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Does the Squeaky Wheel Get More Grease? The Direct and Indirect Effects of Citizen Participation on Environmental Governance in China

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Abstract

We conducted a nationwide field experiment in China to evaluate the direct and indirect impacts of assigning firms to public or private citizen appeals when they violate pollution standards. There are three main findings. First, public appeals to the regulator through social media substantially reduce violations and pollution emissions, while private appeals cause more modest environmental improvements. Second, experimentally adding "likes" and "shares" to social media appeals increases regulatory effort, suggesting visibility as an important mechanism. Third, pollution reductions by treated firms are not offset by control firms, based on randomly varying the proportion of treatment firms at the prefecture-level.

Keywords: citizen participation, social media, environmental governance

JEL: Q52; P26; P28

I.   Introduction

Across the globe, 2.8 billion people breathe air that is considered hazardous by the World Health Organization and 1.5 billion people contend with polluted water. A rapidly growing literature has documented severe consequences for health (Greenstone et al., 2015; Greenstone and Hanna, 2014; Ebenstein et al., 2017), labor productivity (Graff-Zivin and Neidell, 2012; Chang et al., 2016; Adhvaryu et al., 2016), human capital accumulation (Isen et al., 2017; Ebenstein and Greenstone, 2022), and welfare (Kremer et al. 2011; Currie et al., 2015; Ito and Zhang, 2020; Wang and Wang, 2020). At the same time, most countries have strict standards and regulations on the books. It is apparent that the enforcement of regulations is failing in many parts of the world (Greenstone and Jack, 2015; United Nations Environment Program, 2019), but whether that is by design to facilitate economic growth (Greenstone et al., 2012; He et al., 2020) or due to genuine capacity issues (Duflo et al., 2013 and 2018) is largely unknown.

An increasingly popular tool for reducing pollution is to encourage bottom-up participation in environmental governance. Programs that enable citizens, non-governmental organizations, shareholders, and the media to participate in environmental enforcement date back at least to the 1980s when the United States introduced the Toxics Release Inventory (TRI) that required firms to publicly release their toxic emissions. In the subsequent 35 years, similar programs have been implemented in many countries, including Canada, China, India, and Indonesia.[1] Concurrently, many countries created official channels for the public to report pollution and violations of standards.[2] Yet, there has been little rigorous evidence on whether and how citizens can leverage the government-released information to affect the enforcement of pollution standards and emissions at scale.

Additionally, we are unaware of any evidence on the indirect or general equilibrium impacts of bottom-up participation in environmental governance. In principle, the indirect impacts could reduce or even completely undo any direct impacts by reducing regulatory oversight of firms not subject to complaints. Alternatively, public participation could augment the direct effects by causing *all* firms to raise their expectations of the costs of violating pollution standards, leading to reductions in emissions by all firms through deterrence.

This paper evaluates the direct and indirect impacts of public participation in the enforcement of environmental standards in China, the world's largest polluter and manufacturer. A compelling feature of the study's context is that the Ministry of Ecology and Environment (MEE) maintains a Continuous Emissions

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[1] Besides the TRI, specific programs include the Greenhouse Gas Reporting Program (GHGRP) in the US and Canada, the Maharashtra Star Rating Program (MSRP) in India, the Program for Pollution Control, Evaluation, and Rating (PROPER) in Indonesia, and the Continuous Emissions Monitoring System (CEMS) in China.

[2] For example, pollution hotlines and websites to file complaints against pollution violations can be found in countries like the US, the UK, China, Canada, etc.

Monitoring System (CEMS) that automatically collects hourly emissions data for 24,620 major polluting plants nationwide. These plants are responsible for more than 75% of China's total industrial emissions. These data are available in real time both to regulators and the public through provincial CEMS websites, so regulators in principle have all the information they need to detect and punish violations. Despite the widespread availability of these data, environmental compliance remains imperfect: in 2019, more than 33% of the CEMS firms committed pollution violations.

We conducted an eight-month nationwide field experiment that leveraged the CEMS data to file appeals against firms that violated standards. While appeals do not provide new information about the presence of violations, they inform regulators about which violations cause public dissatisfaction. We randomly assigned each CEMS firm to a control arm or one of several treatment arms which mirror the officially sanctioned ways that citizens and non-governmental organizations already reveal their dissatisfaction about pollution to governments. The starting point for implementation was the daily determination of all firms that violated emissions standards the previous day. Specifically, when a treated firm committed a violation, citizen volunteers who cooperated with our experiment filed an appeal through one of two broadly-defined channels: (1) *private appeals* where the citizens complained to the regulator or the firm about the violation in ways that could not be observed by other members of the public; (2) *public appeals* where the citizens complained about the violation on Weibo, a popular Chinese social media platform that is comparable to Twitter. These channels might imply different information to regulators about the nature of public dissatisfaction, the potential for collective action, and the costs associated with providing an unsatisfactory response. In total, the experiment intervened against nearly 3,000 pollution violations. This form of citizen appeal is a regular occurrence in China; for example, annually there are roughly 300,000 appeals about industrial emissions registered with the Ministry of Environmental Protection by citizens and NGOs. Importantly, this experiment operated separately from the Government's regulatory efforts, so our estimates are informative of the way that the regulators respond to citizen complaints within existing regulatory practices.

Moreover, following Crépon et al. (2013), we cross-randomized treatment intensity at the prefecture level to assess whether indirect or general equilibrium consequences of appeals would offset their direct or partial equilibrium effects. In 60% of the prefectural cities, we assigned 95% of the CEMS firms to treatment arms, while in the remaining 40% of prefectural cities, we assigned 70% of the firms to treatments. Besides the direct effect on violators in the treatment group, the treatment could influence the behavior of treatment group firms that do not violate standards and the entire control group. On the one hand, if firms believe regulatory effort is zero sum due to limited capacity, then the firms that were not targeted may increase emissions. On the other hand, if *all firms* in the 95% prefectures believe that regulatory oversight has generically

increased, relative to firms in the 70% prefectures, then even firms that were not targeted will reduce their emissions. Whether one of these forces dominates is ultimately an empirical question.

There are three primary findings. First, public appeals on social media significantly reduced firms' subsequent violations and emissions, while private appeals to regulators and firms had a more modest impact. Specifically, the public appeals treatment arm reduced violations by more than 60%, relative to the control group. Additionally, over the 8-month study period the public appeals treatment caused sulfur dioxide (SO_2) emission concentrations to decline by 12.2% and reduced chemical oxygen demand (COD) emission concentrations by 3.7% relative to the control group. In contrast, even when using essentially the same content and wording as the public appeals, private appeals only caused approximately a 25% reduction in violations relative to the control group. The violations and emissions reductions were concentrated among the firms that frequently violated the standard prior to the experiment, especially those that significantly exceeded it.

Second, experimentally increasing the visibility of appeals about a violation increases oversight by regulators, highlighting that more public attention and greater scrutiny motivates regulatory effort. Specifically, we randomized the visibility of social media appeals about a violation by experimentally adding 10 additional "likes" and "shares" to the Weibo post, which would otherwise obtain 0.66 "likes" and "shares" naturally. This intervention caused regulators to become significantly more responsive: the probability of a reply to appeals increased by 40%, the length of written replies to appeals doubled, and the probability of an onsite investigation jumped by nearly 65%. In contrast, we fail to find evidence of several other potential mechanisms, including that the treatments generated more appeals by other citizens outside of the experiment or that they caused firms to manipulate CEMS data.

Third, across a mix of outcomes, we find that the general equilibrium impacts do not offset the partial equilibrium effects; if anything, the interventions might have created positive spillover effects. For example, the relative probability of a violation was either unchanged or lower among the control and treatment firms in the high-intensity prefectures, although the reduced violation rates are not always statistically significant by conventional criteria. Overall, accounting for both general and partial equilibrium effects, we find that the ambient SO_2 concentrations declined by 3.5% in the 95% treatment prefectures relative to the 70% treatment prefectures (significant at 10% level).

This paper makes four contributions. First, this nationwide experiment allows us to offer unique insights into when and how citizen participation affects governance at scale. While citizen participation has long been promoted as the key to improving government accountability (Stiglitz, 2002; Mansuri and Rao, 2004; World Bank, 2004), the existing literature has found mixed evidence on its effectiveness (Olken, 2007; Banerjee and Duflo, 2006; Björkman and Svensson, 2009; Banerjee et al., 2010; Grossman et al., 2018; Buntaine et al.,

2021ab; Garbiras-Días and Montenegro 2022). These previous studies have addressed whether monitoring by citizens can improve governance by providing new, decentralized information about governmental performance. A distinctive feature of our study is that the key information on performance is already collected and disclosed by the government itself. This allows us to pin down how information about public demands changes the actions of governments (without confounding it with new information about the underlying governance outcome). Moreover, the unprecedented scale of our experiment permits us to vary the publicity, channel, and target of citizen appeals and thereby helps explain how different types of participation affect the enforcement of regulations by providing different signals of public discontent, at least in the Chinese context. Our paper is also the first to connect citizen participation, regulatory effort, and adjustments by firms, showing how participation can meaningfully decrease pollution.[3]

Second, this paper also relates to the burgeoning literature on social media's political and economic consequences. Existing papers have mostly focused on how social media shapes citizens' political attitudes and motivates political behavior (Bakshy et al., 2015; Bursztyn et al., 2019; Allcott et al., 2020; Yanagizawa-Drott et al., 2021; Garbiras-Días and Montenegro 2022), and how it can foster collective action, both in China (Chen and Yang, 2019; Qin et al., 2021), as well as in other authoritarian regimes globally (Steinert-Threlkeld 2017; Acemoglu et al., 2018; Zhuravskaya et al., 2020; Enikolopov et al., 2020). Adding to this literature, our paper is the first to experimentally study how citizens can leverage social media to hold governments more accountable in policy enforcement, focusing on both partial equilibrium and general equilibrium consequences of using social media for bottom-up citizen participation in governance.[4]

Third, we add to the literature on the political economy of environment. Existing work in this area has mostly focused on the strategic behavior of politicians in determining and implementing environmental policies (List and Sturm, 2006; Kahn et al., 2015; Jia, 2017, Greenstone et al., 2020), or the strategic interactions among local governments over environmental externalities (Burgess et al., 2012; Lipscomb and Mobarak, 2016; He et al., 2020; Wang and Wang, 2020). In this paper, we provide experimental evidence on how pollution appeals by citizens hold local governments in China accountable in enforcing existing environmental standards. More generally, this paper also relates to the literature on the cost and benefit of different environmental policies (Henderson 1996; Greenstone 2002; Walker 2013; Ryan 2012; Kahn and Mansur 2013): our results

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[3] Relatedly, there is also an emerging political science literature on authoritarian responsiveness, which studies the factors that determine whether an authoritarian government replies to citizen's requests (e.g., Chen et al., 2016; Distelhorst and Hou, 2017; Anderson et al., 2019). We add to that literature by showing that the government not only replies to appeals and requests when faced with public pressure, but also takes costly actions that result in actual improvements in government accountability.

[4] In a related paper, Mei and Wu (2022) use observational data to document how social media discussions of a vaccine-related scandal in China led to more transparent procurement of vaccines. Our paper echoes these findings in the context of environmental regulation enforcement, and our experimental design allows us to further compare the effectiveness of social media participation to other channels of public participation, as well as investigating the associated general equilibrium effects.

demonstrate that mobilizing the public to engage in monitoring the performance of governments and firms might be a cost-effective way to improve compliance with existing environmental laws.[5]

Fourth, this paper bridges two strands of literature on the political economy of China's local governance model. The existing literature has pointed out that local governments in China have incentives to facilitate growth and provide support to the firms through both formal and informal institutions (Qian and Weingast, 1997; Xu, 2011; Bai et al., 2020, Liu et al., 2022); and when dealing with the citizens, Chinese local governments have strong incentives to maintain local stability (Chen, 2012; Lorentzen, 2013; Campante et al., 2019, Qin et al., 2021; Beraja et al., 2021; Wang and Yang, 2022). Our paper connects these two lines of literature by documenting the interactions between the state-citizen relationship and the state-business relationship: when the public gets more involved in China's process of environmental governance, the regulatory relationship between government and polluting firms is reshaped; the result is increased governmental effort and lower pollution emissions by firms. Investigating the interactions between firms, citizens, and the state in a synthesized framework deepens our understanding of China's system for local governance.

The remainder of this paper proceeds as follows. Section II introduces the institutional background. Section III discusses our experiment and data. Section IV presents the main empirical findings, while Section V investigates the underlying mechanisms. Section VI discusses the general equilibrium effects. Section VII concludes.

II. Institutional Background

This section describes the institutional background for the field experiment. Section II.A details the encouragement and development of citizen participation in environmental governance in China. Section II.B discusses China's Continuous Emissions Monitoring System (CEMS). The section concludes with II.C, summarizing our qualitative learnings about the dynamics between China's citizens, environmental regulators, and polluters and how we used them to develop hypotheses about which types and forms of citizen appeals might be effective in reducing environmental emissions.

A. Public Participation in China's Environmental Governance

China's unprecedented economic growth since the 1980s is accompanied by rapid industrialization and significant environmental degradation. The severe water and air pollution has led to increasing public discontent and social unrest across the country (Jing, 2000; Steinhardt and Wu, 2016), incentivizing the

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[5] Relatedly, this paper clarifies the pathways by which transparency and information disclosure affect government and firm behavior. Increasing the amount of information disclosed to the public has become a common policy to improve regulatory and government performance (Gavazza and Lizzeri 2007; Mattozzi and Merlo 2007; Reinikka and Svensson 2011). Previous research focusing on disclosures about firms has focused on how transparency affects market capitalization (Konar and Cohen, 1997; Bui and Mayer, 2003). We provide evidence of a key alternate pathway that allows information disclosure to affect regulatory outcomes: by allowing the public to hold governments accountable for implementing policies effectively.

Chinese government to undertake significant regulatory changes aiming at improving its environmental quality. These measures include, for example, setting specific environmental performance targets (He et al., 2020), launching nationwide pollution monitoring networks and providing real-time pollution information (Greenstone et al., 2022), and introducing pollution levies and taxes on large emitters (Gowrisankaran et al., 2020).

In addition to these "top-down" command-and-control type approaches to environmental protection, the central government also explicitly encouraged "bottom-up" initiatives in the form of citizen participation. In 2006, the Ministry of Environmental Protection (MEP) issued the "Interim Measures for Public Participation in Environmental Impact Assessment," which emphasized the legal rights of citizens to get involved in making and implementing environmental policies. In the same year, the MEP established the "12369 Environmental Appeals Center," which hosted a national hotline (under the phone number 12369) that allowed citizens across the country to file environmental appeals about potential violations of pollution standards. Later, the platform expanded to include an official website. The MEP also instructed each prefectural city's environmental protection agency (EPA) to open an office to address citizen appeals.[6]

When a citizen makes an appeal via the 12369 platform, either by calling the hotline or leaving a message on the website, her appeal will be directed to the corresponding local EPA, which has legal responsibilities to investigate and issue fines to the polluter if a violation is confirmed. Between 2017 and 2019, the 12369 platform received a total of 1,860,149 appeals, 56% of which arrived by the hotline and the rest by the online platform. Appeals through the 12369 platform are visible to all levels of government.

In 2014, as part of China's grand "war on pollution," the central government introduced additional policies that further encouraged citizen participation in environmental protection, including the "Guiding Opinions on Promoting Public Participation in Environmental Protection" and the "Measures for Public Participation in Environmental Protection." In addition to reiterating the importance of the existing official channels for citizen participation in environmental protection, the central government required all the prefectural EPAs to set up official accounts on popular Chinese social media platforms, namely Weibo and WeChat, to make it easier for the public and local EPAs to communicate.

As of December 2017, all local EPAs in China's 338 prefectural cities operated official Weibo and WeChat accounts. In the past few years, an increasing number of citizens and NGOs have used these social media platforms to report environmental violations and express their discontent (Wu et al., 2021). For example, from 2014 to 2016, we identified 5,336 Weibo posts reporting alleged violations by CEMS firms, 1,563 of which

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[6] The online appeal platform can be accessed via: http://1.202.247.200/netreport/netreport/index

were posted by NGOs, and the rest by individual citizens. In 2018, a Jiangsu-based NGO called Public Environmental Concerned Center (PECC) alone filed 1,579 appeals on Weibo on pollution violations identified from the CEMS data.

B. China's Continuous Emissions Monitoring System (CEMS)

China operates the world's largest Continuous Emissions Monitoring System (CEMS).[7] The system was introduced in 2004 by the MEP, which consisted of the installation of automatic monitoring equipment, including apparatuses and flow (current) meters installed at discharge points, CCTVs covering all pollution prevention and control facilities, and data collection and transmission apparatuses. Each local EPA maintains a monitoring center that automatically collects data for each key pollutant from each installed meter in real time. Beginning in 2013, central policies required each provincial and prefectural EPA to publicize in real time the hourly emissions data of every monitored plant to the public. The data also includes standards for different emission concentrations, allowing the public to check whether each plant violates the standards hour by hour.

The CEMS monitors the emission concentrations of both water pollutants (COD and NH_3-N) and air pollutants (SO_2, PM, NO_X) for all the key polluters in China. If a firm's total emissions of any pollutant exceeded some threshold determined by the MEP in the past two years, it will be considered as a key polluter and thus be continuously monitored by the CEMS. As of January 2020, the CEMS program monitored more than 24,620 plants, which accounted for more than 75% of China's industrial air and water pollutant emissions.

Our research hinges critically on the reliability of the CEMS data. The MEP exerts substantial effort to ensure the quality and authenticity of the CEMS data. First, the list of CEMS firms is publicized on the MEP website so that local governments cannot omit any CEMS firm from the publicized emission data. Second, the MEP has strict protocols for the installation and operation of the CEMS equipment: installation must be conducted by a third-party team designated by the MEP and 24-hour CCTVs are installed near the monitoring equipment as a deterrent to the plant from interfering with the equipment. Third, the MEP uses various algorithms and technologies to detect abnormalities in the CEMS data and hosts monthly supervisory sessions to discuss any anomalies. Fourth, the MEP requires on-site inspections at least once a month to ensure the proper functioning of the automatic monitoring equipment. Because of these efforts, the polluting plants and local EPAs have very limited scope to interfere with the CEMS. While these are all reasons to believe the data is highly reliable, we verify that data manipulation cannot account for the results of this study in Section VC.

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[7] Other countries use similar systems for various regulatory purposes. For example, the United States EPA and many state governments require firms to install CEMS equipment to demonstrate compliance with permitted emission levels (United States EPA 2021). In India, specific provinces have started to require firms to install continuous monitoring equipment to support emissions trading (Greenstone et al., 2020). Likewise, the European Union has made continuous emissions monitoring support operation of its Emissions Trading Scheme (EU 2021). Yet, none of these schemes have approached the scale of the CEMS in China, where it is used as a systematic regulatory tool for all key industrial polluters.

Despite the central government's efforts to collect and publicize high-quality data, environmental law delegates the enforcement of pollution standards to local regulators. Specifically, if the CEMS data indicates that a firm violates an emission standard, the local EPA is required to conduct an onsite investigation to verify the violation before punishing the firm. In addition, the issuance of substantial penalties, like large fines or shutdowns, involves two steps. First, the local EPA must issue a warning after detecting a violation through onsite inspection and give the firm one month to improve its practices. Then, another inspection will be conducted in a month, and the penalty can only be applied if the problem has not been addressed by then.[8] It is thus not surprising that disclosing the CEMS data alone does not lead to perfect compliance and large polluters can potentially defy or capture local regulators. Additionally, both the central and local governments have competing goals of maximizing production and controlling pollution. Maintaining some flexibility in applying emissions standards is likely to be desirable, especially when violations do not cause significant public discontent.

Figure 1 plots the percentage of emission violations using daily data from 2018 to 2021.[9] In January 2018, around 1.5% of monitored stacks violated emission standards of air pollutants on any given day, and 0.6% violated emission standards of water pollutants. In the following three years, violation rates for both water and air pollutants declined steadily, which is consistent with national policy priorities. Nevertheless, around 0.9% of the monitored firms violated air pollution standards and 0.3% violated water pollution standards on any given day in the year before our experiment (2019). Bringing all CEMS firms into compliance with standards would significantly improve China's environmental quality: if violating firms reduced pollutant concentrations to just below the standards in 2019 (assuming no change in emission flows), SO_2 emissions would drop by 279,000 tons, a 7% reduction in aggregate industrial SO_2 emissions and COD emission would drop by 31,000 tons, a 4% reduction in aggregate industrial COD effluents.

C. Interactions between Citizens, Regulators, and Polluters

To help design the treatments, we studied how citizens, regulators, and polluters interacted in the context of environmental governance, with special attention to the role of citizen appeals. These efforts include extensive conversations with environmental regulators, NGOs, polluting firms, and other relevant parties. Our visits with the directors and the staff assigned to respond to citizen appeals of two prefectural environmental bureaus in Hebei and Jiangsu provinces proved especially insightful.

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[8] There are occasions where abnormal readings are recorded in the data, but they do not necessarily reflect actual emission violations. For example, this can happen when the monitor is mal-functioning, when the firm is turning on or turning off the equipment, or when the firm is switching between different production procedures. As a result, onsite verification is needed before firms can be punished for violations reflected in the CEMS data.
[9] Some CEMS firms can have multiple stacks being monitored, and they can be monitored for multiple pollutants.

First, we learned that each local environmental bureau have designated staff to handle citizen appeals filed. After an appeal is received, the staff member would route the appeal to the EPA's relevant office. For less severe violations, they might just call the polluting firm and collect relevant information. For more severe violations, a team of inspectors would conduct on-site inspections. The EPA, therefore, has significant discretion in levying penalties against violators.

Second, the regulators reported that they face two objectives that are often difficult to balance. On the one hand, political leaders, who could influence the career advancement of environmental regulators, emphasize the importance of economic growth. Local regulators are often given unofficial orders by local politicians that their regulations should not affect economic production and employment. This typically means lax regulation for economically important local firms. On the other hand, local regulators also need to minimize the risk of social unrest caused by pollution. Both local politicians and regulators understand that social unrest and protests could be career-ending. Therefore, the regulators must weigh the goal of preventing social unrest against growth pressures that lead to lax enforcement. Incentives pushed toward lax regulation when there is no public discontent, and toward stringent regulation when violations cause public discontent.

Third, citizen appeals provide important signals of public discontent and the potential for collective actions. As a result, local regulators have incentives to monitor public sentiments and opinions online (especially on social media) and address individual complaints before they trigger wider public discontent. Responding to citizen appeals could also showcase their accountability, which might benefit their agencies in the long run. According to the internal rules of the two environmental bureaus we visited, all citizen appeals are expected to be addressed, unless the information provided in the appeals is inaccurate or could not be linked to a specific pollution source (e.g., there were frequent complaints of foul smells that were not connected to specific sources).

Finally, the regulators told us that the presumptive response to an appeal about a specific polluter is to send a team to the facility for an inspection. While regulators routinely conduct general-purpose inspections to check the paperwork and abatement facilities of firms as part of their normal operations, appeals often generate more task-specific inspections that involve testing pollution concentrations onsite. If emission violations get verified by onsite testing, the inspection teams could impose penalties and require the firms to make necessary corrections to their operations.

We accompanied the environmental inspection teams on a handful of inspections. An interesting observation from these visits was that almost all large polluting firms already had the capital equipment necessary to comply with the emission standards. However, firms were inclined to turn off certain equipment or skip abatement procedures occasionally due to the high marginal costs associated with the operation of

such energy-intensive equipment. When this happened, the emission concentrations would elevate and eventually exceed the standards.

The fieldwork and qualitative evidence are instrumental in devising our experimental design. First, all forms of citizen appeals reveal information about public discontent, which can lead to more frequent environmental inspections and raise firms' expected costs of violating environmental standards. Second, public appeals can be more effective than private appeals, because the information about both the violation and discontent is observable to the general public, which may lead to collective actions. Third, social media can be an effective and inexpensive way to improve government accountability and reduce emission violations. This is because social media appeals can be observed by the general public and have high potentials to trigger collective actions.

III. Experiment and Data

This section describes the field experiment and data. Section III.A discusses the experimental design, Section III.B provides details on the experiment's implementation, Section III.C discusses ethical consideration, and Section III.D introduces the data and presents balance tests across the experimental arms.

A. Experimental Design

The sample is comprised of the 24,620 polluting firms required to install CEMS by the central government by January 1st, 2020. We randomly assigned these firms to several experimental arms designed to uncover the effects of private and public appeals, to shed light on the mechanisms that explain government and firm responses, and to learn whether there are general equilibrium impacts. The main outcomes of interest include each firm's daily violation status and daily average air and water pollution emissions concentrations, as well as prefecture-level ambient SO_2 concentrations.

Experimental Arms: Public and Private Appeals. Figure 2 graphically depicts how these firms were randomly assigned to three broad groups of experimental arms: the control group (C), the "private appeals" group (T1), and the "public appeals" group (T2). We use these experimental groups to identify the causal effects of different appeals relative to the status quo regulatory response and each other. Importantly, the treatments mirror existing and approved ways that citizens participate in environmental governance. Specifically, the three experimental arms and the proportion of firms assigned to them are:

- **Control Group (C):** When the CEMS data indicated that the firm violated its emission standards, we *did not* intervene in any way. About 1/7 of the CEMS firms were assigned to this group.

- **Private Appeals Group (T1):** When the CEMS data indicated that the firm violated its emission standards, a volunteer filed a private appeal against that violation that was not observable to the public. About 5/7 of the CEMS firms were assigned to this group.

- **Public Appeals Group (T2):** When the CEMS data indicated that the firm violated its emission standards, a volunteer wrote a post on Weibo (a popular Chinese social media platform comparable to Twitter), and tagged ("@") the official Weibo account of the corresponding local EPA. The appeal and any response were observable to the public. We assigned 1/7 of the CEMS firms to this group.

For each appeal arm, we prepared a detailed script for the citizen volunteers to follow. The core content of these scripts remained consistent across T1 and T2, while we randomly varied the exact wording in each appeal to avoid appearing repetitive to the regulator or firm. The volunteers who made appeals did not reveal information about their background or organizational affiliation, such that the appeals mirrored how the public normally participates. Samples of the appeal scripts are translated and listed in Appendix B.

Mechanisms. Within the groups of experimental treatments, we further randomized firms into specific treatment arms to investigate several potential mechanisms for the overall private (T1) and public (T2) treatment effects. Specifically, the T1 private appeals were delivered in several different ways following the MEE's recommended channels for appeals about environmental issues:[10]

- **Private Appeals to Regulator via Direct Message on Social Media Group (T1A):** A citizen volunteer sent a *private* message to the corresponding local EPA's official Weibo account, notifying them about the pollution violation and requesting that they investigate the issue.

- **Private Appeals to Regulator on Government Website Group (T1B):** A citizen volunteer filed a *private* appeal via the 12369 website to the corresponding local EPA, notifying the local EPA about the violation and requesting that they investigate the issue.

- **Private Appeals to Regulator through Government Hotline Group (T1C):** A citizen volunteer called the 12369 hotline to *privately* appeal to the corresponding local EPA. In the phone call, she notified the local EPA about the violation and requested that they investigate the issue.

- **Private Appeals to Firm through Phone Call (T1D):** A citizen volunteer called the violating firm to *privately* appeal the violation. In the phone call, she notified the firm about its violation and requested that they check the issue.[11]

Furthermore, we cross-randomized T1C and T1D, such that half of the firms receiving T1C also simultaneously received T1D, and vice versa. The sub-arms in T1 are randomized at the firm level. T1A and

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[10] Naturally, our interventions cannot exhaust all the possible channels through which private pollution appeals can be filed. Nevertheless, we believe that the subset of appeal channels that we choose are the most common types of private pollution appeals in China. They were also explicitly endorsed by the MEE itself in its guidelines for citizen participation in environmental governance.

[11] The phone number we used to contact the firms were the official numbers listed on the updated business registration records, which are the same numbers that governments and other businesses would use to contact these firms.

T1B each account for 1/7 of the CEMS firms, while T1C and T1D jointly account for 3/7 of the CEMS firms.

A comparison of the T1A – T1C treatment effects with the T1D treatment effect reveals whether private appeals to local governments are more effective than private appeals to firms. Further, a comparison of the T1A – T1C treatment effects provides an opportunity to assess whether the government is more responsive to appeals through a particular channel. For example, an appeal delivered through a social media direct message (T1A) might indicate a greater risk that the violation will draw widespread attention than an appeal delivered via an older technology like a phone call (T1C). The interaction term between T1C and T1D created by our cross-randomization provides a test of whether there are complementarities in privately appealing to regulators and to firms. Additionally, this interaction is indirectly informative about the nature of the T2 treatment effect since public appeals by their very nature involve informing both the regulator and the firm of the violation.

Within the T2 treatment group, we randomly assigned half of the violations to receive additional public attention by hiring a social media firm to increase the number of "likes" and "shares" for these Weibo posts at the *appeal* level (T2B). Ultimately, the average number of "likes" and "shares" were 10.56 for T2B, compared to 0.66 for T2A that did not include any promotion. Figure A1 shows that the "likes" and "shares" in T2B amounted to 3–4 times the average level of engagement with pollution appeals posted by other NGOs or posts made by the local regulators. The comparison between these two arms reveals the causal effect of publicity, which might be important given the regulator's objective to avoid collective actions and civil unrest. Among the T2B and T2A violations, we compare whether the Weibo appeal receives a response from the regulator, the response's length measured in words, and whether the response includes proof of an onsite inspection or audit of the violator. These outcome variables are collected through our Weibo exchanges with the local regulators and are not available for the other arms.[12]

General Equilibrium. We also experimentally investigate the indirect or general equilibrium consequences of the pollution appeals. Although the public and private appeals are applied at the firm level, they could also affect the ways that regulators enforced standards generally within their jurisdiction. On the one hand, regulatory resources may be fixed and their application to violators in the treatment group may crowd out the regulatory resources that would otherwise have been allocated to control firms or treatment firms that

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[12] It is worth noting that, even if we can collect these violation-level outcome variables for all violations, we still will not be able to draw any causal conclusion from a comparison across C, T1, and T2. This is because the three main arms were randomized at the *firm* level, and during the experiment, any violation after the first one will be endogenous to the treatment received by that firm. As a result, the types of firms that keep committing violations, as well as the frequencies and severities of these subsequent violations could differ across C, T1, and T2, and thereby mechanically lead to differences in regulatory responses and efforts. We can compare these outcome variables across T2A and T2B, because these two sub-treatments were randomized at the *appeal* level.

previously did not violate standards, potentially leading to an increase in emissions and violations by them. On the other hand, the treatment might cause a positive spillover effect by: (1) leading local governments to generically enforce environmental regulations because they interpret the increased appeals as an indication of the public's broad dissatisfaction with environmental quality; and (2) causing these firms to proactively reduce their emissions because they interpret the increased appeals and enforcement against violators as evidence of an increase in regulatory stringency. These broad forces work in opposite directions and whether one dominates is ultimately an empirical question.

To explore these possibilities, we cross-randomize treatment intensity across different regions. Specifically, 95% of the CEMS firms were assigned to the treatment groups in 60% of the prefectural cities ("95% prefectures") and 70% of the CEMS firms were assigned to the treatment groups in the other 40% of the prefectural cities ("70%" prefectures").[13] This "double randomization" design allows us to causally identify the general equilibrium effects of pollution appeals by comparing the violation rates and emissions of firms across the 95% and 70% prefectures, conditional on their treatment status. We also test whether ambient air pollution concentrations were equal in the 95% and 70% prefectures, which provides information on whether there are decreasing returns to scaling up participation.

B. Experimental Implementation

The experimental period started on May 6, 2020 and ended on December 31, 2020. There are three key steps: (1) identify and verified CEMS firms that violated the emission standards based on the data in the previous 24 hours; (2) file appeals through different channels according to the experimental design; and (3) document government responses. Here we provide some more detail on each of these steps.[14]

For the first step, we combined an algorithm that we developed with human judgment to mimic the definition of pollution violations set by the Ministry of Ecology and Environment (MEE).[15] In real-time, the algorithm identified all firms violating the national emission standards, based on their average pollution emission concentrations in the previous 24 hours.[16] If a CEMS plant is being monitored for multiple pollutants, we identify it as a violator if any of the pollutants violate the emission standard. We then employed 12 environmental science graduate students to manually double-check the violations identified by the algorithm.

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[13] These numbers are chosen so that each of the six treatment arms and the control arm will be similar in terms of sample size, which maximizes the statistical power in pairwise tests across different arms.

[14] A few implementation details prior to the study period are worth mentioning. In January 2020, we collected the phone number of all the firms in the sample and the official Weibo account of every local EPA. Between January and March 2020, we trained research assistants to identify and verify violations of emissions standards, and trained citizen volunteers to file appeals via different channels following the experimental assignments. Additionally, we conducted a small-scale pilot in April to ensure all the research assistants could complete the daily tasks on time.

[15] The MEE was established in 2018 in replacement of the MEP.

[16] For each pollutant being monitored, the MEE determines a specific emission standard for each CEMS plant. Appendix Figure A2 plots the distribution of SO2 and COD emissions standards across all CEMS plants in our sample.

Because the CEMS equipment often continues to run after production is suspended and there is little air/water flow, emissions concentrations can be abnormally high for a short period of time. To remove these false positives, the graduate students identified the violations that occurred after production had stopped or that were due to mechanical spikes by examining complementary indicators such as other pollutants concentrations and water/gas flows. Once the students, who were blinded to treatment assignment, identified true violations of the emissions standards, they took screenshots from the CEMS webpages (see Appendix B for details).

The second step was to use the verified pollution violations and file appeals. Every day, we generated the list of verified violations, produced the script used for each appeal, and determined the delivery method based on the firms' treatment assignments. These appeals were eventually filed by group of citizen volunteers, whom we recruited through three environmental NGOs. The volunteers who made appeals did not reveal information about their background or organizational affiliation, such that the appeals mirrored how the public normally participates.

We gave each volunteer no more than 15 appeals per day to provide enough time to follow the protocols (e.g., reporting by phone is limited to the working hours of the recipient). To avoid repetitive appeals, if a CEMS firm committed consecutive violations spanning multiple days, we waited until the next week before filing a second appeal. Appendix B describes the implementation protocol in greater detail and provides sample screenshots of appeals in each arm.

An additional part of the second step was the amplification of half of the public Weibo appeals. For the T2B arm, we hired a social media promotion company to boost the publicity of the appeals. Specifically, the company added roughly 10 "likes" and "shares" to the Weibo appeals (T2B), using a variety of existing and active company-operated Weibo accounts.

In the third step, the volunteers tracked the responses to the appeals from the local governments, which primarily came back as Weibo direct messages, Weibo public replies, 12369 phone calls, and 12369 website replies. We recorded the timing and content of each government response and matched them to the corresponding appeal.

Table 1 summarizes some basic facts about the experiment. First, as mentioned, we include the universe of 24,620 CEMS firms in China's 333 prefectures. During the 8-month treatment period, other citizens, who were not part of the research team, filed a total of 271,859 pollution appeals to the government; 5,478 were explicitly about pollution violations committed by the CEMS firms.[17] Most appeals did not target specific

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[17] These numbers are based on the administrative data covering the universe of citizen appeals filed in 2020, which we obtained from the MEE. We excluded pollution appeals filed by the research team when calculating these numbers.

CEMS firms and were instead about some unpleasant odors or dirty waters that people encountered near their communities. Second, the experiment was conducted in collaboration with the three NGOs, which collectively organized 15 volunteers on an average day to file pollution appeals. During the study period, a total of 120 Weibo accounts were used by the volunteers to file pollution appeals. We also hired 12 Environmental Science graduate students to verify pollution violations. Third, the CEMS raw data showed 12,596 pollution violations during the study period, but our verification process revealed that only 5,366 violations were real, which were committed by 2,363 different CEMS firms.[18] We filed 2,941 appeals according to the treatment assignments; for the rest 2,425 violations, appeals were not filed either because these violations were committed by the control firms, or because we did not file multiple appeals within a week about the same firm's violations. Our appeals generated 1,161 formal responses from the regulators.

C. Ethical Considerations

Prior to implementing this experiment, we carefully considered the ethical implications of working with a partner non-governmental organization to file appeals. While Appendix C discusses the ethical considerations in more detail, several points are worth highlighting. First, the rights of citizens to make appeals against violations is legally protected and explicitly encouraged by national policies. All local governments are mandated to operate the multiple channels of making appeals that we study in this experiment. Second, we consulted with several non-governmental organizations that already had multiple years of experience filing appeals and were not advised of any repercussions to their staff or organizations. Third, we worked with a non-governmental partner that was already active in environmental monitoring, so the treatments were not outside of the existing scope of their work. Fourth, we were in daily contact with our partner organization and never learned of any adverse events or pressures in response to the appeals. Finally, because we did not collect data from or about any individual people, two separate IRBs in the United States determined that this study was not considered research with human subjects.

D. Data, Balance Tests, and Empirical Description of the Treatments

The analysis is conducted on a data set that results from combining several sources of information. These MEE data cover all the CEMS firms and include information on firm name, social credit code, industry, main pollutant type, hourly emission concentrations of various pollutants, hourly gas and water flows, pollution violation status, among other measures. In 2020, due to the COVID-19 lockdown, most CEMS firms suspended production until the economy reopened in mid-March, so we drop the first 10 weeks of the 2020

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[18] Here if a firm simultaneously violated the emissions standards in multiple stacks or for multiple pollutants, we count that as one daily violation. Consecutive violations spanning multiple days are also combined as one violation. As a result, the violation rate in Table 1 is substantially smaller than the violation rate indicated by Figure 1.

CEMS data from the sample.[19] The official database of CEMS firms was matched to our experimental data, including treatment arm, specific appeals, and government responses.[20]

Two other government data sets were critical ingredients. First, we merged in MEE data on all citizen appeals against the CEMS firms in 2020 made either through the 12369 website or by phone. This database of appeals includes appeals filed by other citizens on their own volition and the ones generated by the experiment. Second, we also merged in the Ministry of Commerce's administrative data on firm registrations. This data contains information on the date of establishment, industry, business address, business type, registration status, and other measures. The firm registration data are merged with the CEMS data using the social credit code.

Table 2 reports on balance tests across the experimental arms. In Column (1), we present the mean and standard deviation of the control group, for variables such as the share of firms in different industries, the total amount of pollution penalties paid in the previous year, frequent violators in the previous year, and various measures of pre-treatment environmental performance (in the eight weeks before the treatment began), including the severity of violations and emission concentrations. We then compare each treatment arm to the control arm, implemented by running a regression of each outcome variable on a set of treatment dummies. In Columns (2) to (6), we present the regression coefficients and standard errors for each variable-arm combination. As we can see, the treatment arms are well balanced with the control arm along almost all dimensions, confirming that our randomization was well executed. Appendix Table A1 also reports the detailed breakdown of industries by experimental arm.

IV. Empirical Results

This section presents the baseline results from estimating the following econometric model:

$$Y_{ijt} = \sum_j \alpha_j T_{ij} \cdot Post_t + \gamma_i + \eta_t + \epsilon_{ijt} \qquad (1)$$

where Y_{ijt} is the outcome of interest for firm i, assigned to arm j, on day t. T_{ij} represents the randomly assigned arm of firm i and it is interacted with $Post_t$, which is a dummy variable that equals one after the experiment commenced.[21] We control for firm fixed effects, γ_i, and day fixed effects, η_t. Since we cross-randomize treatment intensity at the prefecture level, we also estimate more saturated specifications that include province-by-day fixed effects to control for time-varying differences in regional enforcement. Standard errors are clustered two-way by prefecture and week.

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[19] From mid-January to mid-March 2020, China was struck by COVID-19 and many CEMS firms suspended their production due to compulsory lockdowns. By late March 2020, almost all Chinese cities re-opened because COVID-19 was already deemed under control. During the experimental period, production resumed and firms operated as usual.

[20] For analysis, we use the official CEMS data provided annually by MEE, rather than the data published daily on the provincial government websites used to identify violations. The official data from MEE are more complete and have been cleaned of basic errors that occasionally appear in the real-time data.

[21] The week of May 7th is the 18th week of the year. Since the first 10 weeks are excluded from our sample due to COVID lockdown, the pre-treatment period corresponds to the first 7 weeks in our sample.

The α_j's are the parameters of interest. They measure the causal effects of different types of appeals relative to the controls during the experiment. In this section, we report on specifications that estimate the average effects of private appeals (T1A – T1D combined) and public appeals (T2A and T2B combined). We examine specific treatment arms when analyzing the underlying mechanisms. These estimates represent the "intention to treat (ITT)" effects, since the assigned appeal type is only triggered after the firm commits a violation during the experimental period.[22]

A. Pollution Appeals and Environmental Performance

Table 3 summarizes the results from the estimation of two versions of equation (1) for three measures of environmental performance. For each outcome, the column "a" specification includes firm and day fixed effects and the column "b" specification replaces the day fixed effects with province by day fixed effects. In columns (1a) and (1b), the outcome is $violation_{ijt}$, which is a dummy variable indicating whether firm i committed any pollution violation on day t. In the next two pairs of columns, the outcome variables are the firms' daily emission concentrations of SO_2 for air pollution, and COD for water pollution. These two pollutants have the highest coverage for CEMS firms and are the most high-stakes "criterion pollutants" for evaluating the environmental performance of local government officials (He et al., 2020).

Violations. The results indicate that appeals greatly reduce violations, especially public appeals. In the preferred column (1b) specification, the private appeals treatment (T1) reduces the probability of a daily violation by 0.227%, relative to the control group; this is about 24% of the control group's mean of 0.936%. The public appeals treatment (T2) decreases the probability of a violation by a larger margin: roughly 62% relative to the control group's mean. The table also documents that the difference in magnitudes is statistically significant as the null hypothesis that the public appeals treatment is smaller in magnitude than the private one is rejected at conventional significance levels. Finally, we note that the results are quantitatively similar across the two specifications.[23]

Figure 3 provides an opportunity to understand how the treatment effects evolve over time. Specifically, it reports the results from fitting a version of the column (1a) specification where the treatment and $Post_t$ interactions are replaced with treatment and week interactions. For both the private and public treatments,

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[22] To quantify the treatment effect of appeals, we also adjust the baseline specification with an instrumental variable (IV) approach, where whether a firm has received an appeal of a certain type is instrumented by "treatment assignment * whether experiment has started." The procedure and results are displayed in Appendix D.

[23] We conduct additional checks in the appendix. First, in Table 3, we defined pollution violations based on whether the monitored emission concentration exceeded the standard value set by the MEE. However, it is possible that some of these monitored values are driven by mechanical errors or production suspensions, instead of actual pollution violations. In Appendix Table A2, we refine the definition of pollution violations to exclude cases with minimal levels of measured air flows as these may be instances when the plant is not operating. The results with this alternative definition of a violation are qualitatively the same as those in Table 3, confirming the powerful effect of the public appeal treatment at reducing violations and the more modest effect of private appeals. Second, in Appendix Table A3, we report alternative standard errors by clustering at either the prefecture level, or at the prefecture-by-arm level, and if anything, the statistical significance increases under these specifications. Third, in Appendix Table A4, we aggregate the data to either the firm-month level or the firm-week level and run Poisson regressions with the same set of baseline fixed effects, and the main results still hold.

nonzero treatment effects emerge within a couple of weeks of the experiment's initiation. It is sensible that they do not appear immediately, because the appeals are not initiated until a firm commits a violation. By week 20, it appears that both the T1 and T2 treatment effects have stopped increasing and stabilized at a level larger in magnitude than reported in Table 3 (which is an average over the entire experiment); this pattern suggests that the Table's estimates understate appeals' long-run potential to reduce the incidence of violations. In Appendix Figure A3, we also disaggregate T1 and present the trends of each sub-treatment arm, which show similar patterns. Moreover, it is reassuring that there is no evidence of a treatment effect in the period before the RCT began for either the private or public appeal arms, confirming that the randomization was well executed. Overall, these figures suggest that the long-run partial equilibrium effect of the treatments are larger than is reported in Table 3.

Emissions. Columns (2) and (3) of Table 3 show that the public appeals treatment (T2) caused substantial reductions in air and water pollution emissions concentrations, while the private treatments (T1) led to much more modest reductions. The results from the preferred (2b) and (3b) specifications reveal that public appeals reduced the average firm's daily average SO₂ emission concentration by 16.2 ug/m^3 and its daily average COD emission concentration decreased by 2.2 ug/L; these are 12.2% and 3.7% declines from the control group's SO₂ and COD emission concentration levels, respectively. Further, the estimates also indicate that the private appeals treatments slightly reduced these two measures of air and water emissions concentrations, but the effects are much smaller and would not be judged to be statistically significant by conventional criteria.[24]

Figure 4 further examines where in the distribution of emissions concentrations the Table 3 columns (2) and (3) estimates of public appeals come from. We report the results from fitting the preferred version of equation (1) with province-by-day fixed effects separately on indicators for whether a firm's daily SO2/COD emissions concentration was 0–40% of the corresponding emissions standard, 40–80%, 80–100%, 100–200%, and >200%. As we can see, under public appeals, firms' emissions concentrations became less likely to exceed the emission standards, which is especially true for extreme violations where their emissions concentrations more than doubled the national standards. Interestingly, these shifts in emissions concentrations appear to be infra-marginal: the T2 firms did not fall in the "barely compliant" bin (80–100%) more frequently. Instead, they became much more likely to fall in the "highly compliant" bin, where their emissions concentrations were below 40% of the national standards. This is consistent with the qualitative observation that most

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[24] Appendix Figure A4 is constructed identically to Figure 3 together with Figure A3 and reports on how the emissions concentration treatment effects evolve over time. Here too, the treatment effect grows over time, especially for the private appeals, presumably as violations cause the polluters to learn about the experimentally induced increase in scrutiny. The takeaway is that, holding constant the regulatory environment, the treatments' long-run equilibrium effects on emissions are larger than those reported in Table 3.

pollution violations committed by CEMS firms were driven by discontinued operations of their abatement facilities (to reduce energy use), and once these facilities become properly functioning, most CEMS firms' emissions concentrations can fall well below the emissions standards.

Additionally, we investigated whether the reduction in average emissions concentrations translated into reductions in total emissions, which are the relevant metrics for determining the impact on individuals' exposure to ambient pollution.[25] The impacts on emission concentrations and total emissions could differ if the treatments cause the plants to change their intensity of operations (e.g., a decline in concentration could be offset by an increase in the number of hours of operation or running the plant at full capacity more frequently during operating hours). While the CEMS data does not directly report hourly data on total emissions, we infer the changes in total emissions by investigating the average hourly gas/water flows recorded by the CEMS; the product of these variables and the emission concentrations equal average total emissions per hour for each pollutant. Appendix Table A5 reveals that none of the treatments had a meaningful impact, either economically or statistically, on the flow of pollutants. We thus conclude that there were reductions both in emissions concentrations and total emissions.

B. Heterogeneity in Treatment Effects, Based on Firm Characteristics

Table 4 tests for heterogeneity in the treatment effects based on whether firms are state-owned enterprises (SOEs), produce "final" (rather than "intermediate") goods that might make them more concerned about their public image,[26] and whether they committed violations during the seven-week pre-treatment period. The state-owned enterprise tests are reported in columns (1a), (1b), and (1c) for the violation rate, SO2 emissions concentration, and COD emissions concentration, respectively. The analogous regression results for the other two categories are reported in (2a)–(2c) and (3a)–(3c). Throughout the table, we report the results from the more demanding specification that includes firm and province-by-day fixed effects.

A salient finding in Table 4 is that the overall treatment effects are driven by the firms that committed violations during the seven weeks prior to the experiment (columns 3a–c). Indeed, there is little evidence of any treatment effect among the subsample of firms that did not violate in the pre-treatment period.[27] Additionally, the treatment effects for the SOEs are larger in magnitude than for private firms, although the difference is not statistically significant by conventional criteria. Finally, there is little consistent evidence that

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[25] It is common for regulators around the world to focus on emissions concentrations, even though it is total emissions that matter for human health. For example, regulators in India and the United States focus on enforcing emissions concentration standards.

[26] This variable is defined based on industry code in the business registration data.

[27] The amount of frequent and non-frequent violators is balanced across different experimental arms. This heterogeneity could be mechanical, given that the frequent violators have a larger room for improvement. Another possibility is that, frequent violators might be more responsive to citizen appeals. Comparing how frequent and non-frequent violators' emissions changed after they received their first appeals in our experiment, we find no evidence of differential responsiveness, thus suggesting that the heterogeneity might be mechanically driven by the difference in baseline violation rates.

the treatment effect is different for final goods firms, compared to the rest of the sample, indicating that "corporate social responsibility" concerns are unlikely to be the driving force behind the main results.

V. Mechanisms

This section explores several channels that might explain the specific estimated treatment effects observed in the previous section.

A. Private Appeals

Appendix Table A6 explores the mechanisms that underlie the overall T1 treatment effect by disaggregating the effects into several treatment arms. There are three observations. First, there is little evidence that private appeals to the government are more effective than private appeals to firms, as we cannot reject the null that the effects of T1A, T1B, T1C, and T1D are equal. Second, there is also no evidence that the channel through which the private appeals are delivered matters, as we cannot reject the null that T1A, T1B, and T1C are equal. Finally, privately informing both the government and the firm has little additional effect on the outcomes, because the coefficient associated with the interaction of the T1C and T1D treatments is statistically insignificant. In other words, there are no complementarities in privately appealing to the regulator and to firms, implying that publicity is not operating exclusively through firm-side changes.

B. Public Appeals

Regulators' Political Incentives

As outlined in Section II.C, regulators face competing incentives of maintaining economic growth and reducing environment-related social unrest. In the absence of credible threats for collective actions, regulators might choose to turn a blind eye to pollution violations committed by important local firms, in exchange for higher economic growth. However, in the presence of public discontent, failures to prevent collective actions could veto the regulators' promotion cases, which gives them overarching incentives to respond to public attention about a violation with greater regulatory effort.

In Appendix Tables A7 and A8, we provide suggestive evidence that political incentives play an important role in our experiment. Specifically, following Guo (2009), we collected the resumes of all the prefectural environmental bureau chiefs in 2020 and calculated how many years they have left in their current five-year terms. Since most bureaucratic promotions happen after the completion of one's term, regulators at the beginning of their terms (i.e., first two years) tend to have weaker political incentives, compared to their peers who are more advanced in their current terms (i.e., last three years). We find that prior to our experiment, the local regulators' promotion incentives are positively correlated with local firms' violation rates (Appendix Table A7), but after the start of our experiment, those with stronger political incentives are significantly more responsive to the public appeals. These empirical patterns are consistent with our interpretation that local

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regulators, in order to advance their careers, tend to tolerate violations for higher economic growth when there is no public discontent, while being significantly more responsive to potentially explosive citizen sentiments against specific violations.

To further highlight the importance of "potentially explosive public sentiment" in driving regulatory actions, we created additional experimental variation in the visibility of public pollution appeals, by experimentally increasing the number of "likes" and "shares" for half of the T2 public appeals posted on Weibo. We then compare regulatory efforts across three outcomes between these promoted appeals (T2B) and appeals that were not promoted (T2A).

Table 5 reports the results of fitting an equation where the outcome is a measure of regulator response to a public appeal on Weibo. The explanatory variable of interest is an indicator for the T2B treatment that involves the appeal's promotion. This is a cross-sectional regression, and the unit of observation is a pollution appeal that we posted on Weibo regarding a T2 firm's violation. The dependent variables include an indicator for whether the Weibo appeal receives a response from the regulator (columns 1a and 1b), the response's length measured in words (non-response is coded as zero) (columns 2a and 2b), and an indicator for whether the response includes proof of an onsite inspection or audit of the violator (columns 3a and 3b). The "a" specifications include fixed effects for the day of the violation and the "b" specifications add province fixed effects.

The Table 5 results make it clear that environmental regulators are responsive to the publicity of appeals. The columns 1a–b results indicate that adding Weibo likes/shares significantly increases the probability that the regulator replies to the Weibo appeal by approximately 6 percentage points, which is a roughly 40% increase in the baseline response rate of 15.5 percentage points. The treatment also doubles the average length of the response (columns 2a– b) and increases the probability of a documented onsite inspection or audit by more than 60%. These results suggest that more publicity significantly increases local governments' responsiveness and effort to regulate pollution, as well as indicating the power of social media networks to provide a forum to launch the publicity.[28]

C. Other Potential Mechanisms

We investigated several other potential mechanisms for the public appeal results but failed to find evidence for them. For example, it is possible that the public appeals posted on social media as part of the experiment

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[28] Since T2B was randomized at the *appeal* (instead of *firm*) level, we cannot use the baseline econometric specification to quantify the impacts of such social media promotion on firm-level emissions outcomes. That said, we still try to shed light on this using the appeal-level analysis, comparing the subsequent violation patterns for firms that randomly received promoted vs. non-promoted Weibo appeals for their first violations during our sample period. As shown in Appendix Table A9, firms whose first violation received a Weibo appeal with promotion, compared to firms whose first violation received a generic Weibo appeal, have a lower probability of committing repeat violations during the experiment.

inspired other citizens to file their own appeals against the pollution violations, thereby increasing the overall impact of public appeals. To investigate this possibility, we obtained the universe of citizen appeals data from the 12369 website and the MEE and matched this information to each CEMS firm in our sample. We found that our interventions did not significantly change the number of appeals filed by other citizens (Appendix Table A10). These null results are precisely estimated, suggesting that the effect of publicly appealing violations is not driven by crowding in other appeals. We also investigate whether the promoted appeals led to more appeals from other citizens and find no such evidence (Appendix Table A11). Relatedly, we also test whether having more citizens appealing about the same pollution violation would cause the violator to reduce emissions more and become more compliant in the following months. We find that conditional on the severity of a violation, receiving multiple private appeals has no extra impact compared to receiving just one private appeal (Appendix Table A12). These observational findings indicate that the role of "publicity" is not limited to just involving more citizens in voicing their appeals.

Another possibility is that polluting firms might respond to public appeals by manipulating the CEMS monitoring data, rather than abating pollution. As explained in Section IIB, the CEMS utilizes a series of technologies and follows strict protocols to ensure the accuracy of the data, which, in principle, leaves little room for firms to influence the automatic emission readings. Nevertheless, we investigated this possibility by comparing the frequency of suspicious readings across the experimental arms. The results in Appendix Table A13 indicate that the experimental interventions had no impact on the probability that the CEMS were operated for fewer than 20 hours in a day or the probability that the CEMS recorded unusually low emission concentrations, suggesting that data manipulation is unlikely to drive our main findings.[29] Moreover, as we will discuss in detail in Section VI, the findings on firm-level emission reductions are corroborated by changes in ambient pollution levels at the prefectural city level, further supporting that the baseline findings that appeals, especially public ones, cause improvements in firms' environmental performance.

Finally, it is possible that public appeals on social media reduce the information asymmetry between local regulators and upper-level government officials, and thereby incentivizing local regulators to vigorously regulate violations to avoid sanctions or oversight from above (Anderson et al., 2019; Buntaine et al., 2021b). This mechanism is unlikely in our context, since any citizen appeal in the 12369 platform is automatically documented in the MEE's central system, meaning there is little scope for information asymmetries across different levels of government. Nevertheless, we tested this possibility directly. In half of the private Weibo appeals (T1a), we randomly threatened the local regulator that "if the issue does not get resolved, we will bring

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[29] We define a firm's missing hours as "unusually high" if it records more than 4 hours of missing CEMS data in a given day; we define a firm's emission concentration reading as "unusually low" if it records an average emission concentration level below 10% of its yearly average on a fully operating day.

it to the upper-level government." The results in Appendix Table A14 indicate that this threat did not have a statistically meaningful effect on any of the three measures of regulatory effort, suggesting that concerns about central government oversight do not drive the baseline findings.[30]

D. Key Takeaways

The mechanism analyses help us better understand why public appeals can be more effective than private appeals. We draw several conclusions. First, private appeals, regardless through which channels the appeals are filed, have limited impacts on firms' behaviors; and firms also do not seem to have a generic interest in responding to citizen appeals, unless they face regulatory burden. Second, public appeals are a powerful tool to make the regulators more accountable, because they can create public pressure. Career-driven regulators take public appeals as signals of collective actions. When the risk of collective actions increases, local regulators take public appeals more seriously and respond more positively to the appeals. Finally, the effects of public appeals do not appear to operate through the following channels: (1) encouraging more citizens to appeal or complain, (2) making CEMS firms more likely to manipulate data, and (3) reducing the information asymmetry between lower- and upper-level governments.

VI. General Equilibrium Impacts of Pollution Appeals

Except for rare exceptions (e.g., Crépon et al., 2013; Egger et al., 2019), randomized control trials produce causal *partial equilibrium* estimates of an intervention but cannot provide evidence on the intervention's *general equilibrium* or indirect consequences. This setting is one where knowledge of the general equilibrium consequences may be especially important. This is because it is at least plausible that regulators responded to public and private appeals by shifting inspections and other regulatory effort between firms, allowing untreated firms to increase their emissions due to the reduced regulatory scrutiny.[31] If appeals only shift enforcement, it is possible that they had little or even zero impact on total emissions and ambient pollution concentrations.

We designed the experiment to learn about the general equilibrium consequences of appeals by cross-randomizing treatment intensity across regions. Specifically, in 60% of the prefectural cities, 95% of the CEMS firms were assigned to the treatment groups, while in the other 40% of the prefectural cities, 70% of the

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[30] Relatedly, we tested the possibility that the local officials were afraid of potential mainstream media exposure, by randomly threatening to contact local newspapers about the violation. As shown in Appendix Table A13, this also had no significant impact on regulatory effort, although some of these coefficients are imprecisely estimated. Since these randomly added threats of escalations (to media or to upper-level governments) signal higher levels of civil unrest, the lack of heterogeneity in Table A13 also indicates that "governments interpreting social media appeals as reflecting more anger" is unlikely to be the main reason that public appeals are significantly more effective than private appeals.

[31] This could be driven by either the regulators' competing goals of balancing economic growth and social stability (so that they allow more pollution for firms not at the center of public attention), or regulatory capacity constraints that limits the scope of regulation (so that regulating treated firms will reduce regulatory resources spent on control firms).

CEMS firms were assigned to the treatment groups. We implement several tests, based on this cross-randomization, to assess the general equilibrium impacts.

Appendix Table A15 examines the impacts on ambient SO_2 pollution concentrations using data from national air quality monitoring stations in China, which are independent from the CEMS network and cannot be influenced by the CEMS firms. The entries come from the estimation of:

$$SO2_{st} = \alpha \cdot High_s \cdot Post_t + \gamma_s + \eta_t + \epsilon_{st} \qquad (2)$$

where $SO2_{st}$ is the average ambient SO_2 concentration recorded in prefecture s on day t; $High_s$ is a dummy variable indicating whether prefecture s was experimentally assigned to the high-treatment-intensity group where 95% of the CEMS firms are assigned to one of the treatment arms; $Post_t$ is a dummy variable indicating whether day t is after the treatments were initiated; and γ_s and η_t are prefecture and day FEs, respectively. The coefficient of interest is α, which measures the effect of the 95% treatment, relative to the 70% treatment (there is not a control group of cities with zero appeals). Standard errors are clustered two-way by prefecture and week. Finally, we note that we only examined SO_2 concentrations, because industrial production is responsible for more than 80% of China's total SO_2 emissions, while less than 50% of China's total COD emissions are from industrial sources, meaning that this outcome is unlikely to have sufficient statistical power.

The estimates displayed in Appendix Table A15 reveal relative reductions in ambient SO_2 concentrations in the high-intensity prefectures. Specifically, SO_2 concentrations decreased by more than 3.5% in the 95% prefectures, relative to the 70% ones. This finding, despite being noisy, is quite striking, because ambient air quality measures have limitations for detecting the effect of the public and private appeal interventions.[32] In addition to testifying to the far-reaching impacts of the nationwide interventions, the findings on ambient pollution also confirm that at a minimum the baseline improvements in firms' environmental performance cannot be entirely explained by their manipulation of the CEMS data. Finally, we note that these results would lead to the rejection of the null hypothesis that there are countervailing general equilibrium forces that perfectly undo the treatment's partial equilibrium improvements in environmental performance documented in the preceding tables. In Figure 5, we see that the treatment effect on ambient SO_2 concentrations appears to increase over time, which is consistent with a positive general equilibrium effect as firms learn about increased public scrutiny, although statistical imprecision prevents definitive conclusions.

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[32] For instance, ambient air quality measures are affected by other local emission sources (e.g., household coal consumption and non-CEMS polluting firms) and emissions from other jurisdictions because SO2 can travel hundreds of miles. Additionally, changes in meteorological conditions can significantly influence ambient air quality.

Table 6 returns to the firm data and estimates the impact of assignment to the 95% prefecture group, relative to the 70% group, on violations, SO_2 emissions, and COD emissions, with the aim of better understanding the treatments' general equilibrium impacts. Specifically, we estimate a version of equation (2) with daily observations on these firm-level outcomes. These regressions provide an opportunity to separately test for a general equilibrium response among control and treatment firms by assessing whether either of the following two opposing forces dominates: 1) limited regulatory resources or muti-tasking incentives that cause regulators to shift enforcement to the CEMS firms subject to appeals, and the firms not subject to the experimentally-induced appeals responding by *increasing* their emissions; and 2) some combination of a secular increase in enforcement or firms' response to a perceived increase in regulatory intensity causing these firms to *reduce* emissions.

In Panel A, the sample is limited to control firms that were not directly affected by the treatment, meaning that this group provides a straightforward test of the net effect of these potential general equilibrium forces. The point estimates in column (1) suggest that control group violations did not vary significantly across the 95% prefectures and the 70% ones. The SO_2 and COD emissions estimates point in opposite directions with neither being near statistical significance.

Panel B conducts the same exercise for the treatment firms. Here too, in the absence of general equilibrium impacts, there is no reason for these measures of environmental performance to vary between the two groups of prefectures. The probability of a violation is 20% lower in 95% prefectures, relative to the 70% prefectures for the treatment firms, in the more robust column (1b) specification; these estimates are statistically significant at the 10% level. There is also no evidence of a difference in average SO_2 or COD concentrations.

Overall, we conclude that the partial equilibrium treatment effects were not zero sum, because the reductions among targeted plants were not undone by increased emissions from other plants. Indeed, it appears that the general equilibrium impacts might even be positive for the treatment firms, perhaps indicating that direct regulation and general deterrence are complementary.

VII. Conclusion

There are three main findings from this paper's nationwide field experiment in China that randomly appealed privately and publicly against pollution violations through officially sanctioned channels. First, public appeals to the regulator through social media reduced violations by more than 60%, and decreased air and water pollution (SO_2 and COD) concentrations by 12.2% and 3.7%, respectively. In contrast, private appeals caused more modest environmental improvements. Interestingly, the emissions reductions were concentrated among the plants that grossly exceeded the standard prior to the experiment, rather than those just above the standard,

and the violations reductions were concentrated among plants that frequently exceeded the standard prior to the experiment.

Second, experimentally increasing the visibility of social media appeals about a violation by adding "likes" and "shares" to the Weibo post increased regulatory effort. Specifically, this intervention caused an increase in the probability of a regulator replying to the appeal by 40%, a doubling of the length of written replies to appeals, and the probability of an onsite investigation to jump by nearly 65%. Overall, we conclude that increased regulatory effort is an important source of the treatments' positive effects on firms' environmental performance.

Third, we find that the general equilibrium effects do not offset the partial equilibrium effects. If anything, they may even strengthen the partial equilibrium effects. This rare opportunity to assess the general equilibrium consequences of an experiment is based on randomly varying the share of firms subject to the treatments across China's 333 prefectures.

A complete cost-benefit analysis of pollution appeals is beyond the scope of this paper, but we can make a few observations about some key components of such a calculation. If publicly appealing every pollution violation generated the same response by firms as in this experiment, then there would be 51,000 fewer violations each year, a reduction of approximately 60%. This response would reduce China's total industrial SO_2 emissions by 9.2% and total industrial COD emissions by 2.9% relative to the baseline. A complete accounting of the resulting benefits would require reliable local air quality models to convert these emissions reductions into reductions in ambient air and water pollution and information on the willingness to pay for these improvements, but current pollution levies (1.26 RMB/kg for SO_2, 1.4 RMB/kg for COD) provide a lower bound. At these rates, which are generally perceived as being too low, the projected emissions reductions are worth at least 360 million RMB per year.[33]

On the cost side, we were unable to obtain data on the costs that firms incurred to reduce their emissions. However, it is worth noting that the marginal costs of citizens filing public appeals are very low — the average time used by our citizen volunteers to file an appeal in our experiment was less than five minutes. Based on the average hourly salary in China (20.1 RMB), the total labor cost of publicly appealing all pollution violations in China would be around 170,000 RMB. Even if the costs of identifying violations and preparing appeals

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[33] China emits 3,954,000 tons of industrial SO_2 every year, roughly 75% of which from the CEMS firms. So by reducing the CEMS firms' industrial SO_2 emissions by 9.2%, China's total industrial SO_2 emissions will fall by 272,826 tons. Similarly, China's yearly industrial COD emission is about 772,000 tons, and roughly 75% of which from the CEMS firms. So by reducing the CEMS firms' industrial COD emissions by 2.9%, China's total industrial COD emissions will fall by 16,791 tons. The total reduced pollution levies will therefore be 272,826*1000*1.26+16,791*1000*1.4= 367,268,160. This calculation is conservative because it ignores the potentially positive spillover effects, as well as the positive impacts on other types of pollutants.

increased these costs substantially, the costs to the public of appealing are likely still orders of magnitude lower than the benefits to them of reduced pollution.

The paper has at least a few broader implications. First, it provides experimental evidence on the impacts of citizen complaints and appeals in environmental governance and underscores the power of social media in facilitating citizen involvement in enforcing policies in China. The results imply that social media provides strong signals of public demand for stringent enforcement, which in turn prompts regulators to recalibrate their approach to the tradeoffs involved with environmental regulation. Second, it deepens our understanding of how governments, firms, and citizens interact in China's local governance system. It shows that regulators use participation and the information it reveals about public discontent to gauge the value of imposing costly regulations on firms, and particularly so when lax enforcement has the potential to generate publicity. In addition, it demonstrates that the failure to strictly enforce existing environmental policies is unlikely due to limited regulatory capacity, but instead largely driven by the lack of bottom-up pressure. A promising direction of future research is therefore trying to understand how to get more citizens to spontaneously participate in environmental governance. Finally, there is an extensive debate about the degree to which governments that are not held accountable by voting are accountable to their citizens. This paper's findings demonstrate that these governments still face important sources of accountability, and indeed China's "War on Pollution" illustrates this point more broadly (Greenstone et al., 2021).

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Figure 1. Violation Rates over Time
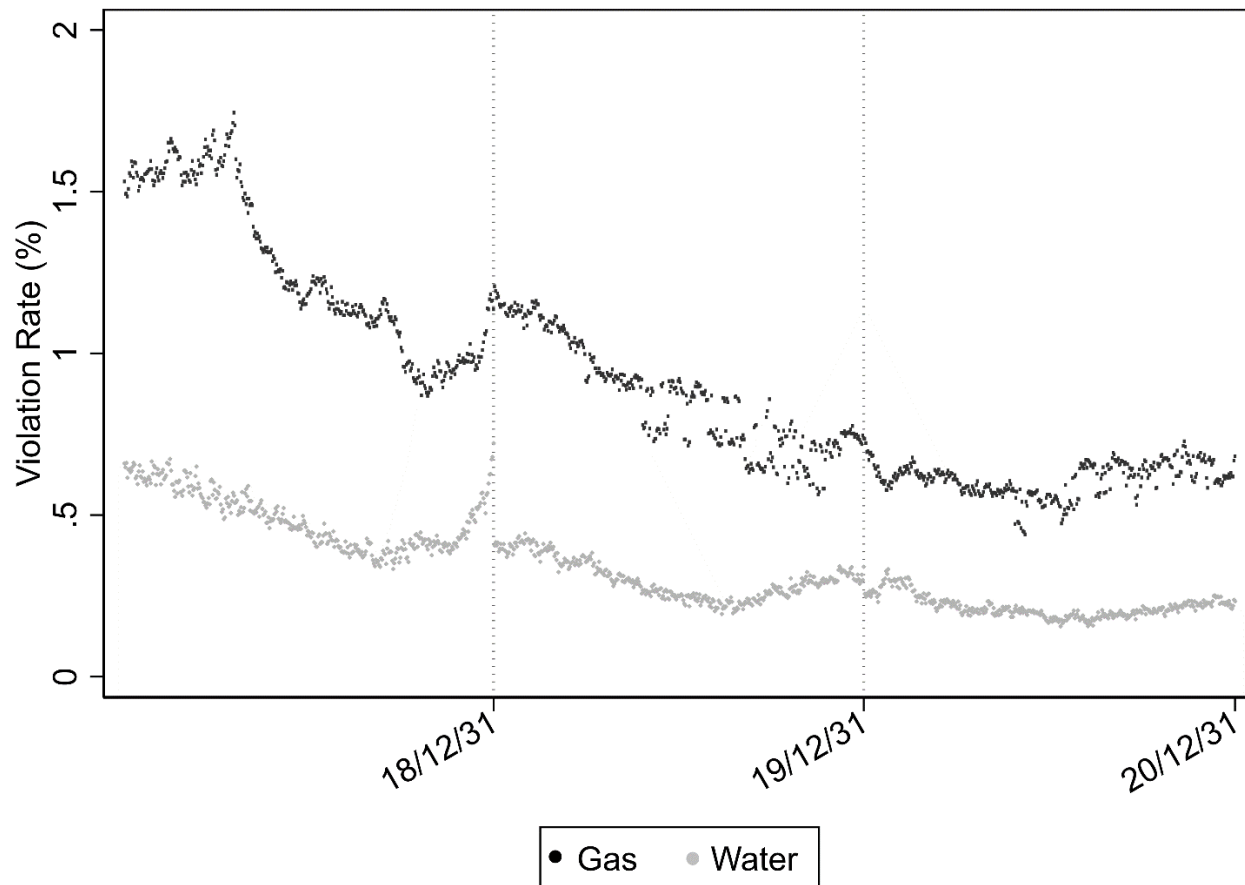
Note: This figure plots the daily trends of waste gas and water emission violations in the CEMS data, between 2018 and 2021. The Y-axis represents the ratio of CEMS firms' stacks that violated the gas/water emission standard on any given day.

Figure 2. Experimental Design
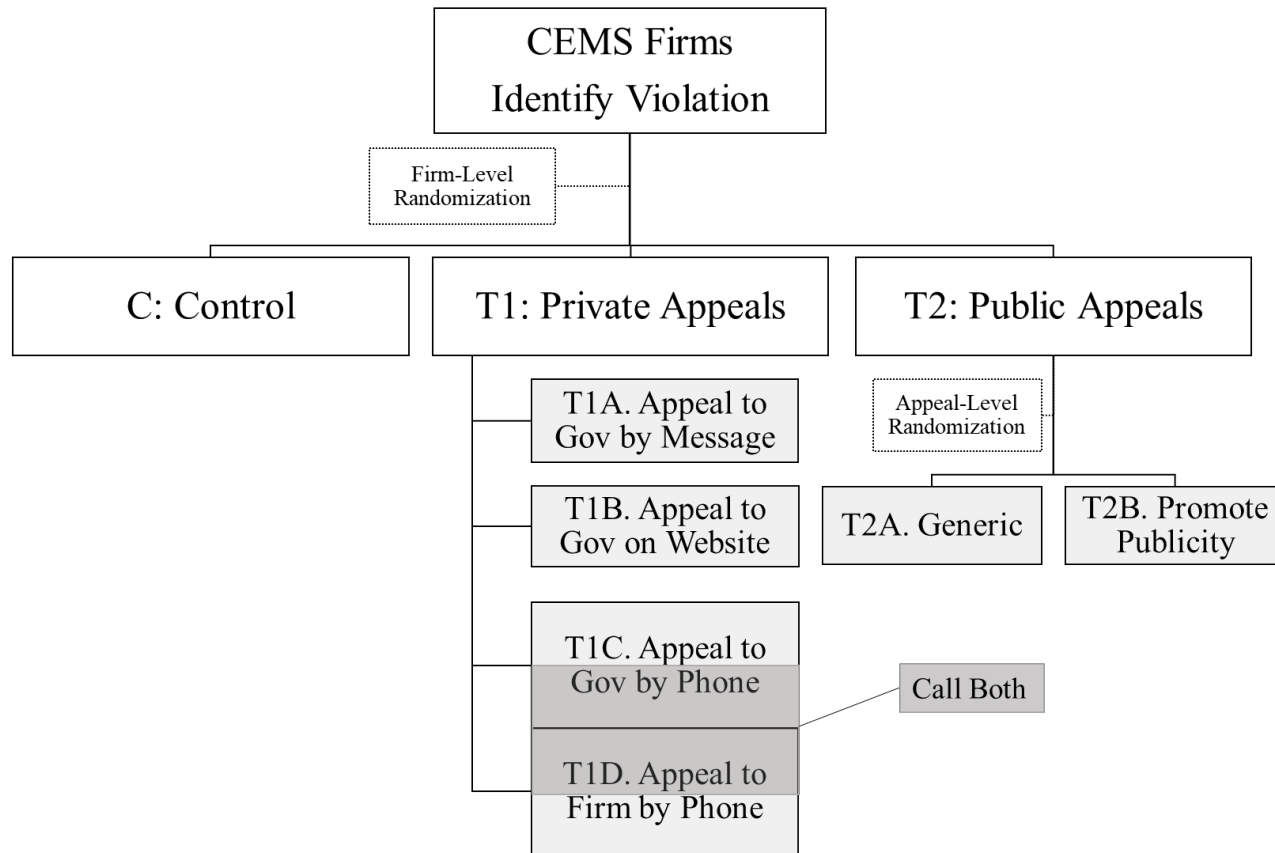
Note: This figure illustrates our experimental design, in which each CEMS firm is randomly assigned to one of seven different arms.

Figure 3. Event Studies

(a) T1: Private Appeal   (b) T2: Public Appeal
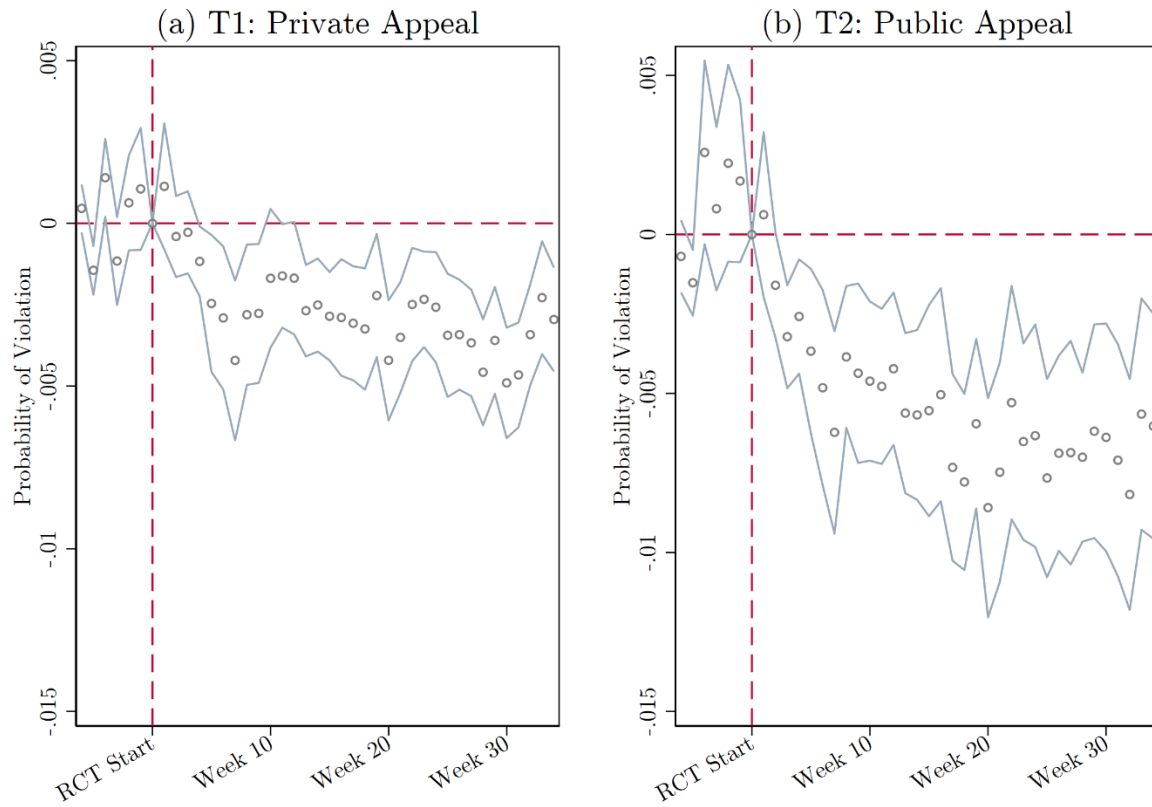
Note: This figure presents coefficients and 90% confidence intervals on Treatment*Week interactions from regressions of violation on Treatment*Week, firm FE, and week FE. Standard errors are clustered two-way by prefecture and week.

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Figure 4. Effects of Public Appeals on Excessive Violations
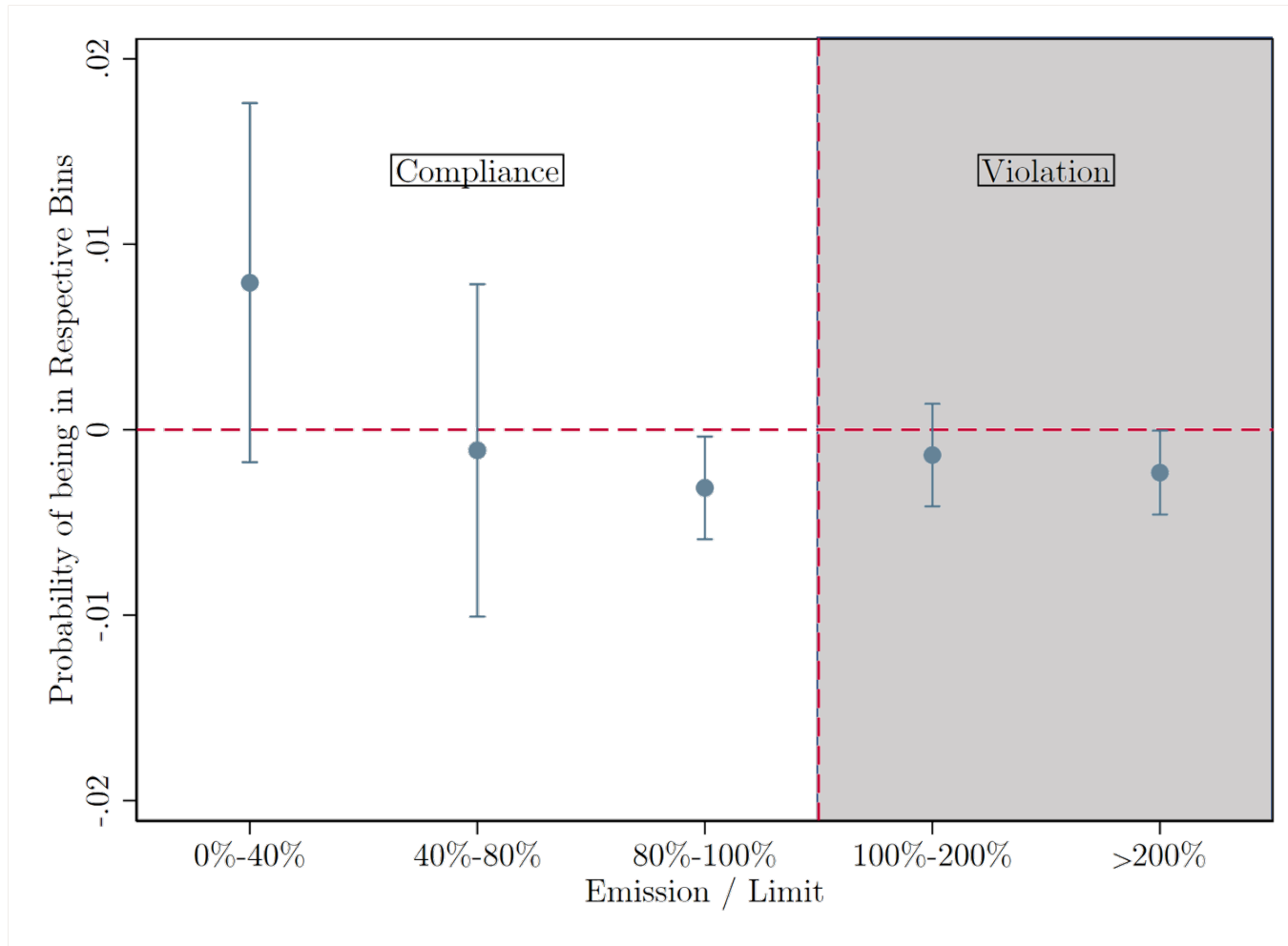
Note: In this figure, we visualize how public pollution appeals shift the distribution of emission concentrations. We divide each firm's SO2 (COD) emission concentration on a given day by the SO2 (COD) emission limit for this firm set by the MEE, and generate five bins based on this standardized emission variable. We regress the dummy variable for each bin on our treatment variables, using the same baseline specification in equation (1), and plot the coefficients and 90% CIs from these regressions. We control for firm FE and province-by-day FE. Standard errors are clustered two-way by prefecture and week.

Figure 5. Event Studies for High-Intensity Region on Ambient Air Pollution Levels

Note: This figure presents coefficients and 90% confidence intervals on High-Intensity*Week interactions from regressions of ambient SO2 air quality on High-Intensity*Week, city FE, and province-by-week FE. Standard errors are clustered two-way by prefecture and week.

Table 1. Summary of Basic Facts about the Experiment

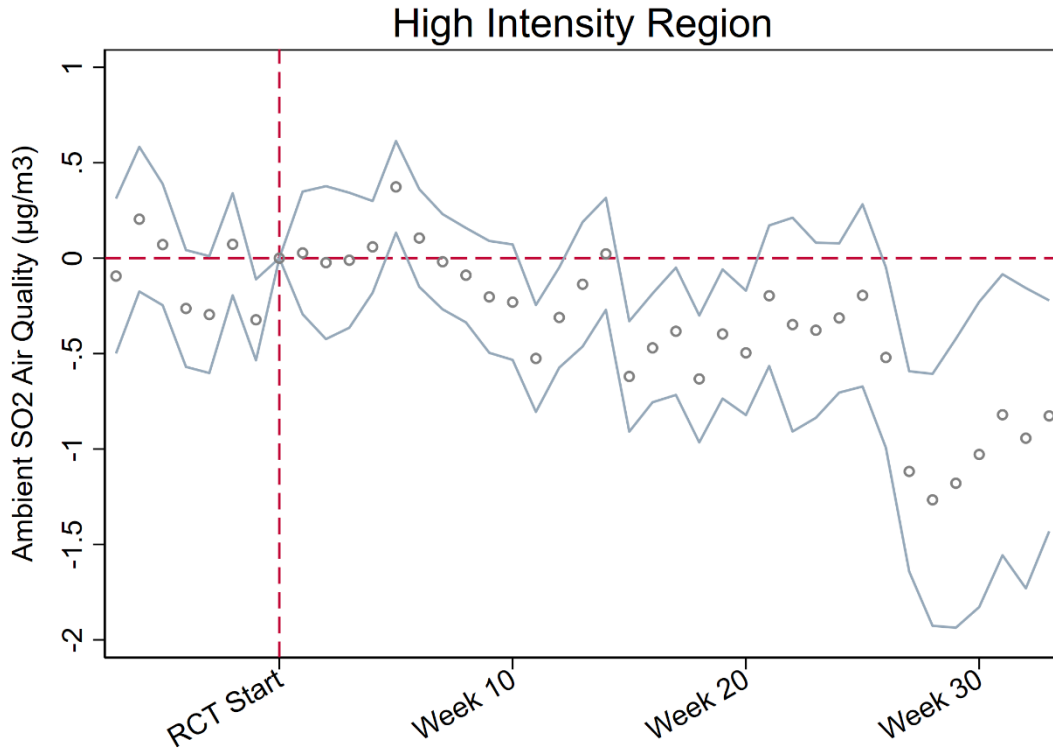|	(1)	(2)	(3)	(4)
Panel A. Experimental Environment				
	Number of Prefectures Covered	Number of CEMS Firms Covered	Number of Appeals Filed by Other Citizens During the Experiment	Number of Appeals Filed by Other Citizens about CEMS Firms During the Experiment
	333	24,620	271,859	5,478
Panel B. Experimental Team				
	Number of Partnering NGOs	Number of Citizen Volunteers on any Given Day	Number of Weibo Accounts Involved in Appeals	Number of Environmental Science Graduate Students Verifying Violations
	3	15	120	12
Panel C. Experimental Implementation				
	Number of Violations During Experiment According to CEMS Raw data	Number of Violations During Experiment Verified by Research Team	Number of Appeals Filed by Research Team	Number of Formal Responses to Appeals Filed by Research Team
	12,596	5,366	2,941	1,161

Note: This table reports the background and the implementation of our experiment. Our experiment started on May 6, 2020, and ended on December 31, 2020. Information on the number of appeals filed by other citizens is obtained from the MEE's administrative record, which we matched to the CEMS sample. The three partnering NGOs, who prefer to remain anonymous, helped us recruit and organize citizen environmental volunteers, maintaining 15 individuals ready to file appeals on any given day. The number of verified appeals is lower than the number of appeals in the CEMS raw data, since we were conservative and excluded case that might be driven by outliers or mechanical errors. The number of appeals filed is lower than the total number of violations verified by the research team, because we did not appeal against violations committed by the control firms, nor did we file appeals repeatedly within a week about the same firm.

Table 2. Balance Test

	(1)	(2)	(3)	(4)	(5)	(6)
	Control	Private Appeals				Public Appeals
		Messaging	Website	Call Gov	Call Firm	Weibo
	C	T1A-C	T1B-C	T1C-C	T1D-C	T2-C
Panel A: Outcomes						
SO2 Violations	0.217	0.011	0.030	0.022	0.052	0.082
	(2.202)	(0.052)	(0.072)	(0.052)	(0.058)	(0.078)
COD Violations	0.095	0.014	0.006	0.001	0.017	0.036
	(0.862)	(0.023)	(0.023)	(0.020)	(0.024)	(0.025)
Total Violations	0.739	0.084	0.034	0.000	0.120	0.181
	(4.927)	(0.125)	(0.128)	(0.128)	(0.136)	(0.156)
SO2	135.2	-21.7	-14.5	-19.1	-37.0	-8.4
Concentrations	(982.0)	(21.6)	(15.8)	(22.9)	(33.5)	(18.2)
COD	57.6	1.3	3.8	1.8	2.0	0.6
Concentrations	(69.1)	(2.2)	(3.1)	(3.5)	(2.4)	(3.6)
Gas Penalty	0.008	0.001	-0.003	-0.000	0.002	-0.003
	(0.146)	(0.004)	(0.003)	(0.002)	(0.004)	(0.003)
Water Penalty	0.001	0.001	0.002	0.003	0.002	0.002
	(0.055)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Total Penalty	0.009	0.002	-0.002	0.003	0.004	-0.001
	(0.156)	(0.004)	(0.003)	(0.003)	(0.004)	(0.003)
Frequent	0.055	-0.004	0.008	0.011	0.005	0.008
Violators	(0.228)	(0.008)	(0.008)	(0.007)	(0.006)	(0.008)
Panel B: Industries						
Mining Industry	0.024	0.001	-0.004	0.001	-0.002	0.003
	(0.154)	(0.004)	(0.006)	(0.005)	(0.004)	(0.006)
Manufacturing	0.730	0.015	0.018	0.021	0.012	0.019
& Power Plants	(0.444)	(0.012)	(0.015)	(0.014)	(0.011)	(0.015)
Sewage	0.166	-0.017*	-0.015	-0.007	-0.007	-0.012
Treatment	(0.372)	(0.010)	(0.012)	(0.013)	(0.010)	(0.013)
Others	0.080	0.001	0.001	-0.015	-0.003	-0.010
	(0.272)	(0.007)	(0.010)	(0.010)	(0.007)	(0.009)

Note: This table reports balance tests across different experimental arms using data from the pre-treatment period. For outcomes on pollution concentrations and violations, the sample includes eight weeks before the start of the experiment. For pollution penalties, the sample is from 2019. For frequent violators, we define a firm as a frequent violator if it violated more than ten times in 2019. Column 1 reports the means and standard deviations of the control arm. Columns 2-6 report the difference between each appeal arm and the control arm. We control for province FE. Standard errors are clustered at the prefecture level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3. Pollution Appeals and Firm Violations / Emission Concentrations

	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)
	Violation	Violation	SO2	SO2	COD	COD
Private Appeals (T1*Post)	-0.003***	-0.002**	-5.6	-5.9	-0.3	-0.4
	(0.001)	(0.001)	(3.6)	(3.6)	(0.9)	(0.8)
Public Appeals (T2*Post)	-0.006***	-0.006***	-15.8***	-16.2***	-2.1*	-2.2*
	(0.002)	(0.001)	(4.4)	(4.4)	(1.2)	(1.2)
H0: T1<T2	P=0.01	P=0.00	P=0.01	P=0.02	P=0.03	P=0.03
Control Mean	0.009	0.009	132.5	132.5	59.1	59.1
Control SD	0.096	0.096	539.5	539.5	78.8	78.8
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Day FE	Yes		Yes		Yes	
Province by Day FE		Yes		Yes		Yes
Observations	7,100,881	7,100,881	2,216,208	2,216,208	2,459,622	2,459,622

Note: This table reports the regression results from estimating Equation (1). In Columns (1a) and (1b), we use firm-day level data, and the outcome variable is a dummy variable that equals 1 if the firm violates an emission standard on that day, and zero otherwise; in Columns (2a) and (2b), we use pipe-day level data, and the outcome variable is the daily average emission concentration of SO2 (mg/m3); in Columns (3a) and (3b), we use pipe-day level data, and the outcome variable is the daily average emission concentration of COD (mg/L). For each outcome, in the column "a", we control for firm FE and day FE; in the columns "b", we control for firm FE and province-by-day FE. Standard errors are clustered two-way by prefecture and week. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4. Heterogeneity Analyses on Firm Violations and Emission Concentrations

	(1a) Violation	(1b) SO$_2$	(1c) COD	(2a) Violation	(2b) SO$_2$	(2c) COD	(3a) Violation	(3b) SO2	(3c) COD
Private Appeals (T1*Post)	-0.002*	-6.9*	-0.3	-0.002	-7.1	-0.1	-0.000	-7.9	0.2
	(0.001)	(4.0)	(1.0)	(0.001)	(4.4)	(1.0)	(0.001)	(5.1)	(0.8)
Public Appeals (T2*Post)	-0.005***	-19.4***	-2.3	-0.006***	-18.9***	-2.2	-0.001	-15.7***	-2.0
	(0.002)	(5.1)	(1.4)	(0.002)	(5.3)	(1.6)	(0.001)	(5.5)	(1.6)
Private Appeals (T1*Post)*SOE	-0.003	8.2	0.6						
	(0.004)	(15.0)	(2.7)						
Public Appeals (T2*Post)*SOE	-0.005	24.4	1.9						
	(0.005)	(17.0)	(2.9)						
Post*SOE	0.003	-6.9	0.1						
	(0.004)	(6.3)	(2.3)						
Private Appeals (T1*Post)*Final				-0.002	7.8	-1.1			
				(0.002)	(4.8)	(1.5)			
Public Appeals (T2*Post)*Final				0.002	10.1*	-0.3			
				(0.003)	(5.9)	(2.5)			
Post*Final				0.002	-3.2	2.7**			
				(0.002)	(4.9)	(1.3)			
Private Appeals (T1*Post)*Frequent							-0.013*	9.4	3.0
							(0.006)	(7.7)	(2.7)
Public Appeals (T2*Post)*Frequent							-0.030***	-2.0	4.9
							(0.009)	(12.8)	(3.2)
Post*Frequent							-0.028***	-5.7	-5.1*
							(0.007)	(4.6)	(2.7)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province by Day FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6,016,662	1,971,513	2,128,655	6,016,662	1,971,513	2,128,655	5,827,579	1,887,624	1,998,089

Note: This table reports the results for heterogeneity analyses. In Columns "a", we use firm-day level data, and the outcome variable is a dummy variable that equals 1 if the firm violates an emission standard on that day, and zero otherwise; in Columns "b", we use pipe-day level data, and the outcome variable is the daily average emission concentration of SO2 (mg/m3); in Columns "c", we use pipe-day level data, and the outcome variable is the daily average emission concentration of COD (mg/L). SOE is a dummy variable that equals 1 if the firm's majority shareholder is the government. Final is a dummy variable indicating whether the firm produces final good instead of intermediate good based on its industry code. Frequent is a dummy variable that equals 1 if the firm committed pollution violations in the seven weeks prior to the experiment. We control for firm FE and province-by-day FE. Standard errors are clustered two-way by prefecture and week. * p < 0.10, ** p < 0.05, *** p < 0.01

Table 5. Social Media Publicity and Government Responsiveness

	(1a) Whether Respond	(1b) Whether Respond	(2a) Response Length	(2b) Response Length	(3a) Onsite Audit	(3b) Onsite Audit
Visibility Promotion (T2B)	0.06*	0.06**	34.6**	33.8**	0.04*	0.05**
	(0.03)	(0.03)	(13.4)	(13.4)	(0.02)	(0.02)
Control Mean	0.16	0.16	33.1	33.1	0.07	0.07
Control SD	0.36	0.36	117.9	117.9	0.26	0.26
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Province FE		Yes		Yes		Yes
Observations	662	658	662	658	662	658

Note: This table reports the regression results for public Weibo appeals on local government responsiveness. We use the sample of firms in the public Weibo appeal to government arm. The unit of analysis is each Weibo appeal. Whether respond is a dummy variable that equals 1 if the government replies to our Weibo appeal, and 0 otherwise; response length is the word count of the government's Weibo reply to our appeal, which is counted as zero if there is no response; onsite audit is a dummy variable that equals 1 if the government replies to our Weibo appeal with proof of an onsite investigation, and 0 otherwise. For each outcome, in the column "a", we control for month FE; in the column "b", we control for month FE and province FE. * p < 0.10, ** p < 0.05, *** p < 0.01

Table 6. General Equilibrium Effects of Pollution Appeals

	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)
	Violation	Violation	SO2	SO2	COD	COD
Panel A: Control Group						
High Intensity*Post	0.000	0.001	-5.3	-9.0	1.5	1.9
	(0.002)	(0.002)	(5.6)	(7.6)	(2.1)	(1.9)
Observations	1,024,692	1,024,692	296,604	296,604	356,265	356,265
Panel B: Treatment Group						
High Intensity*Post	-0.003*	-0.002*	-0.6	0.8	-0.7	-0.1
	(0.001)	(0.001)	(5.4)	(5.8)	(1.7)	(1.6)
Observations	6,062,153	6,062,153	1,919,513	1,919,513	2,103,337	2,103,337
Control Mean	0.009	0.009	132.2	132.2	58.9	58.9
Control SD	0.093	0.093	536.4	536.4	77.0	77.0
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Day FE	Yes		Yes		Yes	
Province by Day FE		Yes		Yes		Yes

Note: This table reports the results of general equilibrium analyses. Panels A and B report the impact of assignment to the 95% prefecture group, relative to the 70% group for the control and treatment groups. In Columns (1a) and (1b), we use firm-day level data, and the outcome variable is a dummy variable that equals 1 if the firm violates an emission standard on that day, and zero otherwise; in Columns (2a) and (2b), we use pipe-day level data, and the outcome variable is the daily average emission concentration of SO2 (mg/m3); in Columns (3a) and (3b), we use pipe-day level data, and the outcome variable is the daily average emission concentration of COD (mg/L). For each outcome, in the column "a", we control for firm FE and day FE; in the columns "b", we control for firm FE and province-by-day FE. Standard errors are clustered two-way by prefecture and week. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$