# Truth-telling by Third-party Auditors and the Response of Polluting Firms: Experimental Evidence from India*

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**Abstract**

In many regulated markets, private, third-party auditors are chosen and paid by the firms that they audit, potentially creating a conflict of interest. This paper reports on a two-year field experiment in the Indian state of Gujarat that sought to curb such a conflict by reforming the system of environmental audits for industrial plants. In the control group, plants remained in the status quo system, wherein they directly chose and paid their third-party auditors, with little oversight. In the treatment group, auditors were randomly assigned to plants, paid a fixed fee (higher than the market price for an audit in the status quo) from a central pool and had their reports subjected to random backchecks by independent agencies. In the second year, auditors working in the treatment were additionally paid a bonus for accurate reporting. There are three main results. First, the status quo system was largely corrupted, with auditors systematically reporting plant emissions just below the standard, although true emissions were typically higher. Second, the treatment caused auditors to report more truthfully and reduced the fraction of plants that were falsely reported as compliant with pollution standards. Third, treatment plants, in turn, reduced their pollution emissions. The results suggest reformed incentives for third-party auditors can improve their reporting and make regulation more effective.

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# I Introduction

The use of third-party auditing to monitor the compliance of firms with regulations is ubiquitous. Third-party audits are the norm in financial accounting , and in many countries credit ratings from third-party agencies serve an important regulatory role (White, 2010). Consumer and commodity markets use third-party auditors to monitor standards, including those for food safety, healthcare, flowers, timber and many durable goods (Hatanaka et al., 2005; Raynolds et al., 2007; Dranove and Jin, 2010). With respect to environmental regulation, the focus of this paper, several countries use third-party auditors to verify firm compliance with national laws and regulations (Kunreuther et al., 2002; Paliwal, 2006). Third-party auditing is also used to enforce international environmental standards, including ISO 14001 certification and verification of carbon abatement in the carbon offset market (Potoski and Prakash, 2005; Bhattacharyya, 2011).

In all of these settings, the auditor is paid by and reports to the audited firm, which creates a conflict of interest between reporting the truth and reporting what is beneficial for the client. To maintain business, third-party auditors have incentives to shade or falsify their reports, which may corrupt information provision and, in turn, undermine regulation. Events brought to light by the recent financial crisis suggest this is a real concern, despite the attempts to increase the oversight on third-party auditors following the Enron scandal. > [1] Yet, despite periodic calls for reform to increase the independence of third-party auditors, we are unaware of a single instance of an enacted reform that fundamentally alters the incentives of third party auditors.[2]

This paper reports on a two-year field experiment conducted in collaboration with the

[1] In 2002, the Sarbanes-Oxley Act made auditors of public companies subject to oversight by a private-sector, nonprofit corporation. This corporation determines who can perform audits, conducts investigations and sets fines. The Act also required the SEC to send a report to Congress on the credit rating agencies but did not reform this sector, where biased reporting has remained a key issue. For a single credit agency, Griffin and Tang (2011) show higher accuracy of the internal surveillance team's judgements on CDO ratings than the business-oriented ratings team's, and that the accuracy difference predicts future downgrades. Strobl and Xia (2011) compared ratings for the same companies provided by two credit rating agencies, where one agency uses a issuer-pay model and the other an investor-pay model. The difference in ratings is more pronounced when the issuer-pay rating agency plausibly has more business at stake. For broad overviews of the corporate audit and credit ratings markets see Ronen (2010) and White (2010), respectively.

[2] Three former SEC Chairmen testified in favor mandatory auditor rotation, which was not adopted. In 2003 the Securities and Exchange Commission adopted rules on auditor independence that focused on restrictions on and disclosure of non-audit activities. In 2008 New York State Attorney General Andrew Cuomo reached an agreement with credit rating agencies which required upfront payment for their ratings. The Dodd-Frank financial reform bill and the Sarbanes-Oxley Act restrict the services that auditors or credit rating agencies can offer plants that they audit.

environmental regulatory body in Gujarat, India. Since 1996, the state has had a third-party audit system for plants with high pollution potential, wherein auditors annually submit pollution readings and suggested pollution control measures for the audited plants to the Gujarat Pollution Control Board (Gujarat High Court, 1996). Although the Gujarat High Court put in place several safeguards to limit conflicts of interest, the basic financial arrangement underlying these audits is the typical practice the world over—plants hire and pay auditors directly, and the work of auditors is subject to very little oversight. In conversations we had before beginning this study, the regulators, auditors, and polluting plants all agreed that the status quo audit system produced unreliable information. Indeed, the reported market price for an audit was frequently lower than the cost of collecting pollution readings, suggesting that measurements were often not even taken.

Our experiment altered the market structure in several complementary ways in order to incentivize accurate reporting. All 473 audit-eligible plants in two populous and heavily polluted industrial regions of Gujarat entered the experimental sample. In each region, half the plants were randomized into a multi-part treatment. First, treatment plants were randomly assigned an auditor that they were required to use. Second, auditors were paid from a central pool, rather than by the plant, and their fee was set in advance at a flat rate, high enough to actually measure pollution and leave the auditor a modest profit margin. Third, a random sample of each auditor's pollution readings were verified with follow-up visits to the audited plants by an independent technical agency that collected readings for the same pollutants at the same places as the auditor, usually within a couple weeks of the auditor readings. We refer to the follow-up visits as backchecks for the remainder of the paper. While the probability of being backchecked was public knowledge, actual backcheck visits were unannounced. Fourth and finally, at the start of the second year, treatment auditors were informed that their pay would be linked to their reporting accuracy, as measured by the backchecks. (During the first year, backcheck data had been used to measure auditor accuracy, but no consequence of good or poor performance was explicitly specified. Auditors may still have expected to be removed from the scheme or even disbarred if they proved to be systematically biased.)

We collate data from several sources. We collected all audit reports for years one and two filed with the regulator. We directly obtained backcheck readings from the agencies doing backchecks. Towards the end of the second year, we hired the same technical agencies to do identical backchecks in a random sample of control plants; these backchecks were unanounced and not used to monitor or reward auditor behavior. The availability of backcheck and auditor results from the same period offers a unique opportunity to compare true plant-level emissions with the auditors' reports of those emissions in both the treatment and control plants. Finally, roughly six months after the last audit visit in the experiment, we ran an independent endline survey of pollution outcomes in all treatment and control plants to measure the impact on emissions.

We have three main findings. First, status quo audit reporting is corrupted, as auditors chosen by control plants systematically report plant pollution readings just below the regulatory standard. The average difference between audit and backcheck pollution readings across all reported pollutants is -0.30 standard deviations in the control group. Consequently, auditors for control plants incorrectly report many pollution readings as compliant. An audit report is 29 percentage points more likely to show a plant as compliant than a corresponding backcheck of the same pollutant. False reporting is higher for pollutants that are more important to the regulator.

Second, the treatment caused auditors to report more truthfully and reduced the fraction of plants that were falsely reported as compliant with pollution standards. Relative to backcheck readings, treatment auditors report pollution readings that are 0.15 to 0.21 standard deviations higher than control auditors. This result is robust to the inclusion of auditor fixed effects that compare the behavior of the same auditors working in *both* treatment and control plants, which suggests that the results are not due to a selection of different auditors in the scheme. Further, auditors working in treatment reduce the share of plants falsely reported as compliant by 23 percentage points.

Third, treatment plants reduced emissions, presumably because they understood that the regulatory authority would receive more reliable audit reports. Average pollution in the treatment group fell by 0.17 standard deviations, with reductions concentrated among plants with the highest readings. In practice, the Gujarat Pollution Control Board reserves the harshest penalties for plants with readings that significantly exceed the standard, so it is not surprising that that the dirtiest plants responded by reducing emissions the most.

The treatment, which included multiple parts, was implemented as a single package. Hence, we cannot separately identify the effects of the treatment components—auditor assignments, fixed pay from a central pool, backchecks and incentive pay—using experimental variation.

Several plausible channels through which the treatment may have changed auditor behavior. First, auditors being assigned to plants at a fixed price means that plants cannot dismiss their auditor to obtain a better report. Nor can auditors hold-up plants once they are assigned.[3] Second, the regulator can use backchecks to monitor auditor quality and, though no sanctions for low quality were specified in the experiment in the first year, auditors may have anticipated a higher return to accurate reporting. We provide non-experimental analysis suggests that financial incentives for accuracy in the second year independently improved reporting. Third, above-market (in the treatment) auditor pay may have increased auditors' expected return to accurate reporting through an efficiency-wage channel, leading auditors in the treatment to report more accurately because they had more to lose if decertified.

The paper contributes to several literatures. We provide empirical evidence on economic incentives in third-party audit markets, a literature that has been mainly theoretical so far (Dranove and Jin, 2010; Bolton et al., 2011). More broadly, we contribute to the literature on corruption and development (Olken and Pande, 2012, for an overview see). Other examples of papers that compare outcomes reported by a potentially corrupt provider with independent estimates include Olken (2007) and Niehaus and Sukhtankar (2012).

Turning to the specific channels underlying our experimental treatment, on assignments, Bazerman et al. (1997) and Bazerman et al. (2002) suggest that auditors find it psychologically impossible to remain impartial when deeply involved in their clients' interests; our treatment may cut this bond. On monitoring, Becker and Stigler (1974) argued that agents with higher wages will be less corrupt if they are under supervision. Rahman (2012) argues theoretically for a contract where the principal randomly changes the state and measures whether the agent reports on this change; backchecks are a good substitute for such manipulation in this context given that pollution itself varies with some randomness. Absent supervision or monitoring, evidence on whether higher pay per se matters remains mixed (Rauch and Evans, 2000). Speaking to the monitoring and wage interaction, Di Tella and Schargrodsky (2003) show that higher wages reduce corruption in hospital procurement in Buenos Aires only when the probability of audit is reasonably high, which is consistent with our findings.

[3] These changes in the market structure would imply a higher market price for auditors. Put differently, audit price as a treatment component is not separable from the other aspects of market structure: low payments in the status quo are an endogenous outcome. Conversely, a high payment alone would be insufficient to change reporting, as auditors could pocket the difference in the absence of any monitoring.

Our findings are of independent interest, because the audit reform led to a reduction in pollution, which has been shown to be harmful to labor productivity and health ((Hanna and Oliva, 2011; Zivin and Neidell, ming), (Hanna and Greenstone, 2011) (Chen et al., 2010, 2011)). These findings are valid in the specific context of the reform evaluated and, of course, may not apply to other sectors or to environmental regulation in other countries. That caution notwithstanding, this paper presents clear evidence that altering economic incentives can cause third-party auditors to switch from biased reporting towards truth-telling, prompting improvements in the underlying regulated behavior.

The remainder of the paper is organized as follows. In Section II, we describe the background and experimental design. In Section III we discuss our data collection and present summary statistics. Section IV presents the econometric approach and results and Section V concludes.

# II Background and Experimental Design

## A Study Context

Our study was conducted in Gujarat, one of India's fastest growing industrial states (Chakravorty, 2003). Since 1991-92, the peak of industrial licensing reform, net state domestic product in Gujarat has grown at an average of 8% per year. Today, the state accounts for about 5% of the Indian population, but 9% of India's registered manufacturing employment and 19% of output (Authors' calculation, Annual Survey of Industries, 2004-05). Rapid industrial growth has, however, been accompanied by a severe degradation of air and water quality. Gujarat contains the two most polluted industrial clusters in India, and three of India's five most polluted rivers (Central Pollution Control Board, 2007, 2009b). Essentially all large cities in the state, as well as some industrial areas, violate the National Ambient Air Quality standards for Respirable Suspended Particulate Matter (RSPM) (Central Pollution Control Board, 2009a), an air pollutant dangerous for human health.

High levels of industrial pollution persist despite a stringent regulatory framework for pollution control (Hanna and Greenstone, 2011).[4] National laws set minimum levels of stringency for pollution standards, but basically all enforcement of environmental regulations occurs at

[4]The Water (Prevention and Control of Pollution) Act of 1974 created the Central Pollution Control Board as a coordinating body to set pollution standards and the state boards as enforcement agencies.

the state level. State Pollution Control Boards, such as the Gujarat Pollution Control Board (GPCB) are responsible for enforcing the provisions of the Water Act and the subsequent Air (1981) and Environmental Protection (1986) Acts and their attendant command-and-control pollution regulations. GPCB is responsible for the monitoring and regulation of approximately 20,000 plants.

## B Environmental Audit Regulation

The main instruments that GPCB uses to limit industrial pollution are plant-level inspections and third party environmental audits. This paper focuses on the environmental audit system.

In 1996, in order to remedy the perceived failure of inspections in enforcing pollution standards, the High Court of Gujarat introduced the first third-party environmental audit system in India (Gujarat High Court, 1996). Under the scheme, plants with high pollution potential must submit a yearly environmental audit, conducted by an audit firm hired and paid for by the plant. Audit-eligible plants are classified as Schedule I (most polluting) or Schedule II (highly, but somewhat less polluting) on a basis of three dimensions: what the plant produces, where it sends its effluent, i.e. wastewater, and the volume of that effluent.[5] Schedule I plants must be audited by Schedule I auditors, usually an engineering college or similar institution. Schedule II plants must be audited by a private audit firm, called a Schedule II auditor. This study concerns reporting of Schedule II auditors and henceforth we refer to plants in Schedule II as "audit eligible." We also refer to regulated industrial plants as "plants" throughout and reserve the word "firms" for audit firms. .

Auditors visit each plant for about one day in each of three seasons of the year to observe environmental management practices and measure pollution. Auditors compile their findings from the three visits in a standardized format, fixed by the audit regulation, and submit the audit report to the plant and GPCB by February 15th of the following year. The final audit report describes the production process and physical state of the plant, including the measures the plant has taken for pollution control and the results of pollution sampling during each of the visits. Finally, auditors provide recommendations on pollution control to the plant.

On paper, the audit system also includes several safeguards and severe penalties for au-

[5]For example, plants that produce certain types of dyes and dye intermediates are classified in Schedule II, roughly, if their effluent is between 25 and 100 thousand liters per day, with variations around this classification based on whether the effluent discharged by the plant goes on to further treatment in a common effluent treatment plant (CETP). A plant with effluent below 25,000 liters would be exempt from the audit requirement.

ditors. Each four-member audit team must meet technical standards and be re-certified by the regulator every two years.[6] Audit teams can audit at most 15 plants per year, and an audit firm, which may employ several teams, can audit a plant at most three years in a row. Auditors with reports found to be inaccurate are liable to be decertified and their reports on behalf of other plants declared void.

On the other side of the market, for an eligible plant, failure to submit an audit is punishable by closure and disconnection of water and electricity. [7] A report showing noncompliance with the terms of a plant's environmental consent can also be punished by closure or fine (Gujarat High Court, 1996). As we demonstrate below, the GPCB issues penalties to plants with a surprisingly high frequency when they have evidence of violations.

Nevertheless, all sides consider the audit system, as originally implemented, as functioning poorly. Industry recently litigated against the scheme, somewhat ironically and without success, to get the High Court of Gujarat to throw out the audit requirement on account of GPCB not following up on audit reports (Gujarat High Court, 2010). The regulator, for its part, believes that inaccurate reporting renders audits useless for enforcement, so review of submitted audits by GPCB is mostly *pro forma*.

Consistent with auditor shopping, we observe strong price competition in the environmental audit market. In interviews conducted prior to the experiment, both auditors and plants claimed that an audit report could be purchased for as low as INR 10,000-15,000 (roughly $200-$300). Our data on actual prices paid by control plants indicate that, conditional on reporting any payment, plants reported a mean payment of roughly INR 24,000 which appear lower than the true cost basis of conducting an audit.

## C Experimental Sample and Design

In collaboration with GPCB, we designed and evaluated a modified audit system which sought to improve the accuracy of auditor reporting.[8]

Our sample is the population of audit-eligible plants in the GPCB regions of Ahmedabad

[6] Team members are required to have degrees in environmental engineering, chemical engineering, chemistry and biology, and minimum of two members must have at least one year's experience in environmental management.

[7] In practice, some plants do not submit reports, usually claiming they are not eligible.

[8] This experiment was designed and undertaken concurrently with the evaluation of another intervention, an increase in inspection frequency for some plants, which was conducted stratified on the audit treatment and which we study in a separate paper.

and Surat, the two largest cities of Gujarat. We obtained from GPCB a list of all 2,771 red category (i.e. high pollution potential) plants with reported capital investment less than INR 100 million (about USD 2 million), which are designated small or medium scale. Based on available data and in accord with the eligibility criteria we selected 633 plants as the provisional sample of audit-eligible plants.[9] Just before the 2009 audit season, we randomly assigned half of the plants within this provisional sample, stratified by region, to the audit treatment group. After the randomization, we collected the detailed sectoral information needed to determine eligibility and eliminated, using exactly the same criteria, plants found to be ineligible from both the treatment and control groups, reaching the study sample of 473 plants, 49.2% of which belong to the treatment group.

Treatment plants were assigned to the audit treatment once, in 2009, for the audit years 2009 (hereafter year one) and 2010 (year two). Treatment plants were formally informed of the changes in the audit regulation that would apply to them by a letter from GPCB.[10] Relative to the status quo, the treatment altered three components of the audit system during year one: an auditor was randomly assigned to the plant, paid from a central pool at a fixed rate and its reports were backchecked for accuracy. In year two only, direct incentive pay for auditor accuracy was added. These components were implemented as follows.

**Assignment and Fixed Pay**. Auditors were randomly assigned to treatment plants by the research team and paid from a central pool of funds raised for the study. The payment was fixed at INR 45,000 in the first year. This rate was estimated by applying GPCB's sampling charges to the average plant characteristics in the audit sample and adding a small margin; it should thus represent the average cost of completing an audit, though as noted above it was in the high range of market prices. (Variation in actual cost arises due to a plant's sector and other characteristics; the textile plants which make up 80% of our sample are at the high end of this range with an estimated cost for an audit of roughly INR 40,000.). Note that the treatment payment is significantly above the average price of INR 24,000 that control plants report paying for an audit in our sample. This control price appears to be well below what would be required for auditors to collect the required pollution samples.

[9] GPCB did not have a definitive list of audit-eligible firms at the time of sample selection.

[10] The text of the letter sent to treatment plants is in the online appendix. Plants commonly receive regulatory notices and we do not believe the letter itself was a treatment channel, since it did not induce treatment plants to submit audits at a higher rate than control plants.

For auditor recruiting, at the start of each year all GPCB-certified Schedule II auditors were solicited for their interest to participate in the treatment. In both years interest was oversubscribed, relative to the number of treatment plants (Auditor interest in the program likely reflected the fact that it offered better working terms, including payments which were in the high range of the market.) Consequently, at the beginning of each year, auditors were randomly allocated a number of plants in proportion to their capacity, measured by number of certified audit teams, which was predetermined.

Auditors could use auditing capacity not allocated in the treatment to conduct audits in the control group. Thus, some of the audit firms were working under two very different sets of incentives at the same time, and we exploit this variation. In the first year, out of 42 audit firms, 24 worked in control only, 9 in treatment only, and 9 in both. In 2010, out of 34 audit firms, 7 worked in control only, 12 in treatment only, and 15 in both.[11]

**Backchecks**. A randomly selected 20% of auditor plant readings in the treatment were backchecked in the field by technical staff of independent engineering colleges. (These colleges were certified as Schedule I auditors and hence would never directly audit the Schedule II plants in our sample.) Backchecks measure the same pollutants as audits and in the same manner; as Schedule I auditors, backcheckers have much experience collecting and analyzing pollution samples when acting as auditors themselves. The median backcheck occurred six days after an audit visit. Auditors were aware of the possibility of being backchecked, and knew that backcheck results would be used for quality control, although in year one no sanctions for poor performance were specified in advance and all auditors were paid the same regardless of accuracy.[12]

**Incentive Pay**. In year two, an explicit incentive pay for auditor accuracy, as measured by backchecks in the treatment, was added to the basic set of reforms. Incentive payments used a formula that was first applied to auditor readings in year one to demonstrate to each auditor how accuracy was measured. The pay formula first calculated the difference $\delta_p$ between audit report pollution concentration readings and backcheck readings, normalized by the standard

[11]The increase of auditors working in treatment only or both in the second year comes from the fact that, in the first year, some auditors were not able to participate because they had already reached their capacity when the program was announced.

[12]In practice, GPCB received aggregated reports that summarized the accuracy of auditors and the ranges that determined the bonuses in year two. This format did not give them the information necessary to levy sanctions or penalties specific plants.

deviation of backcheck readings, for each pollutant $p$. It then averaged the scores for six water and three air pollutants into indices for each media and created the overall measure of auditor quality as the average of these two.

$$\Delta_{Water} = \sum_{p \in Water} \delta_p, \qquad \Delta_{Air} = \sum_{p \in Air} \delta_p, \qquad \Delta_{All} = (\Delta_{Water} + \Delta_{Air})/2.$$

Auditor readings that matched backchecks exactly would thus mean an index value of $\Delta_{All} = 0$, while a value of one means that the weighted average auditor reading exceeded the weighted average backcheck reading by one standard deviation.

Auditors were grouped into three payment categories based on this summary of the difference between their reported readings and the backcheck readings. The least accurate quartile of auditors was paid INR 35,000 per audit. The next least accurate quartile received INR 40,000 per audit, and the most accurate half was paid INR 52,500 per audit. The bonus scheme therefore maintained the average pay of INR 45,000 from year one.

## D How Would the Treatment Change Auditor and Plant Behavior?

Under the status quo market structure, several factors likely contributed to auditor misreporting. First, plants could shop for an auditor who would provide a favorable report and condition payments on the contents of that report. In such a market, an audit firm has an incentive build a reputation for leniency to make it more likely that it would be hired by the same or another plant in the future. This market structure also rewarded cost-cutting by an auditor, since it could offer plants a compliant report at the lowest price if it skimped on data collection, which is redundant if the contents of the report are agreed on beforehand. The last is consistent with data that showed the equilibrium audit price in the control to be below the estimated cost of actually collecting samples and conducting laboratory tests. Working against these auditor incentives to misreport, auditors might be decertified if caught misreporting by GPCB .

The treatment changed auditors' incentives on several dimensions. First, assignment to a plant by an external authority and a fixed pay structure implies that auditor compensation is independent of what it reports. This independence would reduce auditor incentives to report a plant as compliant in order to maximize current and future payments from the plant. Second, the introduction of backchecks increased auditor monitoring and likely raised the perceived likelihood that, although not explicitly part of the experiment, the regulator would disbar auditors who submitted false reports (or at least not assign them to treatment plants in the future). In equilibrium, this also raises the bribe a plant would have to play the auditor to induce false reporting. The introduction of incentive pay in the second year, which explicitly rewards accuracy, enhanced this effect. Third, the level of pay was fixed to be high enough for auditors to cover the actual costs of conducting an audit, including collecting and analyzing the air and water samples. This increase in pay relative to the market price was necessary since we expected auditors to do the work. Higher payment, relative to the control market pay, may have interacted with increased monitoring to further incentivize accurate reporting in the treatment, although this channel would be weakened by two factors. First, the experiment was limited to a two-year horizon. Second, the increase in profit for an auditor who switches from not collecting the data (in the status quo) to collecting the data (in the treatment) would be small: we estimate that doing the work in the treatment would cost at least INR 6,000 in travel and around INR 20,000 in sample collection and analysis charges for a total INR 26,000 increase in costs. This is larger than the increase in INR 21,000 in average payment to an audit firm in the treatment relative to the control.

These changes in auditors' reporting may in turn change plant behavior. Treatment plants will respond to more accurate reporting by increasing abatement if the perceived risk of regulatory sanction is sufficiently high relative to the cost of abatement.

# III Data and Summary Statistics

In this section we describe the multiple data sets used in the analysis, and provide summary statistics on our plant sample.

## A Data Sources

The key outcomes of interest are accuracy of auditor reporting and the pollution response of plants. Two data sources are used to measure accuracy. First, audit reports filed with GPCB in 2009 and 2010. These reports cover a mandated set of water pollutants (i.e., biochemical oxygen demand, chemical oxygen demand, total dissolved solids, total suspended solids, ammoniacal nitrogen) and air pollutants (sulfur dioxide, nitrogen oxides, and suspended particulate matter) described in Table A1. [13]Both treatment and control plants followed the same practice of scheduling audit dates in advance of actual auditor visit to collect air and water samples. This opens the possibility that plants alter short-run behavior to ensure low pollution readings, for example by running air pollution control equipment or reducing boiler load when pollution is to be measured. We expect that, if anything, this should be more likely in the treatment, reducing estimates of how much the treatment increased pollution readings in audits. The second source of data for auditor accuracy is the backchecks, which were conducted in a sample of treatment plants throughout 2009 and 2010. The backchecks were conducted on the same pollutants and locations as the audits, and were scheduled to occur close to the audit visits. Auditors and backchecks use the same technology and standardized procedures to measure pollution. Treatment backcheck data were complemented by "midline" backchecks after the third season of audit visits in year two in both treatment and control groups, using the same process and agencies. Auditors were not informed about the probability of backchecks in the control plants, and these backchecks were neither transmitted to GPCB nor used to compute any specific auditor payment.[14] The midline data allow for a direct measurement of the comparative accuracy of auditors working in treatment and control plants, measured as the difference between auditor and backcheck readings.

Data on plant pollution response comes from an endline survey conducted from April through July of 2011, approximately six months after the last audit visits in the treatment group. Pollution sampling in the survey was mostly conducted by the agencies that did backchecks and included the same pollution samples as discussed above. Our endline analysis includes all plants in the audit sample (treatment and control). Overall, we collected 2,953

[13]Auditors record these pollutants at various stages in the treatment process and with respect to different systems in the plant. We use pollutant concentrations at the final outlet from the plant for water samples, as these are the readings with a direct impact on the environment and are therefore most closely attended by both auditors and GPCB. For air, we focus on boiler-stack samples for the widest comparability across the sample, as most plants have boilers.

[14]The midline sample was drawn from treatment and control groups in order to maximize the number of plants covered by auditors working simultaneously in both the treatment and control groups and to use information on the dates of audit visits to conduct backchecks that were as close as possible to the date of the initial visit. In the treatment group, the sample plants were randomly selected stratified by auditor. In the control group, the sample plants were drawn non-randomly in order to ensure coverage of auditors working in the treatment simultaneously. Priority for the survey was first given to plants that previously submitted an audit report by an auditor working in the treatment group. The control sample was completed by adding those plants for which auditors submitted a date for the audit visit and finally by adding randomly selected plants for which auditors had not submitted a date.

pollution samples from 408 plants in the study sample, an average of 7.2 pollutants per plant.[15] Attrition in the endline survey was balanced across the treatment and control groups. While a somewhat greater share of plants were surveyed in the treatment group, 88.8%, versus 83.8% of control plants, the difference of 5.09 percentage points (standard error 3.16 percentage points) in these rates is not statistically significant. Most attrition was attributable to plant closure.

Finally, to better understand plant and auditor incentives in the status quo and their response to the treatments, we utilize GPCB administrative data. This covers GPCB's plant inspections and pollutant sampling for plants in the audit sample between 2008 and 2011. We link 8,627 GPCB inspections, with their accompanying pollutant readings, to 4,269 subsequent actions or penalties as documented by the regulatory files on each plant. The actions range in severity from letters of warning up to orders that the plant be disconnected from electricity supply.

## B Summary Statistics

Despite being *prima facie* audit-eligible, not all plants in the sample submit audits, for several reasons: GPCB had judged them not-audit-eligible in the past, they changed products since data used to determine eligibility was compiled or closed, they protested their audit-eligible status, or simply chose not to submit and incur the risk of a penalty. Table I shows that treatment and control plants were about as likely to submit audit reports. Treatment plants were slightly less likely to report in the first year (70% versus 74%) slightly more likely in the second year (70% versus 64%), though neither difference is statistically significant. These rates of submission are comparable to those in 2008, the year prior to the experiment (72% in treatment plants and 69% in control plans). The treatment, therefore, does not appear to have induced more plants to submit audit reports.

Though balanced in aggregate, there may be heterogeneity in which plants submit across the treatment and control groups. Probit estimates of the determinants of submission in the two groups using administrative data from before the experiment[16] suggest that plants that are unambiguously eligible or face greater risk of regulatory sanction submit (Online

[15] The audit intervention was conducted concurrently with another treatment at the plant level. We restrict the pollution sample to the subset of audit sample plants not subject to the other experimental intervention.

[16] We use the following variables to predict submission: whether a plant is located in an industrial estate, whether it is in the textiles sector, whether its effluent flows to a common treatment plant, the amount of wastewater it generates, whether it submitted an audit before the experiment, whether it was cited by the regulator for a violation before the experiment, and a dummy for the second year of the experiment.

Appendix Table 1). Specifically, plants in the textile sector, which are for the most part clearly audit-eligible due to high effluent volume, plants which submitted audits or received a regulatory citation in the year before the experiment are more likely to submit. We interact plant characteristics with a treatment dummy and find that treatment itself does not affect submission, conditional on plant characteristics, and that effects of plant characteristics on submission are largely the same in both the treatment and control groups. The exceptions are that treatment plants, as noted above, are relatively more likely to submit in the second year of the experiment and that the effect of being in the textile sector is weaker, though still significant, for treatment plants.

While overall submission rates and the heterogeneity analysis suggest that reasons for submission are generally well balanced across treatment and control groups, it is possible that differences on unobservable dimensions that predict outcomes remain and that this selection biases results on the effect of the treatment on audit reporting. We note, however, that in several specifications we control for true pollution levels using backchecks, so this difference would have to be on a dimension other than pollution to influence the results. Beyond directly controlling for pollution, we check the robustness of our results to selection bias by using the probit model for submission, without treatment interactions, to reweight the distribution of submitters to resemble that of all plants, as in DiNardo et al. (1996) (hereafter DFL). This model with baseline characteristics strongly predicts submission of audit reports. Moreover, the distributions of predicted submission from the model have a broad support that is common across plants that actually submit and those that do not submit.

Table II presents summary statistics for plants in the study sample that submitted an audit report in either year of the experiment using baseline data from GPCB records. We condition the sample on audit submission as most of the subsequent analysis of auditor reporting uses data from submitted audit reports. Columns (1) and (2) report the treatment and control means, and column (3) provides the randomization check by reporting the coefficient on the treatment dummy from a regression which controls for plant region.

Plants submitting audits are similar across treatment and control. Panel A reports plant characteristics. Most sample plants are textile factories eligible for environmental audit due to high effluent volume. Textiles is the largest registered manufacturing sector by employment in India and second largest in Gujarat (Authors' calculation, ASI 2005). Both treatment and control plants have similar pollution potential as measured by effluent quantity and type of fuel used. Treatment plants are 10 percentage points less likely to have a bag filter, a type of air pollution control equipment, installed, but are similar to control plants with respect to other air pollution control equipment such as cyclones and scrubbers. In the same comparison of covariate balance for the full study sample, unconditional on submission (not shown), bag filter installation remains the only difference between treatment and control plants significant at the five-percent level.

The midline sample, the subset of plants that were backchecked during the midline, remains well-balanced along the observables shown in Table II .One final sample of plants of interest (for the fixed effect specification) are those that are audited by auditors working in both the treatment and control group, which is well balanced as well (results not shown). [17]

Table II, Panel B reports on the interactions of sample plants with GPCB in the year prior to the study by treatment status. A little over 80% of this group submitted an audit report in the year prior to the study's initiation. Roughly the same fraction was inspected and over 40% of sample plants were mandated to install equipment.[18] Based on the GPCB records, a significant number of sample plants have been subject to costly regulatory actions: around a quarter were cited for any type of violation and fully ten percent of plants, in both treatment and control, had their utilities disconnected at least once. About three percent were required to post a bank guarantee (i.e., bond) against future environmental performance. These variables are balanced across treatment and control plant. Consistent with being less likely to have a bag filter, treatment plants were more likely (at the ten percent level) to have received a citation for an air pollutant violation than control but equally likely for water pollutants and all citations together. In summary, our experiment occurred in a setting where information reported by auditors should matter to plants as the regulator has a meaningful track record of action.

[17] The one additional imbalance in both sample is that the treatment plants generate more wastewater. This will be controlled for for a plant's true pollution level in the midline.

[18] This 40% is atypically high; during the prior year GPCB had conducted an air pollution control equipment installation campaign that affected many sample plants.

# IV Econometric Approach and Results

The results are divided into three parts. First, we use data on control plants to examine the status quo audit market. Second, we estimate the effect of the treatment on auditor reporting behavior as measured by pollution levels in audit reports and corresponding backchecks. Third and last, we measure how two years of altered auditor incentives influenced plant polluting behavior.

## A Auditor Reporting in the Control Group

A unique feature of this paper's setting is that we observe both auditor reports and an independent measure of the underlying pollution—backchecks of the same pollutants. This allows us to assess whether the market was producing reliable information for the regulator.

Figure 1, Panel A begins the analysis by showing the distributions of concentrations of Suspended Particulate Matter (SPM), an important air pollutant, from audit reports and backchecks for the control group. In each distribution a vertical line marks the SPM regulatory standard of 150 mg/m$^3$ and the share of the probability mass that falls between 75% of the standard and the standard is shaded in gray, the zone where we might expect measurements to fall if auditors are trying to show firms as compliant without being too conspicuous.

We observe several striking facts. The top half of Panel A shows that auditors report the vast majority of plants (93%) as compliant with the SPM standard, and there is a high concentration of readings just below the standard; 73% of plants have auditor-reported SPM concentrations in the narrow range from 75% to 100% of the pollution standard. Such bunching below the limit is consistent with two explanations aside from targeted misreporting by auditors. First, plants may be minimizing abatement costs subject to the constraint of not exceeding the standard. Second, the standard may be deliberately set at a level that allows plants to narrowly comply.

The second half of Panel A uses backcheck data to distinguish these possibilities. Our second finding is significant dispersion in the backcheck distribution for SPM. Only 19% of the plants have readings in the range that covers 75% to 100% of the standard, which is 54 percentage points less than in the audit distribution. Further, substantial probability mass exceeds the standard: 59% of backcheck readings exceed the standard compared to just 7%

in the audit distribution. There are also more very low backcheck readings. This increase in the left tail of backchecks, relative to audits, may be explained by the cost of measuring air pollution concentrations, making it cheaper for auditors to report narrow compliance by default than to properly sample and document a very low reading. The audit and backcheck distributions together provide striking evidence that, at least in the case of SPM, auditors file false reports showing that plants narrowly comply with the regulatory standard. Backchecks do not similarly cluster beneath the standard, belying the two alternative explanations for the pattern of auditor reporting.

Next, we use a regression analysis to check whether the difference in pollution readings between audit reports and backchecks holds across the full range of pollutants, continuing to use the control sample only. Table III reports results from OLS regressions on the stacked data, including both backchecks and audit readings, of the form:

$$\mathbf{1}\{Compliant\}_{ij} = \beta_1 \mathbf{1}\{AuditReport\} + \alpha_r + \epsilon_{ij}. \tag{1}$$

In Panel A, $1\{Compliant\}_{ij}$ equals 1 for readings of pollutant $i$ from plant $j$ that are between 75% and 100% of the regulatory standard, and in Panel B $1\{Compliant\}_{ij}$ equals 1 if the reading is below the standard. The coefficient of interest is $\beta_1$ on the dummy $\mathbf{1}\{AuditReport\}$; each plant appears twice, as the data is pooled across matched pairs of audits and backchecks, so $\beta_1$ indicates how likely a pollutant report is to be compliant in audits relative to the omitted category of backcheck readings. The specification includes fixed effects $\alpha_r$ for the regions $r \in \{Ahmedabad, Surat\}$, on which treatment assignment was stratified, and we cluster standard errors at the plant level to account for correlation in the errors for different pollution samples from taken at the same plant (on average we have seven pollutants per plant).

Table III, Panel A, column (1) shows that across all pollutants pollution levels in audit reports are 27 percentage points more likely to show narrow compliance than backchecks. This large increase is against a baseline of just 10% of backcheck readings that fall in the 75%-100% range. As to whether the reading is compliant, Panel B, column (1) shows that across all pollutants 55.7% of backchecks are below the relevant regulatory standard. The coefficient for audit reports indicates that an additional 28.8% of readings from audits are falsely reported as below the standard. Taken together, Figure 1 and Table III confirm that neither plant abatement behavior nor regulatory capture underlies the clustering observed in Panel A of Figure 1; rather, the status quo audit system failed to provide accurate information to the regulator.

## B The Effect of the Treatment on Auditor Behavior

### B.1 Truth-telling about Regulatory Compliance

Next, we examine the impact of the treatment on auditor reporting. Figure 1, Panel B shows the distributions of SPM concentrations as reported by audits and backchecks for treatment plants during the midline.

The top half of Panel B reveals that in the audit treatment group 39% of readings are within 75% to 100% of the standard. This is far below the 73% that were in this range in the control group audit reports. Further, the support of the audit and backcheck distributions is much more similar among the treatment plants than it was among the control. The similarity is especially evident for readings above the standard, which were very sparse among control audits. While the treatment increased truth-telling by auditors, it did not end false reporting. The 39% share of audit readings in the shaded area still exceeds the 14% share in the backcheck distribution.

To examine the impact across all pollutants we pool samples of *all* pollutant readings from audits and backchecks for plant $j$, both collected in the final season of year two. We estimate ordinary least-squares regressions of a difference-in-difference form:

$$\mathbf{1}\{Compliant\}_{ij} = \beta_1 \mathbf{1}\{AuditReport\} \times T_j + \beta_2 \mathbf{1}\{AuditReport\} + \beta_3 T_j + \alpha_r + \epsilon_{ij}, \qquad (2)$$

where $1\{Compliant\}_{ij}$ and $\alpha_r$ are defined as before, and standard errors are clustered at plant level. The treatment coefficient $\beta_3$ controls for any difference in true compliance, as measured by backchecks, across treatment and control. There are two coefficients of interest. The first is $\beta_2$, which measures how much more likely an audit report is than a backcheck to be compliant in the control group. The second, $\beta_1$, measures how treatment changes this difference between pollution levels in audit reports and backchecks.

The results are in Table IV. The treatment increased truth-telling about compliance with the regulatory standard across the full set of pollutants. In Panel A, column (1), audit reports are 19 percentage points less likely to report a reading in the narrow range of 75-100% of the standard in the treatment than in the control. This is a reduction of 69%, relative to the control mean. Similarly Panel B, column (1) reveals that audits in the treatment are 81% (-0.234/0.288) less likely to report compliance with the standard. These effects are evident separately for both water pollutants, in column (2), and air pollutants, in column (3).[19]

The effect of the treatment on reporting of compliance and narrow compliance is basically unchanged after applying the DFL reweighting for selection.[20] This approach reweights observations in the sample of plants that submit by the odds ratio of non-submission, to approximate the observable characteristics of all plants. As the results are steady with this reweighting, the difference in compliant pollution levels in audit reports across the treatment and control groups does not appear due to a change in which plants report. It is also possible that the backcheck compliance measure in these regressions already controls for selection effectively.

Figure 2 summarizes the changes in the density of the audit report pollutant distribution due to the audit treatment. To normalize pollution readings relative to the regulatory standard, we subtract the standard for each pollutant, and divide by the pollutant standard deviation in backchecks. The horizontal axis therefore marks the number of standard deviations above or below the regulatory limit. We then fit 40 separate regressions for indicator variables for a pollutant reading belonging to a particular 0.05-standard-deviation-width bin on an indicator for an audit report being from the audit treatment. Weakly negative (positive) values indicate that treatment auditors are less (more) likely to report readings in that bin. The treatment dramatically reduces the amount of mass just beneath the standard. The treatment auditors instead report significantly more readings in the bins more than 0.1 standard deviations below the standard and especially in the bins stretching up to 0.5 standard deviations above the standard. Note that for all pollutants together, unlike for SPM, we do not see systematic increases in pollution reports at levels well below the standard.[21]

The treatment, then, succeeded in greatly increasing the frequency with which auditors

[19] For comparability, we omit pH from all panels as both high and low readings can be harmful, unlike for the other pollutants. In Panel A the results are unchanged if we include pH, for which the standard is a range rather than a maximum limit.

[20] For example, the coefficients of interest (standard error) on Audit report X Treatment corresponding to the first row of Table IV Panel B, where compliance is the dependent variable, are -0.190 (0.041), -0.158 (0.049) and -0.241 (0.067).

[21] Across all pollutants, a significant 4 percentage points more audit readings in the treatment than in the control are below 75% of the regulatory standard. This difference shrinks to 2 percentage points, and is no longer significant, after conditioning on backchecks below 75% of the standard.

truthfully report pollution readings above the relevant standard. The problem in the status quo was clearly not only one of low effort by auditors—which may have been an equilibrium response to the pressure of giving a favorable report—but also systematic distortion, and the treatment seems to have ameliorated it.

### B.2 Truth-telling about Pollution Levels

Though the compliance threshold is discrete, the regulator needs to know the level of continuous pollution emissions, since this ultimately affects public health. We therefore examine next the reported concentrations of pollutants in audit reports. We standardize pollutants by subtracting the mean and dividing by the standard deviation of the same pollutant reading among backcheck samples. Thus, a one-unit change in the pollution measures can be interpreted as a change of one standard deviation. We then estimate:

$$y_{ijt} = \beta T_j + \alpha_r + \alpha_t + \epsilon_{ijt}, \tag{3}$$

where $y_{ijt}$ is the standardized audit report reading of pollutant $i$ in plant $j$ taken in year $t$. The set-up is similar to equation 2, except the data are drawn only from audit reports and cover multiple years. (The advantage of using only audit data and not backchecks is data availability for both years of experiment.) To account for annual variation in pollution, $\alpha_t$ are fixed effects for the year $y \in \{2009, 2010\}$. We cluster standard errors at plant level.

The parameter of interest, $\beta$, measures whether auditors' reported concentrations differ in treatment and control groups. In specifications with auditor fixed effects $\alpha_a$, $\beta$ is identified from cases where the same auditor works under both treatment and control audit systems.

The results are in Table V. In Panel A, we present estimates from specifications where the outcome is a dummy for the audit report being below the regulatory standard (compliance). This is the same outcome as in Panel B of Table IV, except here we use all submitted audit reports instead of just those from the midline. The column (1) results suggest that treatment audit reports were 15 percentage points less likely to report that a pollutant reading was in compliance with the relevant standard. In the column (2) specification that includes auditor fixed effects, the point estimate is essentially unchanged. The remaining columns suggest that the decline in reported compliance is greater for water pollution readings than for air pollution ones.

Panel B turns to the results for continuous measures of reporting. In column (1), the mean audit report reading for all pollutants in treatment plants is a significant 0.103 standard deviations higher than the mean report in the control. The addition of auditor fixed effects, as shown in column (2), increases the point estimate of the audit treatment to 0.131 standard deviations, but the estimates with and without auditor fixed effects remain statistically indistinguishable. It is noteworthy that the effect of the treatment remains constant, even when the identification comes from within-auditor variation, showing that the same audit firms report differently under the two regimes. The coefficients on the audit treatment are similar for both water and air pollutants considered separately, as shown in columns (3) through (6).

The magnitude of these effects is substantial. Consider the estimate in column (2) of 0.131 standard deviations for all pollutants, which is roughly of the same size as the effect for Biochemical Oxygen Demand (BOD) estimated alone (not shown). For plants where the effluent did not go on for further treatment, the standard deviation of BOD in backchecks in final-outlet samples was 203 mg/l and the mean 191 mg/l, as against a concentration standard of 30 mg/l. An effect of size 0.131 standard deviations thus represents 26.7 mg/l for BOD, or 89% of the standard. The mean and standard deviation of $SO_2$ readings in backchecks were 64 and 108 parts per million (ppm), respectively, as against a standard of 40 ppm. A 0.131 standard deviation movement is thus 35% of the standard. Consistent with our earlier discussion of the distributions, these specifications show that the change in pollutant reports shifts several plants from compliance to non-compliance and is economically significant, in representing a meaningful increase in reported pollution.

One concern with using auditor reports of pollution concentrations as an outcome is that they combine true pollutant emission, measurement error and potential auditor manipulation of the results. If the treatment caused plants to reduce emissions, then auditor reports conflate changes in auditor reporting and pollution emissions such that the estimate of $\beta$ will understate the effect of the treatment on auditor reporting. We investigate this possibility by returning to the midline data using the difference between audit and backcheck readings of the same pollutant,

$$y_{ij}^{D} = y_{ij}^{Audit} - y_{ij}^{Backcheck},$$

as an outcome, where $y_{ij}^D$ is the audit reading less the backcheck reading. This difference controls at the plant level for any possible effect of the intervention on actual pollution levels, but is only available on a comparable basis for the midline sample. Readings are matched on pollutant $i$, plant $j$, sampling location (boiler stack or final outlet) and date. Negative values indicate underreporting, with auditors reporting lower readings than the true emissions concentrations, treating backchecks as the truth.

The results are reported in Table V, Panel C. The difference between audit and backcheck readings is -0.304 standard deviations in the control, reflecting the negative bias in auditor reporting that we have observed. The treatment coefficient of 0.210 standard deviations (standard error of 0.073 standard deviations) indicates that treatment auditors report substantially higher readings, after accounting for any changes in actual emissions across the treatment and control groups. Indeed, this finding implies that the treatment erases nearly 70% of the underreporting in the control group. Unlike in the specifications in audit levels, the treatment coefficient in audit differences becomes somewhat smaller in specifications with auditor fixed effects, though not significantly so.

The results for compliance in the full sample of audit reports and for continuous audit reporting are qualitatively unchanged by correcting for selection of plants into submitting audit reports. In Panel A, the DFL correction somewhat, but not significantly increases the magnitude of point estimates. In Panel B, for specifications in pollutant levels, the DFL correction also increases the point estimates for audit treatment for the all and water pollutants specifications. The point estimates in the difference specification of Panel C are about 20% smaller, which is within one standard error of the Table V estimates. Further, the DFL correction increases the standard errors in panels B and C such that the 95% confidence intervals of all of the Table V and corresponding DFL estimates easily overlap.[22] Note that the changes in point estimates are larger in the levels specifications, where selection may be a greater concern since backchecks are not used as a control for contemporaneous pollution. [23] We conclude that correcting for selection into audit submission on observables does not change our findings with respect to auditor reporting.

[22]With the DFL correction, the treatment coefficients (standard errors) corresponding to the specifications in odd columns of Table V Panel B are 0.230 (0.114), 0.314 (0.168) and 0.064 (0.025). The same coefficients in the difference regression of Panel C are 0.165 (0.119), 0.117 (0.152) and 0.241 (0.165).

[23]Heckman (1979) gives an alternative approach to selection correction. Using that approach, and the same variables as used to predict submission in the DFL approach, all coefficients are nearly identical to the main results in both Panels.

### B.3 Evidence on Possible Treatment Channels

Our treatment spanned two calendar years and included several components—random assignment of auditors to plants, payment from a central pool, and backchecks—each of which may have independently influenced auditor reporting.

The results allow to rule out some channels. The fixed effect results suggest that higher pay did not directly improve reporting through an income effect, as extra income would have given auditors more resources in both treatment and control. Selection of better auditors into treatment participation and Hawthorne effects at the auditor level are also inconsistent with the within-auditor estimates of a significant treatment effect.

But all the channels described in section 2.4 may have been at play. The sample size was too small to separately randomly assign each component and obtain meaningful estimates of their effects. In year two, however, we introduced incentive pay for treatment auditors based on their reporting accuracy. The associated non-experimental variation provides an opportunity to examine the independent effect of financial incentives on reporting.

An challenge in doing so is that treatment plants were reducing their emissions over time, relative to control plants (on this, see Section C below). Hence, in identifying the effect of incentive pay one needs to account for a differential decline in pollution across treatment and control plants. In particular, we need to accurately specify the time trends and assume no other factors that influence audit reports changed discretely between the two years in the treatment relative to the control. To measure changes in reporting over time, our sample is the full sample of audit reports in both years, and our outcome variable, as we lack backcheck reports for control plants in year one, is the reported pollution reading from audits only.

To check validity of time-series identification of incentive pay, Figure 3 shows the trends in audit reporting over time. The figure plots the mean standardized pollution reading in audit reports for all pollutants, by year and each of the three audit seasons, for plants in the treatment and control groups. Pollution in audit reports in the control group is more or less flat at -0.3 standard deviations for all seasons. In the treatment group, pollution reports are generally well above those in the control, and show an interesting pattern in time. Over the course of the first year, pollution levels in treatment audits decline with each season, and over the second year they also decline, somewhat less sharply. Between years, pollution levels in the treatment jump up sharply. This jump is coincident with the introduction of incentive pay and consistent with incentive pay having an independent effect on the accuracy of audit reports.

This finding is documented statistically in Table VI. Column (1) duplicates the base specification of Panel B, Table V for reference, showing the coefficient on the year two (2010) indicator. In column (2) we include as additional covariates the interaction of that dummy with the treatment, a linear trend in fractional years since January 1, 2009 and the interaction of the linear trend with a treatment indicator. The interaction aims to capture treatment-induced emission changes throughout the two years.

Three findings in the Table echo those of Figure 3. First, as already seen in Table V, Panel A the year one treatment features are associated with an immediate increase in the pollution levels in audit reports. Second, the interaction of the treatment indicator and the year variable, which is fractional in the date of visits and therefore runs continuously from zero to two, shows a significant downward trend in reported pollution among treatment plants. This finding foreshadows the finding in the next section that the treatment caused plants to reduce their emissions. In contrast, there is no declining trend in the control group. Third, there is a discrete increase in pollution levels in audit reports in the treatment group, relative to the control, at the beginning of year two that interrupts the relative decline of reports in the treatment. Specifically, treatment pollution levels in audit reports are a statistically significant 0.257 standard deviations higher in year two, when incentive pay was in effect, compared to year one. This discrete increase is robust to alternative functional form assumptions about the time trend.[24] These results suggests that the incentive pay component of the treatment led auditors to report higher, more accurate audit readings.

Table 2 in the Online Appendix provides additional, but very tentative, evidence that the risk of being backchecked may also have had an independent effect on auditor behavior. To this end, we ask whether an audit firm that was backchecked recently reports differently, using data from the midline (conducted over the last season of audit visits in the experiment). When this midline began, auditors had been backchecked regularly for nearly two years in the treatment, but not backchecked at all in the control. The table shows that having recently been backchecked during the midline increases the accuracy of reporting in control plants but not

[24]The addition of quadratic and cubic trends and their interaction with the treatment indicators causes the discrete jump in reporting in year two to increase to 0.284 standard deviations (standard error 0.116 standard deviations).

in treatment plants. We find this consistent with auditors updating their beliefs, to recognize that backchecks were possible in control plants, and improving reporting in response.

## C The Effect of the Treatment on Plant Emissions

Given the observed increase in truth-telling, a natural next question is whether plant abatement behavior responded to more accurate auditor reporting. We expect plants to respond only if truthful reports on high pollution will cost them more, through regulatory sanctions, than pollution abatement.

Our analysis utilizes data on plant-level pollution emissions from the endline survey, which is described in Section III. Our sample is all surveyed plants, not just those which filed audit reports. The advantage of this broader sample is that the audit treatment may have affected the emissions of all plants, not just those that chose to submit audit reports (and high polluters may be most likely to not submit). Further, the type of self-selection that suggested selection corrections for audit reporting is absent in this data because plants cannot influence selection into the endline sample.

To measure the effect of the treatment on pollution emissions, we report OLS regressions like equation 3, but with a single cross-section of endline survey data on pollution outcomes. The outcome variables are both the continuous pollution outcome and a compliance dummy. Further, we also estimate quantile regressions of the form

$$Q_{y_{ij}|X_j}(\tau) = \beta T_j + \alpha_r + \epsilon_{ij},$$

where $Q(\tau)$ is the $\tau$-quantile of the pollutant concentration conditional on treatment status and regional indicator variables.

Table VII reports the results. In Panel A, the outcome variable is a standardized measure of pollution emissions that is calculated in the same manner as in the auditor accuracy tables above. On average, the treatment plants reduced pollution by a statistically significant 0.211 standard deviations.[25] This effect is driven by a large decrease of 0.300 standard deviations

[25] The coefficient for all pollutants is due in part to several control plants with very high pollution readings. It decreases in magnitude to -0.143 standard deviations (standard error 0.068 standard deviations) and -0.114 (standard error 0.058 standard deviations) when the readings are top-coded above the 99.5 and 99 percentiles of the pollutant distribution, respectively. There are, however, several pieces of corroborating evidence that these readings are genuine and should not be top-coded. The plants in question have track records of noncompliance, and other pollutant samples collected at the same plants, both before the high endline pollutant readings and

in water pollutant concentrations, shown in column (2). The estimated effect on air pollution in column (3) is smaller and insignificant. Since the volume of effluent emitted did not change in response to the experimental treatments, these reductions in concentrations represent reductions in the total emitted effluent load—i.e. less water pollution—among treatment plants.

In Panel B, the outcome variable is whether the pollutant reading is compliant, i.e. below the regulatory standard. In column (1), compliance is estimated to have increased by a small and insignificant 2.68 percentage points. Columns (2) and (3) indicate that the effects are similar for water and air pollution. The fact that reductions in pollution did not increase compliance suggests that these reductions were concentrated among plants with pollution levels far from the regulatory threshold.

To explore the possibility that pollution reductions are coming from high-polluting plants, Figure 4 plots quantile treatment effects from quantile regressions of standardized endline pollutant levels on audit treatment and region fixed effects. Quantile effects are estimated from the 0.05-quantile to the 0.95-quantile at 0.05-quantile intervals. The shaded area is the 95% confidence interval. While no individual quantile coefficient is significant at conventional levels, the point estimates show a clear pattern wherein the treatment reduced pollution more at higher quantiles of the pollution distribution. Up to the 0.75-quantile, the point estimates are very close to zero, but from the 0.80-quantile onwards the point estimates sharply decrease to less than -0.5 standard deviations at the 0.95-quantile. It is evident that the mean reduction in pollution is largely a consequence of reductions in the right tail of the pollutant distribution.

This pattern of pollution reductions, like the pattern in auditor reporting, appears to be related to the GPCB penalty structure. Figure 5 investigates how GPCB's regulatory actions correspond to the degree of observed pollution violations. We consider GPCB's regulatory follow-up to its own inspections, as observed during the three years beginning the year before the study and running through its end. The figure plots how often GPCB takes different types of regulatory actions in response to observing pollution at different levels above the pollution standard, measured during GPCB inspections of audit sample plants. We classify follow-up in four categories of increasing severity from the bottom (dark grey bars) to the top (light grey bars): a letter is official but not legal correspondence to the firm noting the violation

at the endline, also show pollution levels far above average.

and possibly threatening some action; a citation is a legal notice requiring a response from the firm; a closure is a warning that the plant will be closed unless a violation is immediately remedied; and a disconnection is an order to the utility that a plant's power be disconnected.

The severity of GPCB's actions increases monotonically in the amount by which the standard is exceeded and sharply only for very high violations, suggesting that GPCB does not keep to the (perhaps excessively) ambitious standard, and pursues the most egregious cases. For example, plants polluting at above the standard but less than 1.5 times the standard receive the most severe actions (i.e., closure warning and disconnection) in less than 10% of the cases, and even plants between two and five times the standard less than 40% of the time. In contrast, plants with readings higher than ten times the standard receive these actions more than 70% of the time.

Thus the most costly sanctions are, in practice, reserved for the right tail of the pollutant distribution. This relationship between high pollution concentrations and likely penalties is a logical explanation for why the treatment, which broadly improved the quality of auditor reporting, induced only high-polluting plants to clean up. All else equal, these firms have a much higher expected fine once the auditors' report are accurate.[26]

## V Conclusion

This paper reports on a large-scale reform to the third-party environmental audit system in the Indian state of Gujarat, conducted in collaboration with the environmental regulator there. The goal was to change the structure of the audit market to incentivize accurate reporting and ultimately pollution abatement. The treatment consisted of random assignment of auditors to plants, payments to auditors at a fixed rate from a central pool (rather than from the plant), random backchecks of auditors, and, in the second year, a bonus for accurate reporting.

There are three primary findings. First, the status quo audit system appears corrupted, with auditors systematically underreporting the pollution emissions of control plants at levels just below the regulatory standard. Second, the treatment greatly increased the accuracy of auditor reporting, viewed in terms of compliance, levels, or differences with backchecks. This

[26]Compared to the results in Panel A of Table VII, the effect of the treatment on pollution emissions is smaller and insignificant in the selected sample of plants that submitted audits. This reduction in the treatment effect supports the idea, introduced in the audit reporting results above, that especially dirty plants do not submit audit reports in the control group. Indeed, the GPCB's penalty structure that reserves the harshest penalties for plants that greatly exceed the regulatory standard sets incentives that induce this selective audit reporting.

finding is robust to the inclusion of auditor fixed effects that compare the behavior of the same auditors working in both treatment and control plants. Third, treatment plants reduced their pollution emissions. This decrease largely comes from water pollutants which were the original spur for the development of the audit scheme and remain regulatory priorities. Further, the reductions are concentrated entirely in the highest polluting plants, which face the greatest risk of regulatory sanction.

We attribute these results to the package of random assignment of auditors to plants, fixed fees paid from a central pool, increased monitoring, and bonus pay for accuracy together creating incentives for accurate reporting. Although we cannot separately identify the influence of each treatment component, we make several observations on what underlies the treatment effects. First, where we have some variation, the non-experimental evidence suggests that the incentive pay increased the propensity of auditors to accurately report. Second, fixed effects estimates show that the treatment causes within-auditor changes in reporting. These estimates suggest that auditor-level effects, such as income or Hawthorne effects, do not drive the treatment impact. Third, economic intuition and existing evidence from the literature on corruption and monitoring suggest that higher auditor pay without the other treatment components would not have changed auditor behavior. Without random assignment or monitoring, rational auditors would pocket any increase in payments or kick it back to their clients rather than improve audit quality.

These results are encouraging in the context of environmental regulation in India. A critical regulatory challenge is gathering accurate information in the face of agency problems either in third-party reporting or within the regulator itself. Like the environmental audit system in Gujarat, the national system for Environmental Impact Assessments (EIA) has foundered on this problem, because, as put by a former Minister of Environment, "[T]he person who is putting up the project will be preparing the report" (The Hindu, 2011). The reform we study here shows it is feasible for regulators to collect accurate information on pollution emissions. Further, at least in the cases where the regulator is known to assign strict penalties, the provision of this information to the regulator causes the plants most likely to be penalized to reduce their emissions.

More broadly, we consider whether the results here are relevant to the many other settings where third-party audits are used as a regulatory tool. We believe that the core problem—that auditors face a conflict of interest, or, at least, poor incentives to tell the truth—is present in all existing third-party audit markets. Indeed, we are unaware of a single instance where the audited party does not directly hire the auditor. At least in the case of the Gujarat environmental audit market, this market structure produced very unreliable audit reports. In the same case, however, we find that a politically and logistically feasible set of reforms can greatly improve market outcomes. The reform package we test is unlikely to be the optimal contract for auditors, which would balance the costs of increased monitoring, auditor effort and pollution abatement against the benefits of lower pollution. We only measure its impact over two years, and it is possible that, in the longer run, things would unravel, for example if the backcheckers began colluding with auditors. Nonetheless, the reform model incorporates the main elements of independence, monitoring and incentives that would shape any optimal system, and our experiment demonstrates their value in changing behavior towards regulatory goals. A logical conclusion is that restructuring other third-party audit markets to better incentivize auditors to tell the truth has great potential to raise the quality of auditing.

The exact nature of any audit reform will reflect a particular market's status quo functioning and institutional details. For example, in complex settings with a fixed cost of establishing an auditor-client relationship, such as the cost of a financial auditor learning a client's books, assignment or rotation of auditors to firms for periods longer than one year may be preferable. One of the greatest differences between markets for auditors appears to be the strength of reputation effects, with concentrated market structures giving better incentives for high-quality audits even when auditors are hired by the firms on which they report. Recent history in the United States, however, suggests that even very concentrated markets, in the absence of regulatory oversight, do not suffice to keep the quality of reporting reliably high. This underscores the value of designing systems where the first order incentive is for auditors to tell the truth. Our study suggest that, in at least some instances, it may be possible to do so.

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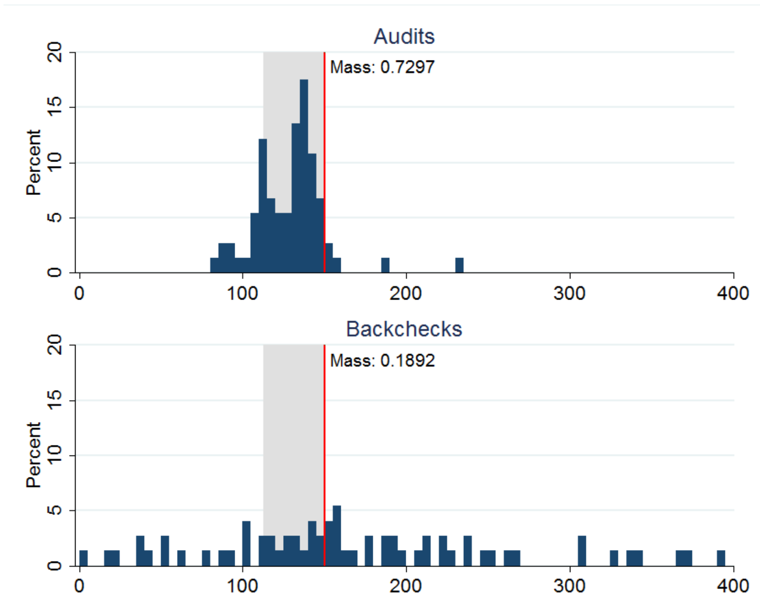
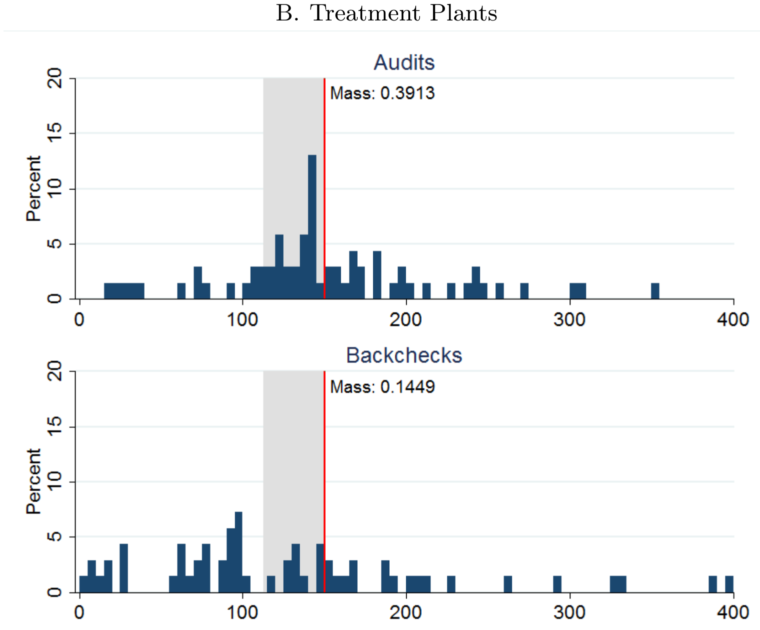
# VI Figures

Figure 1: Readings for Suspended Particulate Matter (SPM, mg/Nm$^3$), Midline

A. Control Plants

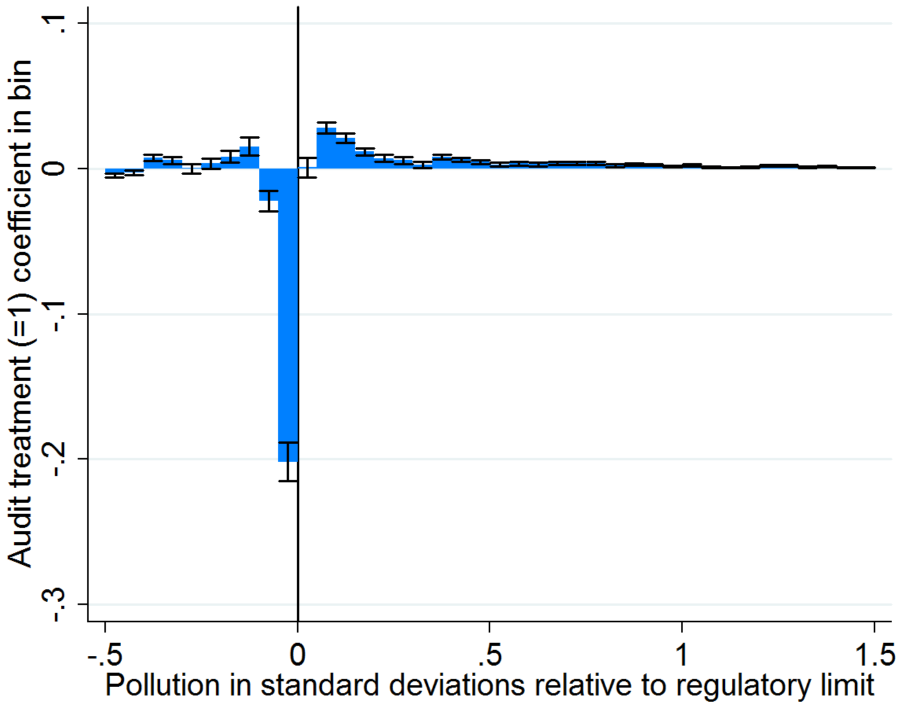



B. Treatment Plants



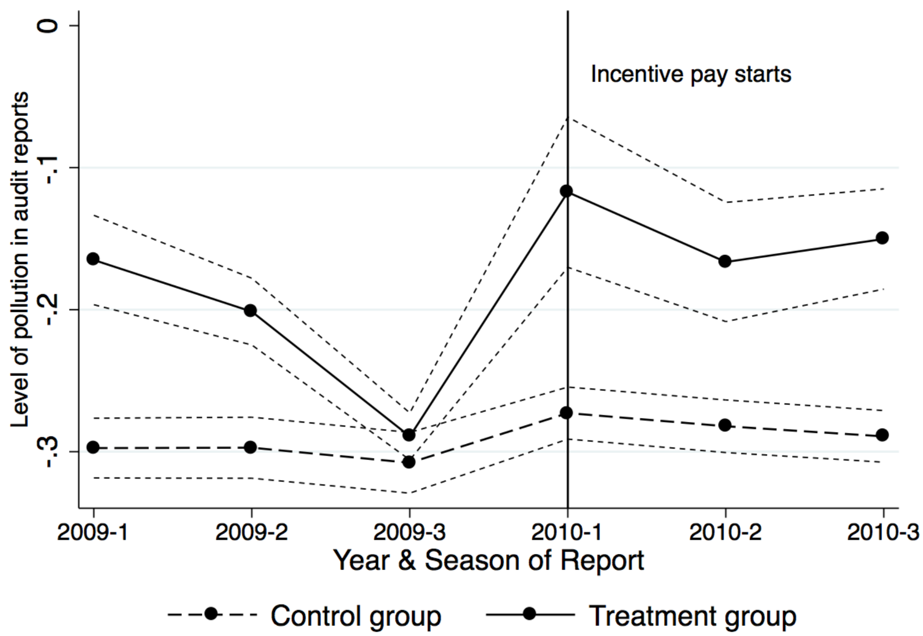

The figure shows distributions of pollutant concentrations for Suspended Particulate Matter (SPM) in boiler-stack samples taken during the midline survey. Panel A shows the distributions of readings at control plants from audits and backchecks, respectively, and Panel B readings at treatment plants from the same two sources. The regulatory maximum concentration limit of 150 $mg/Nm^3$ for SPM is marked with a vertical line and the area between 75% and 100% of the limit is shaded in gray.

Figure 2: Audit Treatment Effect in Density Bins, All Pollutants



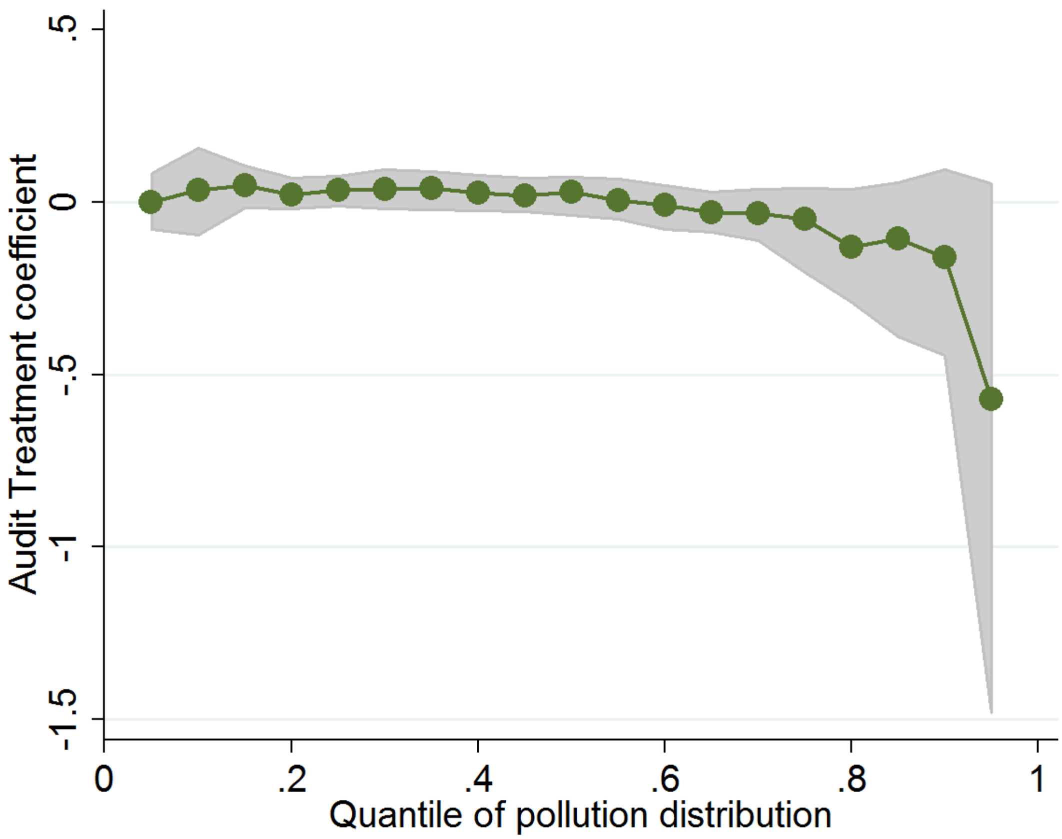

The figure reports point estimates and standard errors from 40 OLS regressions where the dependent variables are indicators for a pollutant reading being within a given density bin and region fixed effects and the independent variable is audit treatment. All pollutants are included with $Water = \{NH_3\text{-N}, \text{BOD}, \text{COD}, \text{TDS}, \text{TSS}\}$ and $Air = \{SO_2, NO_x, \text{SPM}\}$ and $All = Water \cup Air$. Pollutants are standardized by subtracting the regulatory standard for each pollutant and dividing by the standard deviation in backchecks of that pollutant. Density bins are 0.05 standard deviations wide.

Figure 3: Time Series of Audit Reports by Treatment Status, All Pollutants



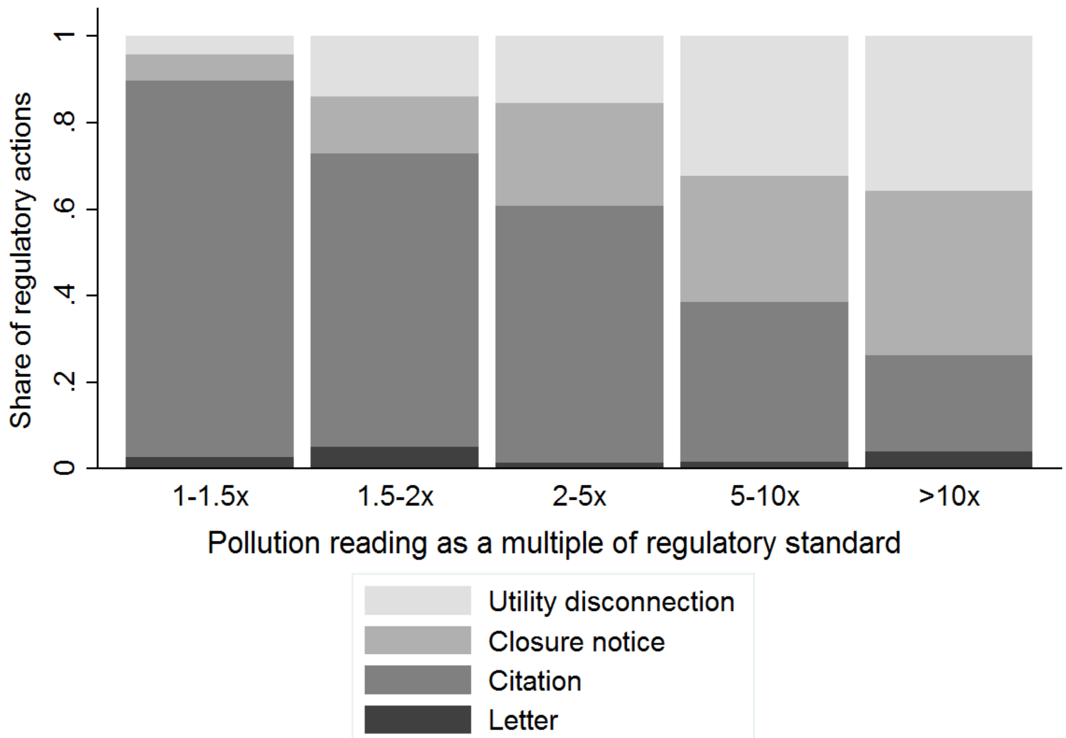

The figure reports the mean standardized pollution level reported in audits by time and treatment status. Time is divided into two years and the three seasons of the year in which auditors are required to monitor pollution. All pollutants are included with $Water = \{NH_3\text{-N}, \text{BOD}, \text{COD}, \text{TDS}, \text{TSS}\}$ and $Air = \{SO_2, NO_x, \text{SPM}\}$ and $All = Water \cup Air$. Pollutants are standardized by subtracting the mean for each pollutant and dividing by the standard deviation, where both statistics are calculated from backchecks of that pollutant.

Figure 4: Quantile Treatment Effects of Audit on Endline Pollution




The figure reports estimates from quantile regressions of standardized endline pollution for all pollutants on a dummy for audit treatment assignment and region fixed effects in the audit sample of plants not subject to the cross-cut experimental treatment, analogous to the OLS specifications in Table VII. The quantiles are from 0.05-quantile to the 0.95-quantile at 0.05-quantile intervals. The confidence intervals shown are at the 95% level from a cluster-bootstrap at the plant level with 200 replications.

Figure 5: Regulatory Actions by Degree of Violation




The figure reports the regulatory responses to pollution readings measured at different levels of noncompliance during regulatory inspections for audit sample plants over the three years beginning one year prior to the study. Pollutant readings are shown in bins of readings at specified multiples above the regulatory standard. The bars indicate the type of regulatory action taken in response to a given reading. Actions increase in severity from bottom (dark bars) to top (light bars): a letter is official but not legal correspondence to the firm noting the violation and possibly threatening action, a citation is a legal regulatory notice requiring a response from the firm, a closure is a warning that the plant will be closed unless a violation is remedied, and a disconnection is an order to the utility that a plant's power be disconnected. All of these actions were coded based on complete administrative records of plant interactions with the regulator. Going left to right across the bars, the number of violating plants (actions) used to calculate the action shares at each degree of violation are 153 (305), 102 (159), 141 (178), 70 (120) and 72 (126).

## VII Tables

TABLE I
Submission of Audit Reports

| | Treatment (1) | Control (2) | Difference (3) |
|---|---|---|---|
| | *Panel A. 2009* | | |
| Audit submitted | 163 | 177 | |
| Total plants | 233 | 240 | |
| Share submitted | 0.70 | 0.74 | -0.038 |
| | | | (0.041) |
| | *Panel B. 2010* | | |
| Audit submitted | 164 | 153 | |
| Total plants | 233 | 240 | |
| Share submitted | 0.70 | 0.64 | 0.066 |
| | | | (0.043) |

The table reports on the number of audit reports submitted to the regulator for plants in the audit sample over the two years of the experiment. Column (3) shows differences between treatment and control group submission rates with standard errors in parentheses. $^{*}$ $p < 0.10$ , $^{**}$ $p < 0.05$ , $^{***}$ $p < 0.01$ .

TABLE II
Audit Treatment Covariate Balance

| | Treatment (1) | Control (2) | Difference (3) |
|---|---|---|---|
| *Panel A. Plant Characteristics* | | | |
| Capital investment INR 50m to 100m (=1) | 0.092 | 0.14 | -0.051 |
| | [0.29] | [0.35] | (0.033) |
| Located in industrial estate (=1) | 0.57 | 0.53 | 0.042 |
| | [0.50] | [0.50] | (0.051) |
| Textiles (=1) | 0.88 | 0.93 | -0.030 |
| | [0.33] | [0.26] | (0.025) |
| Effluent to common treatment (=1) | 0.41 | 0.35 | 0.078 |
| | [0.49] | [0.48] | (0.049) |
| Wastewater generated (kl / day) | 420.5 | 394.6 | 35.4 |
| | [315.9] | [323.4] | (31.6) |
| Lignite used as fuel (=1) | 0.71 | 0.77 | -0.024 |
| | [0.45] | [0.42] | (0.029) |
| Diesel used as fuel (=1) | 0.29 | 0.25 | 0.038 |
| | [0.45] | [0.43] | (0.046) |
| Air emissions from flue gas (=1) | 0.85 | 0.87 | -0.0095 |
| | [0.35] | [0.33] | (0.016) |
| Air emissions from boiler (=1) | 0.93 | 0.92 | 0.026 |
| | [0.26] | [0.27] | (0.027) |
| Bag filter installed (=1) | 0.24 | 0.34 | -0.10** |
| | [0.43] | [0.47] | (0.046) |
| Cyclone installed (=1) | 0.087 | 0.079 | 0.0010 |
| | [0.28] | [0.27] | (0.027) |
| Scrubber installed (=1) | 0.41 | 0.41 | -0.018 |
| | [0.49] | [0.49] | (0.050) |
| *Panel B. Regulatory Interactions in Year Prior to Study* | | | |
| Whether audit submitted (=1) | 0.82 | 0.81 | 0.022 |
| | [0.38] | [0.39] | (0.038) |
| Any equipment mandated (=1) | 0.42 | 0.49 | -0.047 |
| | [0.50] | [0.50] | (0.047) |
| Any inspection conducted (=1) | 0.79 | 0.78 | 0.016 |
| | [0.41] | [0.42] | (0.042) |
| Any citation issued (=1) | 0.28 | 0.24 | 0.035 |
| | [0.45] | [0.43] | (0.045) |
| Any water citation issued (=1) | 0.12 | 0.12 | -0.0031 |
| | [0.33] | [0.33] | (0.034) |
| Any air citation issued (=1) | 0.027 | 0.0052 | 0.021* |
| | [0.16] | [0.072] | (0.013) |
| Any utility disconnection (=1) | 0.098 | 0.094 | 0.0029 |
| | [0.30] | [0.29] | (0.031) |
| Any bank guarantee posted (=1) | 0.033 | 0.026 | 0.0045 |
| | [0.18] | [0.16] | (0.017) |
| Observations | 184 | 191 | |

The sample includes firms in the audit sample that submitted an audit report in either year; the balance for all audit sample firms, discussed in the text, is similar. Columns (1) and (2) show means with standard deviations in brackets. Column (3) shows the coefficient on treatment from regressions of each characteristic on treatment and region fixed effects. 50 INR ≈ 1 USD. $^{*}$ $p < 0.10$ , $^{**}$ $p < 0.05$ , $^{***}$ $p < 0.01$ .

TABLE III

Compliance in Audits Relative to Backchecks, Control Group Only

| | All pollutants (1) | Water pollutants (2) | Air pollutants (3) |
|---|---|---|---|
| *Panel A. Dependent variable: Narrow compliance (Dummy for pollutant between 75% and 100% of regulatory standard)* | | | |
| Audit report (=1) | 0.270*** | 0.297*** | 0.230*** |
| | (0.025) | (0.034) | (0.033) |
| Control mean in backchecks | 0.097 | 0.110 | 0.077 |
| *Panel B. Dependent variable: Compliance (Dummy for pollutant at or below regulatory standard)* | | | |
| Audit report (=1) | 0.288*** | 0.273*** | 0.311*** |
| | (0.023) | (0.033) | (0.032) |
| Control mean in backchecks | 0.557 | 0.538 | 0.586 |
| Observations | 1132 | 688 | 444 |

Regressions include region fixed effects. "Audit report" is a dummy for a pollutant reading reported in an audit, as opposed to reported in a backcheck, which is the omitted category. Sample of matched pollutant pairs from audit reports submitted to the regulator in the control group only and corresponding backchecks from the midline survey. Pollution samples from final-stage effluent outlet for water and boiler-stack for air. Pollutants included are $Water$ = {$NH_3$-N, BOD, COD, TDS, TSS} and $Air$ = {$SO_2$, $NO_x$, SPM} with $All = Water \cup Air$. Standard errors clustered at the plant level in parentheses. $^{*}$ $p < 0.10$ , $^{**}$ $p < 0.05$ , $^{***}$ $p < 0.01$ .

TABLE IV

Compliance in Audits Relative to Backchecks by Treatment Status

| | All pollutants (1) | Water pollutants (2) | Air pollutants (3) |
|---|---|---|---|
| *Panel A. Dependent variable: Narrow compliance (Dummy for pollutant between 75% and 100% of regulatory standard)* | | | |
| Audit report × Treatment group | -0.185*** | -0.212*** | -0.143*** |
| | (0.034) | (0.044) | (0.046) |
| Audit report (=1) | 0.270*** | 0.297*** | 0.230*** |
| | (0.025) | (0.034) | (0.033) |
| Treatment group (=1) | -0.0034 | -0.013 | 0.011 |
| | (0.0176) | (0.025) | (0.024) |
| Control mean in backchecks | 0.097 | 0.110 | 0.077 |
| *Panel B. Dependent variable: Compliance (Dummy for pollutant at or below regulatory standard)* | | | |
| Audit report × Treatment group | -0.234*** | -0.166*** | -0.345*** |
| | (0.039) | (0.050) | (0.056) |
| Audit report (=1) | 0.288*** | 0.273*** | 0.311*** |
| | (0.023) | (0.033) | (0.032) |
| Treatment group (=1) | 0.058* | 0.0075 | 0.145*** |
| | (0.034) | (0.0477) | (0.041) |
| Control mean in backchecks | 0.557 | 0.538 | 0.586 |
| Observations | 2236 | 1378 | 858 |

Regressions include region fixed effects. "Treatment group" is a dummy equal to one for plants where auditors were randomly assigned, paid a fixed rate from a common pool and subject to bakchecks. "Audit report" is a dummy for a pollutant reading reported in an audit, as opposed to reported in a backcheck, which is the omitted category. Sample of matched pollutant pairs from audit reports submitted to the regulator and corresponding backchecks from the midline survey, in both the treatment and control groups. Pollution samples from final-stage effluent outlet for water and boiler-stack for air. Pollutants included are $Water = \{NH_3\text{-N}, BOD, COD, TDS, TSS\}$ and $Air = \{SO_2, NO_x, SPM\}$ with $All = Water \cup Air$. Standard errors clustered at the plant level in parentheses. $^{*}\ p < 0.10$ , $^{**}\ p < 0.05$ , $^{***}\ p < 0.01$ .

TABLE V

AUDIT TREATMENT EFFECTS ON AUDITOR REPORTING

| | All pollutants | | Water pollutants | | Air pollutants | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| *Panel A. Dependent variable: Compliance (Dummy for pollutant in audit report at or below regulatory standard)* | | | | | | |
| Treatment group (=1) | -0.151*** | -0.180*** | -0.172*** | -0.188*** | -0.108*** | -0.149*** |
| | (0.015) | (0.022) | (0.021) | (0.030) | (0.016) | (0.021) |
| Auditor fixed effects | No | Yes | No | Yes | No | Yes |
| Control mean | 0.828 | 0.828 | 0.807 | 0.807 | 0.863 | 0.863 |
| Observations | 13170 | 13170 | 8373 | 8373 | 4797 | 4797 |
| *Panel B. Dependent variable: Level of pollutant in audit report* | | | | | | |
| Treatment group (=1) | 0.103*** | 0.131*** | 0.117** | 0.131** | 0.0853*** | 0.132*** |
| | (0.035) | (0.038) | (0.053) | (0.059) | (0.0214) | (0.022) |
| Auditor fixed effects | No | Yes | No | Yes | No | Yes |
| Control mean | -0.291 | -0.291 | -0.350 | -0.350 | -0.194 | -0.194 |
| Observations | 13170 | 13170 | 8373 | 8373 | 4797 | 4797 |
| *Panel C. Dependent variable: Level of pollutant in audit report minus level of pollution in backcheck* | | | | | | |
| Treatment group (=1) | 0.210*** | 0.153 | 0.152 | 0.156 | 0.312*** | 0.166* |
| | (0.073) | (0.099) | (0.102) | (0.138) | (0.083) | (0.095) |
| Auditor fixed effects | No | Yes | No | Yes | No | Yes |
| Control mean | -0.304 | -0.304 | -0.354 | -0.354 | -0.225 | -0.225 |
| Observations | 1118 | 1118 | 689 | 689 | 429 | 429 |

Regressions include region fixed effects in all Panels and year fixed effects in Panels A and B only. Pollution samples are from the final-stage effluent outlet for water and boiler-stack for air. Pollutants included are $Water$ = {$NH_3$-N, BOD, COD, TDS, TSS} and $Air$ = {$SO_2$, $NO_x$, SPM} with $All = Water \cup Air$. Panels A and B use the full sample of audit reports that reached the regulator over the two years of the experiment. The outcome variable is a dummy for whether a pollutant was reported compliant in audit reports, in Panel A, and the level of pollution reports, in Panel B. Panel C uses the midline survey sample with pollution in audit reports less pollution in backchecks as the outcome. Standard errors clustered at the plant level are in parentheses. $^{*}$ $p < 0.10$ , $^{**}$ $p < 0.05$ , $^{***}$ $p < 0.01$ .

TABLE VI

INCENTIVE PAY FROM TREATMENT EFFECT OVER TIME

| | Level of pollutant in audit report, all pollutants | |
|---|---|---|
| | (1) | (2) |
| Treatment group (=1) | 0.103*** | 0.218*** |
| | (0.035) | (0.063) |
| Incentive pay (year=2010) | 0.051** | 0.002 |
| | (0.027) | (0.033) |
| Incentive pay X treatment | | 0.257*** |
| | | (0.092) |
| Years (fractional) from Jan 1, 2009 | | 0.029 |
| | | (0.023) |
| Years (fractional) X treatment | | -0.220*** |
| | | (0.068) |
| Region fixed effects | Yes | Yes |
| Observations | 13166 | 13166 |

Regressions include region fixed effects. Pollution samples from final-stage effluent outlet for water and boiler-stack for air. Sample of all audit reports to the regulator over the two years of the experiment. Pollutants included are $Water$ = {$NH_3$-N, BOD, COD, TDS, TSS} and $Air$ = {$SO_2$, $NO_x$,SPM} with $All = Water \cup Air$. Standard errors clustered at the plant level in parentheses. $^{*}$ $p < 0.10$ , $^{**}$ $p < 0.05$ , $^{***}$ $p < 0.01$ .

TABLE VII

Endline Pollutant Concentrations on Treatment Status

| | All pollutants (1) | Water pollutants (2) | Air pollutants (3) |
|---|---|---|---|
| *Panel A. Dependent variable: Level of pollutant in endlien survey* | | | |
| Audit treatment assigned (=1) | -0.211** | -0.300* | -0.053 |
| | (0.099) | (0.159) | (0.057) |
| Control mean | 0.076 | 0.114 | 0.022 |
| Observations | 1439 | 860 | 579 |
| *Panel B. Dependent variable: Compliance (Dummy for pollutant in endline survey at or below regulatory standard)* | | | |
| Audit treatment assigned (=1) | 0.027 | 0.039 | 0.00175 |
| | (0.027) | (0.039) | (0.0282) |
| Control mean | 0.573 | 0.516 | 0.656 |
| Observations | 1439 | 860 | 579 |

Regressions include region fixed effects. Pollution samples from final-stage effluent outlet for water and boiler-stack for air. Pollutants included are $Water = \{NH_3$-N, BOD, COD, TDS, TSS$\}$ and $Air = \{SO_2, NO_x, SPM\}$ with $All = Water \cup Air$. Endline survey data in the audit sample of plants not subject to the cross-cut experimental treatment. Standard errors clustered at the plant level in parentheses. $^{*}$ $p < 0.10$ , $^{**}$ $p < 0.05$ , $^{***}$ $p < 0.01$ .

# A Data Appendix

TABLE A1
Pollutant Descriptions

| Pollutant | Description |
|---|---|
| | *Panel A: Water Pollutants* |
| Biochemical Oxygen Demand (BOD) | A measure of the amount of dissolved oxygen consumed by microscopic organisms in a confined sample of water. The BOD and volume of an effluent determine the oxygen demand that will be imposed on receiving waters (Boyd, 2000). The demand for oxygen from effluent may deplete available molecular oxygen, precluding other biological processes, such as marine plants or life, that require oxygen (Waite, 1984). |
| Chemical Oxygen Demand (COD) | A measure of the oxygen demand of the organic matter in a sample as determined by oxidation of the organic matter with potassium dichromate and sulfuric acid. Often used as a proxy for BOD in determining the oxygen demand of effluent. |
| Total Dissolved Solids (TDS) | Primarily inorganic substances dissolved in water, including calcium, magnesium, sodium, potassium, iron, zinc, copper, manganese, etc. Water with high dissolved solids is said to be mineralized and decreases the survival of plant and animal life, degrades the taste of water, corrodes plumbing and limits use of water for irrigation (Boyd, 2000; IHD-WHO Working Group, 1978). Depending on the composition of solids TDS may have adverse health effects on people with cardiac disease or high blood pressure. |
| Total Suspended Solids (TSS) | Organic and inorganic or mineral particles too large to be dissolved but small enough to remain suspended against gravity in an effluent (Boyd, 2000). Contribute to turbidity and color of water and proxy for adverse effects from individual solid components. |
| Ammoniacal Nitrogen ($NH_3$-N) | The nitrogen contained in unionized ammonia and ammonium. Though nitrogen is a vital nutrient, some forms of ammonia nitrogen are toxic to aquatic life (Boyd, 2000). The toxicity of ammonia nitrogen increases with decreasing dissolved oxygen concentrations. |
| | *Panel B: Air Pollutants* |
| Sulfur Dioxide ($SO_2$) | A reactive oxide of sulfur. Short-term exposure has been linked to adverse respiratory effects particularly damaging for asthmatics. $SO_2$ also contributes to formation of fine particles (World Health Organization, 2006). |
| Nitrogen Oxides ($NO_x$) | A group of reactive gases including nitrous acid, nitric acid and $NO_2$. Nitrogen oxides are toxic at high concentrations and contribute to formation of ozone and fine particles, which are detrimental to health (World Health Organization, 2006). |
| Suspended Particulate Matter (SPM) | A mixture of small particles and liquid droplets with a number of components, including acids, organic chemicals, metals and soil or dust (U.S. Environmental Protection Agency, 2010). Particulate matter affects respiratory and cardiovascular health and has been shown to increase infant mortality and shorten lifespans (World Health Organization, 2006; Currie and Walker, 2011; Chen et al., 2010). |

The table describes the pollutants used throughout the analysis of both auditor reporting and plant pollution emissions. Auditor incentive pay during year two of the experiment was based on these pollutants and the water parameter $pH$. We omit $pH$ from the analysis because environmental damages from $pH$ are not monotonic in the reading—both high (alkanine) and low (acidic) readings can be bad—which makes results for $pH$ incomparable to results for other pollutants. For this same reason, the regulatory standard for $pH$ is a range, rather than a maximum concentration.