. Other controls are regulatory deterrence and weather variables described in Table 2. Linear probability model is used for violation dummy equation. Clustered standard errors in parentheses. Asterisks denote statistical significance: *** p<0.01, ** p<0.05, * p<0.1

Table 7: Diff-in-diff-in-diff impact of signage

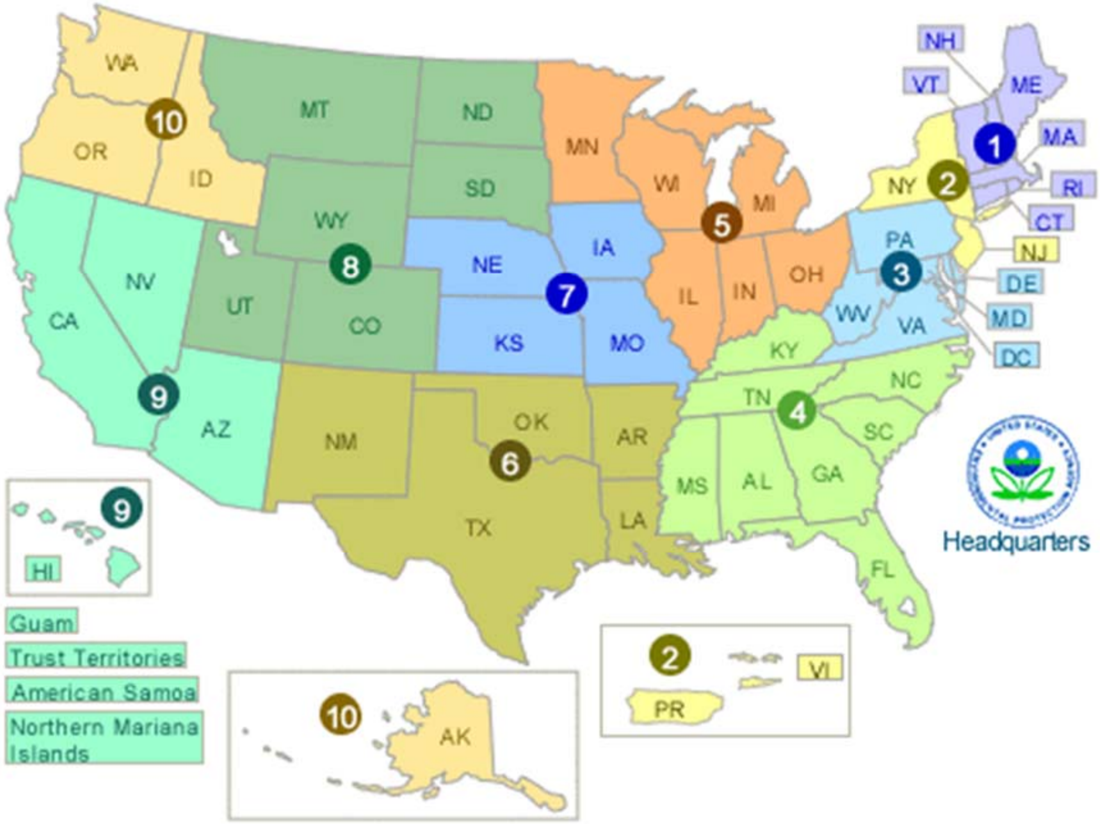
	Full sample			Wastewater Plants in Hi Educ Areas		
VARIABLES	Viol	BOD	TSS	Viol	BOD	TSS
Post	-0.003	-0.002	0.007	0.001	0.007	0.003
	(0.003)	(0.005)	(0.18)	(0.004)	(0.003)	(0.010)
Post x OH	-0.016***	-0.019***	-0.062***	-0.025***	-0.017**	-0.084***
	(0.004)	(0.006)	(0.022)	(0.005)	(0.004)	(0.013)
Any Renewal	-0.001	0.002	-0.017	-0.005	-0.004	-0.014
	(0.003)	(0.004)	(0.015)	(0.004)	(0.003)	(0.009)
Any Renewal x OH	-0.001	0.020***	0.007	-0.012**	0.001	-0.011
	(0.004)	(0.007)	(0.027)	(0.006)	(0.004)	(0.015)
Post-Renewal	0.011***	0.006	0.001	0.015***	0.009	0.025**
	(0.003)	(0.004)	(0.017)	(0.004)	(0.003)	(0.010)
Post-Renewal x OH	-0.003	-0.012	0.009	-0.001	-0.020*	0.024
	(0.005)	(0.008)	(0.029)	(0.007)	(0.005)	(0.017)
Facility FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Season FEs	Yes	Yes	Yes	Yes	Yes	Yes
Other Controls	Yes	Yes	Yes	Yes	Yes	Yes
Facilities	973	793	959	412	372	403
Observations	58,234	47,324	57,285	24,650	22,188	24,072

Notes: Post equals to 1 if the month is after June 2011. Other controls are regulatory deterrence and weather variables described in Table 2. Linear probability model is used for violation dummy equation. Clustered standard errors in parentheses. Asterisks denote statistical significance: *** p<0.01, ** p<0.05, * p<0.1

Appendix 1. Discharge point and signage examples

NOTE: Example signs are from combined sewer outfalls rather than municipal or industrial final discharge points, but the signs themselves are roughly representative of those required for all types of discharges. Photo credits: It is our understanding that the first three pictures are in the public domain. The fourth photo was given to the authors with permission to disseminate.

Appendix 2: Map of EPA regions



Source: https://www.epa.gov/aboutepa#pane-4.

Appendix 3. Weather Data Construction

We obtain weather station-by-month rainfall and temperature data from the National Oceanic and Atmospheric Administration (NOAA)'s Global Historical Climatology Network (GHCN). For each month of our sample period, we match each facility in our dataset to all weather stations within 50 miles using provided latitude and longitude information. We then use inverse distance (squared) weighting to obtain facility-by-month rainfall and temperature data, following Ashraf et al. (1997).

By using inverse distance weighting, we are assuming that rainfall or temperature for facility *i* in month *t* is reasonably approximated by a combination of the K closest weather stations' records weighted by a function of the inverse of the distance between the facility *i* and the nearby K stations. The standard equation applied to our setting is:

$$f_{it} = \frac{\sum_{k=1}^{K} s_{kt} w_k}{\sum_{k=1}^{K} w_k} \quad \text{and} \quad w_k = \frac{1}{d_k^2},$$

where f_{it} is the weather value (rainfall or temperature) for facility *i* in month *t*, s_{kt} is the observed weather values for the k^{th} weather station in month *t*, w_k represents the weight determining the relative importance of the station, d_k represents distance between facility *i* and the k^{th} station, the inverse square of distance is used to calculate the weights, and K is the total number of weather stations that are within 50 miles of facility *i*.

Reference

Ashraf, Muhammad, Jim C. Loftis, and K. G. Hubbard. 1997. "Application of geostatistics to evaluate partial weather station networks." *Agricultural and forest meteorology* 84.3: 255-271.