Regulatory Sanctions and Firm Environmental Performances in China

Qing Deng, Political Science, Penn State
Yangyang Li, School of Business, Central South University
Xun Cao, Political Science and Public Policy, Penn State
Zhen Lei, Energy and Mineral Engineering, Penn State

September 14, 2020[1]

Abstract: One major challenge for improving environmental quality in developing countries is the enforcement of environmental regulations. There are different enforcement instruments available to environmental agencies and it is still not well understood how effective these instruments are in developing countries. This paper evaluates the effectiveness of regulatory sanctions – monetary fines, temporary suspension of production, rectification, and long-term factory shutdown – in changing a non-compliant firm' environmental spending and emissions. We construct a panel dataset of 6,529 major polluting firms in Hunan province, China, 2011-2015. Combining matching with a difference-in-differences (DID) approach, we find that sanctions concerning water pollution violations increased firms' operating expenses in wastewater treatment and decreased water pollutant emissions. We did not find significant impact for sanctions associated with air pollution, which might be explained by Hunan's greater susceptibility to water pollution and its provincial government's emphasis on improving water quality. We further find that it was mostly the mere action of sanctioning that serves warning purposes to violators, rather than sanction severity or media disclosure of sanctions, that changed firm behaviors. Finally, there exists little spillover effect from sanctioned firms to unsanctioned firms in the same county or same industry.

[1] Contact author: Xun Cao (xuc11@psu.edu). Earlier versions of this paper were presented at EPG 2020, Oslo, June 25, 2020 and APSA 2020, September 10-13, 2020. We received valuable comments from discussants Jonas Nahm and Vally Koubi and other participants of the conferences. All errors are ours.

1. Introduction

The effectiveness of an environmental policy is a function of both its design features and enforcement. For developing countries in particular, enforcement is often a major challenge due to factors such as low state capacity and poor infrastructure (Ryan 2014), lack of incentive for local officials to seriously enforce (Duflo et al. 2013; Kahn et al. 2015; McRae 2015; Chen et al. 2018), and various degrees of corruption (Burgess et al. 2012; Oliva 2015). Indeed, many developing countries have tough environmental regulations on the books, yet have trouble achieving their environmental goals (Greenstone and Hanna 2014). For instance, legal scholars have long pointed out that the Chinese environmental legislation on paper is plentiful and powerful, but the problem is local enforcement (Van Rooij 2006). Local agencies could decide non-, weak, or selective enforcement, which undoubtfully impacts effectiveness.[2]

The choice of enforcement instruments also matters for policy effectiveness. A variety of enforcement instruments exist, and they could differ in effectiveness. For instance, to deter non-compliance, a local environmental agency can increase the frequency of its inspections, publicize information of non-compliers, or impose on non-compliers with sanctions that can range from monetary fines (with various amounts), temporary suspension of production (of a production line or a whole factory), to long-term or even permanent factory shutdown. Moreover, even for a specific enforcement instrument that a local agency uses to enforce regulation, firms might respond differently in different contexts. Indeed, except for the case of inspections (e.g., Dasgupta et al. 2001; Escobar and Chavez 2013), it is still not well understood how effective the aforementioned enforcement instruments are, particularly in a developing country context.

This study focuses on the effectiveness of probably the most basic regulatory enforcement instrument – sanctions (i.e., government punitive actions or penalties) such as monetary fines, temporary suspension of production, rectification, and long-term factory shutdown – in changing firms' environmental spending and emissions. Regulatory sanctions are often the foundation and the last resort for command and control policies: for instance, emission standards are set up by the government; noncompliance by a firm would be sanctioned.[3] Furthermore, sanctions matter also for other major types of environmental policies. For example, market-incentive regulations such as environmental taxes require firms to self-report emissions (subject to government verification), and sanctions can be issued to penalize fraudulent reporting. For information-based

[2] One example of non-enforcement by local officials is the case of water pollution in China's Huai River. Despite central government efforts, it remains one of the most polluted rivers in the country. Polluting factories along the river contribute significantly to local economy. Therefore, local officials had strong incentives not to (fully) enforce environmental regulations (Economy 2004). Selective enforcement implies that regulatory agencies target some firms more than others. For instance, Konisky and Reenock (2013) and Konisky and Teodoro (2016) find that factors such as local community characteristics and facility ownership affect government enforcement decisions in the US. Empirical studies of China show that state-owned firms and those in a dire financial situation are much less likely to be targeted by local agencies (Wang et al. 2003; Wang and Wheeler 2005; Maung et al. 2016).

[3] Even though command and control policies are more traditional regulatory policies compared with information-based (e.g., the environmental rating of firms) and market-based (e.g., environmental taxes) policies, we still do not have a good understanding of their effectiveness in developing countries other than a few command and control policies that have received a critical mass of studies such as license plate-based driving restrictions (Blackman, Li, and Liu 2018).

regulations such as the Toxics Release Inventory (TRI) Program by the US EPA and a recent Chinese five-color ranking scheme of nationally key monitored firms, where firms need to make information available to the government, non-compliance such as withholding critical information or data manipulation is subject to government sanctions as well.

Surprisingly, past literature has done little to study the effectiveness of environmental regulatory sanctions in developing countries. One explanation for this lack of scholarly attention is probably a common belief that a non-compliant firm, once received a sanction, would naturally make efforts to comply, which makes it almost trivial to study the effectiveness of sanctions. However, there are many anecdotal cases from developing countries showing that sanctions might not always lead to compliance. For instance, fines might be too low to the point that paying the fine makes economic sense than investing in better equipment. Some firms obey the sanction of temporary suspension of production during the day while secretly resuming production during the night when monitoring by local agencies is difficult. Another explanation for the scarcity of scholarly work on this topic is the lack of firm level information on both sanctions and environmental performance.

To estimate the impacts of regulatory sanctions on business behavior, we construct a unique firm-level data set of 6,529 major polluting firms in Hunan province, China, between 2011 and 2015. Hunan province, located in central China and ranked in the middle in GDP,[4] is quite representative in terms of economic development and environmental challenges in the country. Nearly half of its GDP is from industrial and manufacturing sectors including some that are energy intensive and environmentally challenging such as metallurgy and steel industries. Perhaps more importantly, Hunan province, where the Yangzi River winding through and numerous tributaries and lakes dotting across, is endowed with abundant water resource and thus a crucial region for rice cropping. Compared to air pollution, water pollution is much more severe and salient in Hunan, and the provincial government prioritizes water pollution reduction. This provides an interesting context to study whether the effectiveness of sanctions differ between water pollution and air pollution that vary in salience and policy priority.

The data compiled in our study is unique and comprehensive for our study purpose. The longitudinal firm-level data on environmental performance was obtained from the provincial Ecology and Environmental Department (EED) and covers 6,529 biggest polluting firms that are subject to official inspections and verifications. Together, their emissions account for 85% of total emissions of at least one pollutant (COD, NH, and SO_2) in the province. The data contains annual information about firm expenditure and emissions relate to both water and air pollution. The firm level sanction data was compiled using manual labelling of the raw sanction documents which were disclosed by the government and collected (via continuous web crawling) by the Institute of Public & Environmental Affairs (IPE), a Beijing-based environmental nongovernmental organization.

We combine matching with a difference-in-differences (DID) approach. The DID analysis finds that sanctions concerning water pollution increased firms' operating expenses in wastewater treatment devices and decreased water pollution. The dynamic analysis, comparing sanctioned firms and matched unsanctioned firms in each year, shows that these two groups of firms were largely comparable prior to sanctions, but sanctioned firms increased water treatment spending and reduced water pollution throughout the post-treatment years, including the year of

[4] In between provinces in Eastern China that are more advanced and developed and provinces in Western China whose development lags behind.

the sanction, suggesting a causal linkage between sanctions and improvement in firm environmental performance. By contrast, for sanctions concerning air pollution, we did not find significant impact. These results are consistent across many robustness checks. Our further analysis provides supportive evidence that the uncovered discrepancy in firm responses to water-based sanctions versus air-based sanctions may have to do with the salience of water pollution on Hunan's political and public agenda.

We then explore two potential mechanisms through which sanctions affect firms' water-related environmental performance: media disclosure and sanction severity. First, we differentiate between sanctions that were only disclosed on environmental agencies' websites and those disclosed via public news and media. The former often takes the forms of official enforcement orders and documents: one needs to go to the website and devote time and effort to read through the text and data to extract sanction information. The latter provides direct and succinct information to a general audience, name-and-shames violators in public, therefore would have a stronger effect if media exposure increases effectiveness. However, we find that firms responded to water-related sanctions largely because of sanctions themselves, not due to public media disclosure. Next, we examine whether firm response is sensitive to the level of sanction severity. We find that more severe penalties do not entail additional effect, suggesting again that it was mostly that mere action of sanctioning that serve warning purposes to violators, not sanction severity, that changed firm behaviors. Finally, we explore potential spillover effects of sanctions, either geographically (within a same county) or industry wise (within the same industry). Our results suggest no strong spillover effects within the Hunan sample.

To our best knowledge, this is the first study that carefully examine the effects of regulatory sanctions on firm environmental performance, including ratcheting up environmental spending as well as reducing emissions, in developing countries. Seroa de Motta (2006) is the only existing study that addresses such questions in a developing country context.[5] Using a cross-section data of 325 firms from a firm-level survey conducted in Brazil in 1998, he finds that a firm's self-reported regulatory sanction is positively associated with its environmental practice index based on its current level of environmental management procedures.[6] Our study differs from Seroa de Motta (2006) in significant ways. First, we use a five-year panel data involving a much larger sample of firms (6,529) that cover all major polluting firms in a Chinese province. This allows us to combine matching with a difference-in-differences (DID) design to identify the causal effect of sanctions. Second, we examine a set of dependent variables which, we believe, better capture the de facto environmental performance of a firm than indices based on self-reported environmental management procedures. Third, we explore the causal mechanisms through which water-related sanctions impacted firm behaviors and whether sanctions entailed spillover effects.

Our analysis yields several interesting findings that have important policy implications for China and could provide insights for other countries. First, the finding that the effectiveness of regulatory sanctions depends on whether the focal environmental issue is prioritized by the government agenda, which could be in turn due to issue salience among the public, offers

[5] Almer and Goeschl (2010) study criminal sanctions' effect on firms' environmental performances in Germany.
[6] In Seroa de Motta (2006), firms face two types of legal sanctions: administrative fines imposed by Brazilian state EPAs and remediation and clean-up legal sanctions imposed by the judiciary. Sanctions in China do not include the judiciary type.

important insight for environmental policies. Greenstone and Hanna (2014) find that in India, air pollution regulations are associated with substantial improvements in air quality but water regulations had no measurable benefits. They cautiously conclude that higher demand for air quality prompted the effective enforcement of air pollution regulations in weak institutional settings. Our study provides direct evidence that the effectiveness of regulatory enforcement depends on salience and priority of the issue. Second, we find that it was neither sanction severity nor public media disclosure, but rather the mere action of sanctioning that serves warning purposes to violators, that changed firm behaviors. This suggests that in China, an authoritarian country where the governments are powerful and extensively involved in economic management, any gestures and intentions by the government might be important for environmental regulation. Third, we find little spillover effects of sanctions in unsanctioned firms in the same county or same industry as sanctioned firms.

Our paper makes important contributions to an emerging literature on the effectiveness of specific instruments of regulatory enforcement. Our study focuses on one of the most common enforcement instruments, regulatory sanction.[7] It speaks to several recent studies that examine the effectiveness of other enforcement instruments that often are less widely used in a developing context. For instance, focusing on government environmental interviews with firms (Huan Bo Yue Tan), Shen and Zhou (2017) find that the environmental performances of companies in the interviewed areas have improved significantly. Karplus and Wu (2019) and van der Kamp (2018) study the effect of central government top-down inspection campaigns, a form of police patrol policy. The former shows that such crackdowns resulted in a large reduction in SO2 emissions in short periods before pollution reverted to prior levels after crackdowns. The latter finds that such a police patrol instrument did not even have a short-term effect in reducing pollution. Finally, Hanna and Oliva (2015) focus on plant closure and van der Kamp (forthcoming) on blunt force enforcement (i.e., destroy factories).

Our paper also contributes to an expanding literature on environmental policy in China. Environmental challenges in China not only pose considerable threats to the country's long-term development, but also have profound global implications.[8] The Chinese government has made great efforts to address domestic environmental and climate crisis by experimenting with various policy schemes and regulatory instruments such as green credit policies, environmental insurance, and information-based instruments. A better understanding of policy effectiveness and associated scope conditions is not only vital for China, but also for other countries facing similar climate and environmental challenges: many, if not all, of these policies are being implemented or could be implemented in other countries.

Indeed, China is the country that has received the most scholarly attention in developing countries regarding the effectiveness of environmental policies (Blackman, Li, and Liu 2018). However, even in this most-studied case, past studies often focus on either the design features of a policy,[9] or the overall effectiveness of government regulatory schemes (e.g., three rivers and

---

[7] Dasgupta et al. (2001) and Escobar and Chavez (2013) study the effect of inspection, which is another common instrument to increase firms' compliance.

[8] With its increasing economic size and energy consumption, China is now the largest emitter of carbon dioxide.

[9] For instance, the most studied environmental regulatory policy in China is pollution levies. Its design features often are used as explanations for its (in)effectiveness (Dasgupta et al. 2001; Wang 2002; Wang and Wheeler 2005). Pollution levies are in theory taxation on pollution,

three lakes basins (3Rs3Ls) and two control zones (TCZ)),[10] while paying much less attention to policy enforcement. This is in particular one critical shortcoming for studies on policy schemes as they can include multiple enforcement instruments. For example, in the case of Two Control Zones (TCZ), a local government can use a varieties of policy enforcement instruments, including increasing the frequency of inspections, publicizing polluting firms' non-compliance to name and shame, linking firms' environmental performances to chances of borrowing from state-controlled banks (i.e., a green credit policy), imposing fines, and in an extreme case, using a blunt force to forcibly shutter or destroy factories to reduce pollution. The overall effectiveness of the TCZ (or the lack thereof) does not tell us which policy instrument(s) worked and which did not. Our paper makes important contributions to this large literature by taking a much more focused approach and studying the effectiveness of one basic and widely used enforcement instrument – regulatory sanction.

The next section discusses regulatory enforcement and an increasing role of sanctions in China. The following empirical sections present data, method, main results, robustness checks, and further analysis on causal mechanisms and potential spillover effects. We conclude and discuss future research at the end.

2. Regulatory Enforcement and an Increasing Role of Sanctions in China

While China's rapid industrialization has generated remarkable economic growth, industrial pollution poses severe environmental problems. For example, the principal source of Beijing's choking smog is coal-fired heavy industries located in neighboring cities (Guan, Zheng and Zhong 2017). In summer, heavy industries in Hebei province are estimated to account for 50% to 70% of Beijing's PM2.5 concentrations (Streets et al. 2007). Industrial pollution also leads to

---

therefore a market-incentive based regulation. However, such levies have been largely ineffective in China because: (1) the average level of pollution levies is often too low to create a real incentive for firms to improve their environmental performance; (2) if a firm emits multiple pollutants, it only pays for its worst-performing one; (3) about 80 percent of pollution levies collected go back to firms for environmental projects and/or equipment purchases in the form of state loans and subsidies; (4) pollution levies paid by firms are tax exempt; and finally (5) local governments often offer exemptions (Dasgupta et al. 2001; Jiang and Mckibbin 2002).

[10] Several examples include: (1) since 1996, the central government has identified "three rivers and three lakes basins" (3Rs3Ls) as key regions for tougher water pollution control. Wang et al. (2018) find that more stringent regulations in 3Rs3Ls basins forced out small, heavily polluting firms; (2) In 1998, a Two Control Zones (TCZ) policy was implemented by the central government in which tougher environmental regulations were imposed in TCZ cities. Cai et al. (2016) find that environmental regulations in the TCZ cities affects patterns of foreign direct investment, especially for those from countries with better environmental protections; (3) In 2006, the 11th Five-Year Plan assigned different pollution reduction targets to provincial governments. Shi and Xu (2018) reveal a heterogeneous effect of cross-province variations in pollution reduction targets on the content and volume of Chinese firm exports; (4) Zhang et al. (2018) study the National Specially Monitored Firms (NSMF) program in which central supervision is applied to firms that are listed as NSMF. They find that central supervision significantly reduces industrial COD emissions; (5) Pollutant quantity control policies (Lin 2013; Jin and Lin 2014); and (6) most recently the green credit policy (Sun et al. 2019).

severe algae outbreaks in Taihu Lake, the third largest freshwater lake in China. In 2007, a blue-green algae outbreak affected 2.3 million residents in the city of Wuxi (Chen 2009). In addition to air and water pollution, industrial waste contaminates agricultural soil. Assessments conducted in Hunan find that heavy metal concentrations in vegetables and rice exceed the allowable limits (Liao et al. 2005; Wang and Stuanes 2003).

To effectively reduce industrial pollution, the central government has strengthened environmental regulations by first creating political incentives to realign local officials' interests with a central directive for sustainable growth. For a long time, the Chinese Communist Party (CCP)'s merit-based cadre evaluation system had prioritized economic performance. Local leaders who successfully promote economic growth stand a higher chance of promotion. This incentive structure promotes a rampant expansion of pollution-intensive industries. Since the 11th Five Year Plan (2006-2010), the central government has incorporated an increasing number of binding environmental targets into the cadre evaluation system. Local officials failing to meet reduction targets risk demotion or removal from office (Schreifels, Fu, and Wilson 2012).

Moreover, the Chinese government has reformed its regulatory system. For decades, market incentives in the form of pollution fees dominated pollution control in China. Since fee rates are too low, pollution fees fall far short in controlling industrial pollution.[11] The central government now promotes regulatory pluralism to incentivize more stakeholders (e.g., consumers, NGOs, and communities) to develop and enforce environmental regulations. More market mechanisms and information tools have been applied, including cap-and-trade, information disclosure, and environmental insurance. However, the existing evidence, still thin, shows that the usefulness of these new instruments is limited (Yin, Zhang, Wang 2020).

Another route taken by the government is to develop new mechanisms to strengthen the enforcement of traditional command-and-control regulations, expanding the use of regulatory sanctions. Such efforts include top-down police patrol and bottom-up fire alarm mechanisms to make local malfeasance and business noncompliance more likely to be detected. Since late 2015, the central government has launched rounds of national environmental inspections to detect and investigate violations, holding both polluting entities and local officials accountable. In 2016, 487 people were punished for data manipulation in Hebei province alone, hundreds of them being local officials (Kostka and Zhang 2018). In addition to police patrols, citizens are encouraged to call into the national 12369 hotlines to report violations. With more information collected, regulatory sanctions can be applied more often and with better accuracy.

Another factor that makes sanctions more important is an institutional change that has empowered the environmental regulatory authorities and protected them from interferences of local governments. In the past, local governments, which controlled personnel appointment and budget allocation, exerted a strong influence over local environmental agencies (Lieberthal 1997). Local regulators had to prioritize local governments' preferences over regulatory responsibilities. To centralize power of environmental regulation, the central government formed the Ministry of Environmental Protection (MEP) in 2008. In 2018, the Ministry of Ecology and Environment (MEE) superseded the MEP to take on more responsibilities. In addition, more

[11] For example, in 2005, coal-fired power plants are charged 0.63 yuan for each kilogram of SO2 emissions. In comparison, the unit abatement costs of SO2 were about 5 yuan on average (Jin, Andersson, and Zhang 2016). With the abatement costs approximately eight times the pollution fee, polluters have no incentive to reduce emissions through means such as upgrading production processes and installing control devices (Stavins and Whitehead 1992).

institutional reforms were introduced to prevent local governments from interfering with enforcement. For instance, local regulators became directly responsible to environmental authorities at higher levels (Ma 2017). These institutional rearrangements allow environmental authorities to sanction polluters without being pressured by local leadership.

Thus, it is crucial to understand whether and under what conditions regulatory sanctions are effective. The empirical findings from our study shed lights on these important questions and have important policy implications for the future of Chinese environmental regulations.

3. Data and Empirical Approach

3.1 Data

Our firm-level data on environmental performance is from the Ecology and Environmental Department (EED) of Hunan province, which has the most comprehensive pollution data on manufacturing enterprises within the jurisdiction. Companies on file include major manufacturers that are subject to official inspections and verifications: these are 6,529 biggest polluting firms whose emissions account for 85% of total emissions of at least one pollutant in the province. The purpose of official inspections and verifications is to verify self-reported data. Self-reporting has played a central role in pollution control because it serves as a basis for the calculation of pollution fees and total emission control. However, as scholars and practitioners have pointed out, self-reporting can be problematic as polluters can underreport and delay reporting (Kostka 2015). The fact that our data, though self-reported by firms, is verified by environmental regulators through means such as production-based estimations and surprise inspections, provides confidence on the quality of the data.

Another piece of empirical evidence also suggests that data manipulation by self-reporting firms is unlikely to be prevalent in our data. We only find a spending-increasing and pollution-reducing effect in the case of water pollution sanctions, but not the in the case of air pollution sanctions. Manipulating data by a firm is often done by changing emission numbers. A firm reports water and air pollution data together to the same local agency. Therefore, it is unlikely that a sanctioned firm would only manipulate environmental spending and emission data in water pollution, but not in air pollution.[12]

We focus on environmental performance in wastewater-related and waste air-related emissions, and operating expenses of wastewater and waste air treatment devices. Specifically, we examine how sanctions concerning water pollution affected firm expenditure on wastewater treatment and water-related emissions including total wastewater emissions, COD (chemical

[12] Even if we cannot completely rule out cases of some firms over-reporting their environmental spending and under-reporting water and air pollution, such self-reporting is biased against us finding our key result. This is because practically, biased reporting is more difficult for sanctioned firms than those without sanctions since local governments pay more attention to the former: e.g., after receiving a sanction of temporary suspension of production, a firm would be closely monitored and examined by a local EED. Therefore, when comparing a sanctioned firm's change in pollution after and before receiving a sanction to that of a similar but unsanctioned firm, and if we find that the former reduces more pollution than the latter, it is unlikely that such difference in pollution reduction is a function of the sanctioned firm under-reporting more than the unsanctioned firm: the sanctioned firm is simply much more closely monitored by the local EED.

oxygen demand) emissions, and NH (ammonia) emissions. Likewise, we test how sanctions concerning air pollution influenced expenditures for operating waste air treatment devices and waste-air-related emissions including total waste air emissions, SO_2 (sulfur dioxide) emissions and, NO_x (nitrogen oxides) emissions. A firm's operating expenses of waste treatment devices reflect its efforts in reducing pollution emissions. The pollutants we study have been considered major pollutants and used by the government as yardsticks to assess local environmental performance.

Information about regulatory sanctions is based on officially disclosed enforcement decisions, which in their original formats (including texts, images, and tables) were collected (via continuous web crawling) by the Institute of Public & Environmental Affairs (IPE), a Beijing-based environmental nongovernmental organization. In 2007, the government issued the Open Government Information (OGI) Regulations, with disclosure of regulatory and enforcement activities being a core element. The then Ministry of Environmental Protection adopted a set of specific regulations on the disclosure of environmental information and put them into effect in May 2008 together with the general OGI Regulations.[13] Given that our study period covers years 2011-2015 when the OGI regulations had been implemented, and that the sample of firms are those closely monitored by the provincial EED, we expect that sanction documents that IPE collected, on these firms, should be comprehensive.

On a less positive note, we find that not all enforcement documents disclosed or publicized is satisfactory in quality; local environmental agencies sometimes release partial information indicating a firm is sanctioned without clear information about what the sanction is.[14] Accordingly, in our main analysis, we pooled together sanction of all kinds (including warning of violation, temporary suspension of production for corrections, fines, business license revoking, and forcible closure of production lines or plants), to assess the effect of environmental sanctions on firms' environmental performance. We also conducted analyses that differentiate sanction types, dropping firms with sanctions of unclarified types.

In the main analysis, the treatment group consists of firms sanctioned in ONLY one year during the period of 2011-2015. If a firm was sanctioned multiple times but only in one year, this firm is still assigned to the treatment group.[15] We excluded 10 firms that were sanctioned in more than one year. This is because they might be different from those with only one treatment year. On the one hand, firms that received repeated sanctions might be more likely to increase their environmental spending and decrease emissions, causing a bias in favor of finding sanctions effective. On the other hand, they could be more "stubborn" or defiant firms that did not change their emissions following previous sanctions; including such firms in our analysis creates a bias

[13] According to Article 11, environmental agencies should proactively disclose details about coercive measures taken by regulators.
[14] For example, in 2013, three subsidiaries of Hunan Shuikoushan Nonferrous Metals Group *Co*., *Ltd*. were detected by Hunan EED to have wastewater pH values higher than the required range. In addition to immediate corrections, Hunan EED required that local environmental agencies impose further penalties and inspections. However, detailed sanction information, including types of sanctions, was missing in the document.
[15] In the robustness section, we include an interaction term between the DID indicator variable $(D_{i,t})$ and a dummy variable indicating repeated sanctions in one year. Table I.2 shows that repeated sanctions do not have additional effect than single sanctions.

against finding sanctions effective. In Table B.3 in the online appendix, we show that including these firms do not change our result.

We differentiate between water pollution related and air pollution related sanctions: 1) sanctions aiming at reducing water-related substantive emissions and procedural violations and 2) sanctions aiming at reducing air-related substantive emissions and procedural violations. Firms with no sanction of any kind during the study period are assigned to the control group. Table A.5 shows descriptive statistics of the variables for the treatment group and control group respectively.

3.2 Matching

There exist potential endogeneity concerns because it is possible that sanctions were not randomly assigned among firms. Thus, a simple difference-in-differences (DID) model, comparing sanctioned firms versus unsanctioned firms, might lead to biases in estimation. Although there is no significant difference between the two groups in terms of the outcome variables before matching, as shown in Table A.3-1 of the online appendix, we use matching methods to pair treated firms with control firms. Matching allows us to further address endogeneity concerns by ensuring that firms in the treatment and control groups are comparable based on the observables.

There are three commonly used matching approaches, the propensity score matching (PSM), the coarsened exact matching (CEM), and the Mahalanobis distance matching (MDM). Recent studies suggest the adoption of CEM because PSM can be insensitive to information in covariates not represented in the propensity score (King and Nielsen 2019). Moreover, applied in our data, CEM outperforms PSM in improving the balance between the treatment and the control groups (Figure 1 and Figure 2). CEM also enables us to include many covariates, especially industry dummy variables, in the matching process which MDM fails to do so.[16]

Our main analysis therefore is based on the CEM approach,[17] using covariates measuring a firm's scale measured by its output value (log), business starting year, distance to Hunan EED, and its industry.[18] We coarsen the first three variables by setting up cut-off points,[19] with the fourth one (industries) being dummy variables. We use these covariates because they strongly influence firms' emissions and abatement costs. For example, larger firms, older firms with

---

[16] Du and Takeuchi (2019) use Mahalanobis Distance Matching (MDM). However, a problem with MDM is that it does not work well when there are too many covariates. While Stuart (2010) suggests that the number of covariates should be fewer than 8 for MDM, we have 39 industry dummy variables to include in matching. Since industries have a large influence in firm environmental performance, we choose not to use MDM in the analysis.

[17] CEM coarsens each variable by recoding and grouping values based on substantive information. For instance, instead of using one person' actual age – 0, 1, 2, 3, … 100, 101, …, the recoded age variable coarsens the values of age into a few categories such as, for example, below 10, between 11 and 20, etc. It then creates a set of strata. Each stratum has the same coarsened values of covariates. Units in strata with at least one treated and one control unit are kept, and units in the remaining strata are removed from the sample.

[18] Three covariates, business start year, distance to Hunan EED, and industry, are time-invariant. For firm output value, we calculate its mean value during the study period.

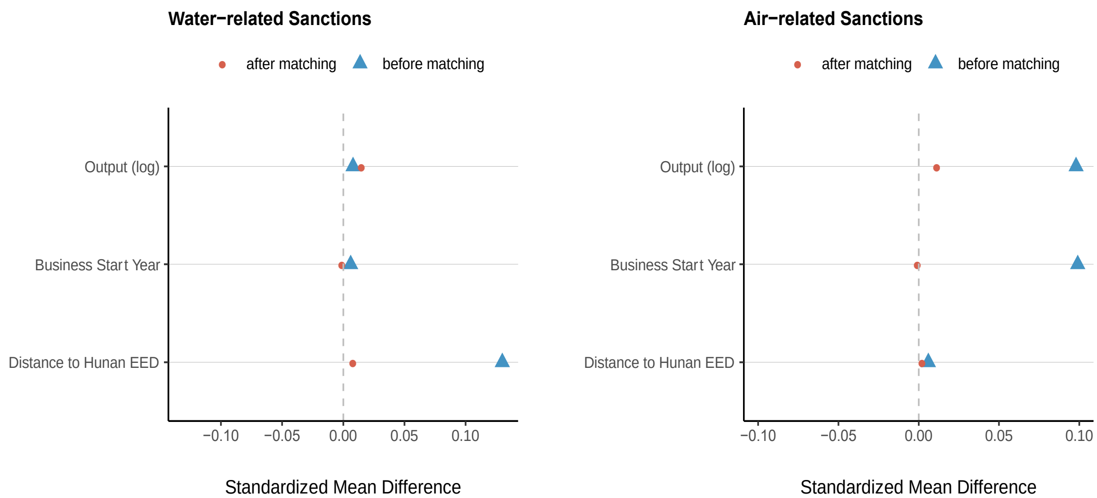
[19] Specifically, we use three cut-offs – 5 million, 10 million, and 20 million – for output, 1990 and 2000 for business start year, and 100km, 200km, and 300km for distance to Hunan EED.

outdated equipment, and firms in emission intensive industries generate more pollution. Firm polluting behaviors may also be influenced by their distances to regulators. Since enforcement is costly, regulators may find it easier to monitor firms closer to them.

For the analysis on the effects of water-related sanctions, CEM creates 98 strata with 193 treatment firms and 3893 control firms. For the analysis on the effects of air-related sanctions, there are 83 strata with 127 treatment firms and 3275 control firms. Since CEM keeps all strata with at least one treated and one control unit, these strata vary in sizes. Following Iacus, King and Porro (2017), we weight the observations according to the size of their strata.[20]

Figure 1 illustrates the standardized mean difference for output (log), business start year, and distance to Hunan EED before and after matching. After matching, the balance between the treatment and control groups significantly improves. Moreover, CEM exactly matches industries, that is, in each stratum, the treatment and control units are from the same industry. Finally, one might still question the existence of systematic difference between sanctioned and unsanctioned firms after CEM. In Table A.3-2 of the appendix, we show the differences between these two groups in terms of environmental performance indicators (i.e., our outcome variables) and total output. As the t-test results suggest, there is no significant difference between these two groups.

FIGURE 1: CEM, Standardized Mean Difference (Industry Exactly Matched)

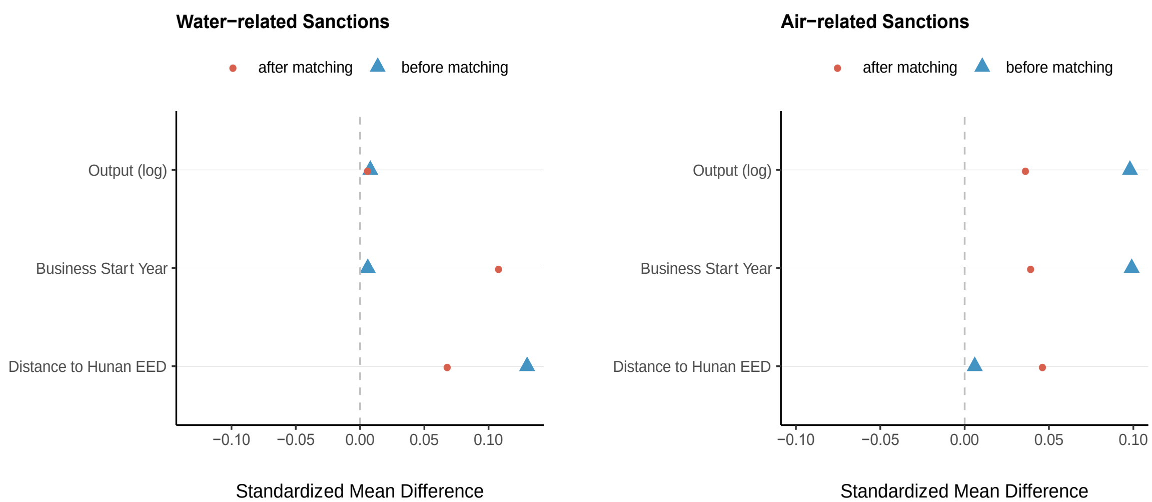


To check the robustness of our results, we also present results using propensity score matching (PSM) in our main regression table (Table 1). Propensity scores are used to "balance" the treatment and control groups on the same set of firm covariates as in the CEM matching, so that firms in the two groups are similar in terms of the probability of being sanctioned as a function of those covariates. The propensity matching yielded 201 pairs of firms (a total of 402 firms) for our analysis on the effects of water-related sanctions, and 130 pairs (a total of 260 firms) for the analysis on the effects of air-related sanctions.

[20] The formula for calculating weights is $weight = \frac{\#\ of\ treatment\ units\ inside\ the\ stratum}{\#\ of\ control\ units\ inside\ the\ stratum} / \frac{\#\ of\ all\ control\ units}{\#\ of\ all\ treatment\ units}$.

Figure 2 illustrates the standardized mean difference for output (log), business start year, and distance to Hunan EED before and after matching using PSM. PSM does not effectively reduce the imbalance between the treated and controlled groups. The balance between the two groups even worsens for the logged output variable and the business starting year variable. A possibility of a greater imbalance after matching is that industry dummy variables may overpower other covariates in determining propensity scores: Figure A.4 of the appendix presents the distribution of industries before and after matching; after matching, the two groups have similar distributions of industries. Indeed, some industries are more likely to be targeted by regulators (van der Kamp 2020). Increased imbalance between the two groups after matching is among the main criticisms against PSM; for example, King and Nielsen (2019) discuss why propensity scores should not be used for matching, pointing out that PSM ignores information from blocking and randomly selects corresponding controls.[21]

Figure 2: PSM: Standardized Mean Difference (Industry Included)



3.3 A Difference-in-Differences (DID) Model

Our analysis employs DID models to estimate the effects of sanctions, involving treated firms and matched control firms. Matching renders the parallel trend assumption underlying DID estimations more plausible. Also, compared to controlling for a batch of variables in DID regressions, forming statistical pairs via matching reduces misspecification errors (Gebel and Voßemer 2014). Stimulation evidence also shows that matched DID have better performance than standard DID (Ryan et al. 2018). The specification is as follows:

$$ln(y_{i,t}) = \beta_0 + \beta_1 D_{i,t} + \beta_2 X_{i,t} + \delta_i + \gamma_t + \epsilon_{i,t} \,, \quad (1)$$

where t indexes years and i indexes firms. The outcome variable of interest $ln(y_{i,t})$ represents the natural logarithm of environmental performance indicators. We include firm fixed effects δ_i

[21] The PSM approach ensures the balance among the matched sets on the scalar propensity score, and therefore, distances between the original covariate values do not necessarily inform the matching decisions. In other words, several units with different values on the original covariates may have the same propensity score. Selecting control units from this pool may or may not improve the covariate balance between the matched units (Ripollone et al. 2018).

to control for time-invariant unobservable firm characteristics. Year fixed effects γ_t is included to control for time trends in emissions. The explanatory variable of primary focus is $D_{i,t}$; it takes the value of 1 in and after the year in which a firm was sanctioned. The time-varying variable $X_{i,t}$ controls for firm output so that we focus on firm environmental performance per output.

4 Main Empirical Results

4.1 DID Effects of Sanctions

The main results of our analysis are presented in Table 1: the first four columns provide estimates from the CEM-DID approach, and the remaining four columns show the results estimated by the PSM-DID approach, which, despite poor balancing performance of PSM, is to demonstrate that our main result is not an artifact of using one particular type of matching method. Indeed, our main result still holds when we use a DID without matching (see the robustness check section). We report in Panel A the effects of water-related sanctions, and in Panel B, the effects of air-related sanctions. Standard errors of all estimates are clustered at the firm level.

Table 1: The Effects of Sanctions on Environmental Performance Indicators

	CEM				**PSM**			
Panel A: Water-related Sanctions								
Model	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
DV	**Spending**	**Wastewater**	**COD**	**NH**	**Spending**	**Wastewater**	**COD**	**NH**
	logged	*logged*	*logged*	*logged*	*logged*	*logged*	*logged*	*logged*
$D_{i,t}$	0.39^{*}	-0.57	-0.28	-0.75^{***}	0.40^{**}	-1.07^{**}	-0.26	-0.47
	(0.21)	(0.47)	(0.18)	(0.26)	(0.17)	(0.51)	(0.20)	(0.34)
N of Treated Firms	125	169	168	128	130	176	175	135
R^2	0.91	0.90	0.96	0.93	0.98	0.85	0.94	0.95
Panel B: Air-related Sanctions								
Model	**(9)**	**(10)**	**(11)**	**(12)**	**(13)**	**(14)**	**(15)**	**(16)**
DV	**Spending**	**Waste Air**	**SO_2**	**NO_x**	**Spending**	**Waste Air**	**SO_2**	**NO_x**
	logged	*logged*	*logged*	*logged*	*logged*	*logged*	*logged*	*logged*
$D_{i,t}$	0.04	-0.15	0.14	0.22	0.41	0.31	0.51^{*}	0.53
	(0.23)	(0.15)	(0.29)	(0.26)	(0.29)	(0.32)	(0.30)	(0.33)
N of Treated Firms	79	111	100	100	81	113	102	102
R^2	0.97	0.99	0.89	0.93	0.96	0.95	0.92	0.94
Firm FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Output (log)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

a. * significant at 10%; ** significant at 5%; *** significant at 1%.
b. Standard errors are clustered at the firm level.
c. $D_{i,t}$ takes the value of 1 in and after the year in which a firm was punished.

We find that firms improved their environmental performance after receiving a water-related sanction, as reported in Panel A. First, firm annual spending on wastewater treatment increased following sanctions. The coefficient estimate is statistically significant for both matching methods. This result indicates that sanctions lead firms to invest more resources in treating wastewater. The coefficient estimate from the CEM-DID model is 0.39; thus, in terms of percentage change, a sanction increases the spending by approximately 47.7%.[22]

Second, wastewater-related emissions, including wastewater, COD, and NH, all decreased following sanctions, indicating that firms reduced emissions of water pollutants after being punished by regulators. Signs associated with these estimates are consistent across matching methods, though these estimates differ in statistical significance for the two matching methods. If we base our analysis on CEM-DID, we find that firms reduced NH emissions by 52.8% following a sanction.

In contrast, for sanctions on air-related violations, the results suggest that firms did not change their polluting behaviors after being sanctioned, as suggested in Panel B of Table 1. None of the estimates are statistically significant, and the sign of the coefficients regarding air pollutant emissions is positive in most cases. The discrepancy between these two pollution sources may have to do with the salience of the water pollution issue on Hunan's public and political agenda. We provide more discussion in Section 6 of this paper.

4.2 Dynamic Effects of Sanctions

After establishing our results on the average effects of sanctions during the post-sanction years, we turn to examination of the dynamic impacts of sanctions in each year following sanctions. More specifically, we interact the sanction treatment variable with year dummies that indicate, for a treated firm included in the analysis, whether this is the year being sanctioned (t), or one year ($\mathrm{t}+1$), two year ($t+2$), three year ($\mathrm{t}+3$), four year ($\mathrm{t}+4$) after the treatment. This test informs whether the treatment effect fades out, stays constant, or even increases over time. We also interact the sanction treatment variable with year dummies that indicate whether this year is two year (t-2), three year ($t-3$), or four year ($t-4$) before the sanction, to test the parallel assumption that treatment firms and control firms trended in parallel prior to treatment. The year before the treatment year (t-1) is used as the base year. Control variables include firm fixed effects, year fixed effects, and firm's annual total output. The regression model takes the following form:

$$ln(y_{i,t}) = \beta_0 + \sum_{j=t-4}^{t-2} \alpha_j Pre_j + \sum_{j=t}^{t+4} \theta_j Post_j + \beta_2 X_{i,t} + \delta_i + \gamma_t + \epsilon_{i,t}. \qquad (2)$$

Figure 3, focusing on water-related environmental sanctions, maps out the time path of the differences between the treatment and control firms, both prior to and post treatment, relative to the base year (year t-1). We report the results of our models based on Equation 2 and the CEM matching, for four wastewater-related outcome variables, and plots the coefficient estimates of three indicator variables for years before sanction (t-4, t-3, and t-2) and five variables for years in which a sanction was made and afterward (t, $\mathrm{t}+1$, $\mathrm{t}+2$, $\mathrm{t}+3$, $\mathrm{t}+4$). The figure also contains 95% confidence intervals of these estimates. The base year t-1 is marked as 0 with no confidence interval.

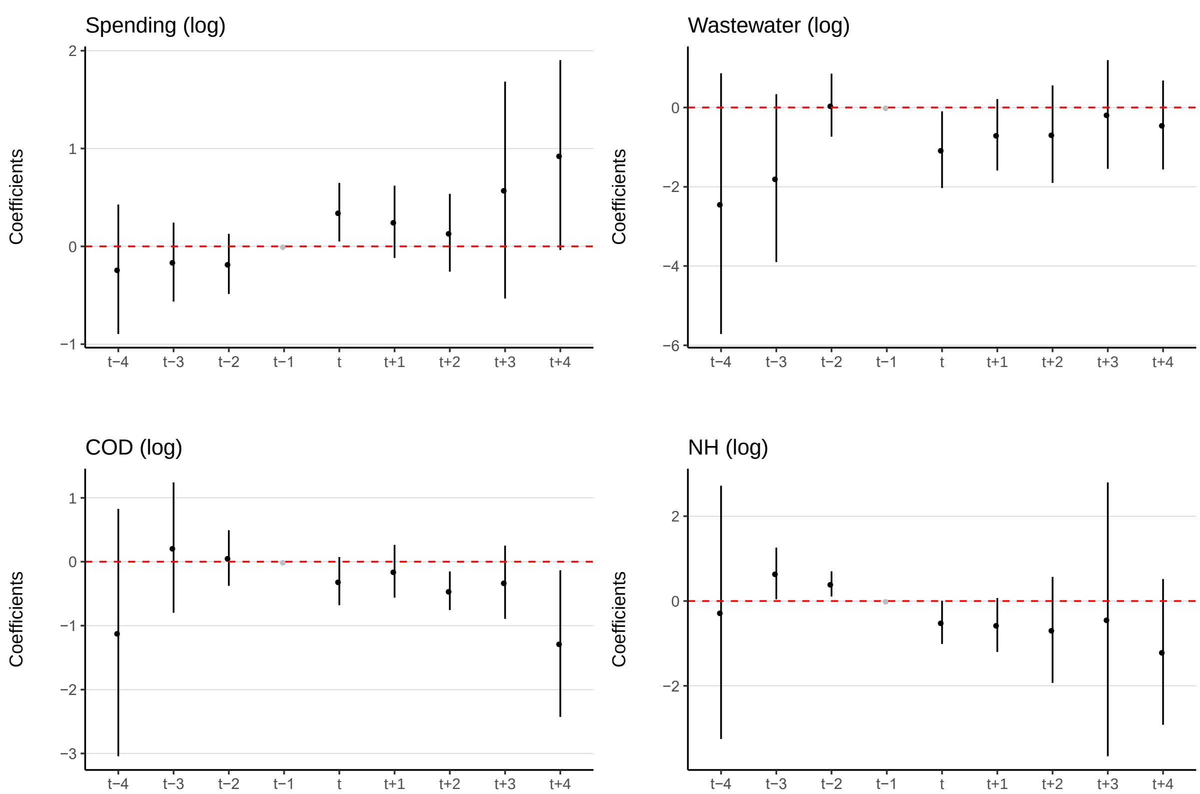
As exhibited in Figure 3, prior to sanctions, there was no significant difference between the treatment and control firms in firm spending on wastewater treatment, total wastewater emissions, and COD emissions, supporting the parallel trend assumption. In the case of NH emissions, firms in the treatment group had higher level of NH emissions than those in the

[22] Since our dependent variable is log-transformed, we calculate the exponential value of 0.39, which is 1.477, to get the following ratio $y_{i,t}|D_{i,t} = 1/\ y_{i,t}|D_{i,t} = 0$.

control group at years t-2 and t-3. This indicates that the parallel trend assumption does not hold for these two pre-treatment years; nevertheless, such higher level of emissions changed after treated firms receive the treatment and the lags at t and $t + 1$ are both negative and statistically significant. This renders our finding that firms in the treatment group reduced NH emissions relative to control firms to be more interesting and reflect the impacts of the treatment.

More importantly, firms in the treatment group, following sanctions, increased wastewater treatment spending and reduced all the three types of emissions, relative to control firms. The effect of sanctions has been largely persistent: throughout the post-treatment years, the difference between treated and control firms has been positive for wastewater treatment spending and negative for the three types of emissions, though the magnitude and significance levels vary. The loss of significance in later years is likely due to the smaller sample size because firms that had sanctions in the first or second year of the study period, so that their performance in years t+3 or t+4 are included, are small in numbers. These results suggest that sanctions likely entailed some lasting impacts.

Figure 3: Time Path of the Difference between Treatment and Control Firms using CEM.



a. Black solid dots reflect estimated coefficients on indicator variables from Equation 2.
b. Solid lines reflect 95 percent confidence intervals.
c. Grey solid dot reflects the base year, t-1.
d. The results are based on the CEM approach.

The results that firms in the treatment group were largely comparable to matched control firms in spending and emissions during years prior to sanctions, but they significantly increased water treatment spending and reduced the three types of emissions post sanctions, suggest a causal linkage between sanctions and firm environmental performance. This further address the endogeneity concern we discussed earlier. Moreover, if a firm, due to some random shocks, had higher than usual emissions and thus greater probability of receiving sanctions in a given year, it is likely that the firm's emission, even without sanctions, will have a tendency to reverse to its normal level, i.e. experiencing reduction in emissions in the following years or what we often refer to as a "reversion to the mean." However, the findings that sanctioned firms significantly increased spending and reduced emissions *in the year of sanction* (year *t*), suggest that the issue of "reversion to the mean" is not substantiated. In summary, we are confident in making the inference that firms increased spending on wastewater treatment and reduced water-related emissions after they were sanctioned.

5. Robustness Checks

In this section, we conduct multiple robustness checks. We run DID without matching and DID with matching based on alternative samples of the data. We experiment with different CEM setups. We conduct a placebo test to see whether water related sanctions affect firm air related performances and vice versa. We test whether the effect of sanctions depends on industry pollution intensity, firm size, and repeated sanctions in one year.

5.1 Alternative Samples

First, we conduct a standard DID analysis using an unmatched full sample, as recent simulation studies show that matching on time-varying covariates or past outcomes may increase estimation bias when performing DID estimations (Daw and Hatfield 2018; O'Neill et al. 2016). Table B.1 in the appendix presents the result. It shows that the effects of sanctions on firms' environmental performances are robust to different DID approaches. Figure B4 shows the dynamic treatment effects of sanctions based on a DID using unmatched sample: these are similar to what we see in Figure 3.

Moreover, as we discussed earlier, we exclude firms being sanctioned in multiple years from our sample in the main analysis. There were three firms sanctioned in two different years for water-related violations; seven firms were sanctioned in two different years due to air-related violations. Here, we add these ten firms back to our analysis. For these firms, the indicator variable $D_{i,j}$ takes the value of 1 since the first year they received a sanction. The results in Table B.3 are similar to those presented in Table 1.

Finally, some firms were sanctioned for both air- and water-related violations in the same year. Among sanctioned firms, 41% were sanctioned due to both water- and air-related violations, 45% were sanctioned only for water-related violations, and 14% only for air-related violations. In this robustness check, we include firms with wastewater-only or waste air-only sanctions, removing those with both water- and air-related sanctions in a year.[23] We present the

[23] There are also procedural violations that may cause both air and water pollution. A typical noncompliance of this kind is the violation of the "three simultaneities principle" (三同时原则). This principle requires that "pollution control equipment design, construction, and operation are undertaken at the same time as design, construction, and operation of the associated facility or

results in Table B.4 and the findings in the main analysis are robust to restricting our analysis to this more specific (and much smaller) sample.

5.2 Alternative CEM Matching

In the main analysis, we match on one time-varying covariate, a firm's production level, and three time-invariant covariates: the year in which business started, distance to the EED of Hunan province, and industry dummies. For the production level, we calculate its mean value during the study period, which might cause concerns, as sanctions may affect firm production level through for example production suspension.

To address this concern, first, we regress firm annual total output on the sanction indicator variable $D_{i,j}$, following a similar CEM based DID approach. Results in Table E.1 show that sanctions did not have a significant effect on firm total output. This result alleviates the concern regarding the use of the average firm output during the study period as one of the matched variables. Second, we conduct CEM based on firm output in the first year of our research period (year 2011) along with other two time-invariant variables and industry dummies, therefore dropping the year of 2011 from analysis. The results are similar to those in Table 1: see Table C.1 in appendix.

Finally, Table C.2 shows the results from an analysis in which we also include city-level GDP as one of the matched variables in the CEM. Local GDP may be related to firm environmental enforcement in a variety of ways. For example, the public or the government may value environmental quality more in economically more prosperous cities; they are more likely to keep firms under supervision. Conversely, manufacturing firms, which may be the primary driver of the local economy, could exert influence on environmental regulations and enforcement. We include city-level GDP in CEM. The results in Table C.2 are similar to our main results shown in Table 1.

5.3 Placebo Tests

We conduct placebo tests by testing the effects of wastewater-specific sanctions on air-related environmental performance and the effects of waste air-specific sanctions on water-related performance. Table D.1 shows that firms' environmental performance indicators are not affected by sanctions targeting the other type of violations. This finding, that is, sanction effects do not exist when they are not supposed to, provides additional supportive evidence that there exist causal effects of water-related sanctions on water performance and that there is no effect of air-related sanctions.

5.4 Heterogeneous Treatment Effects

In this section, we test potential heterogeneous treatment effects. We separate sanctioned firms into subgroups based on the industry they are in and their firm size. To test whether there is a difference between clean and dirty industries, first, in Table F.1, we create a dummy variable of dirty industries, I_i, according to an official categorization published by the Ministry of Ecology

---

construction project" (Ferris and Zhang 2005, p.78). Firms may move forward with proposed projects or construction without filing environmental impact assessment or receiving mandatory certificates. These violations have general environmental implications. In this robustness check, we also remove these procedural violations from the sample.

and Environment in 2017.[24] Second, in Table F.2, we adopt an I_i that is a continuous variable indicating industrial level emission intensities in China, using nationally aggregated data on total outputs and wastewater/waste air emissions for each of the two-digit industries in 2010.[25] Both Table F.1 and F.2 show that there is no heterogeneous treatment effect as a function of clean vs. dirty industry and industry level pollution intensity.

In Table F.3, S_i is a dummy variable: it is 1 if a firm's output is above the median output level within our sample; 0 otherwise. Table F.3 shows that there is no evidence for a heterogeneous treatment effect of sanctions as a function of firm size.

5.5 Adding City-year Fixed Effect

To rule out a potential bias generated by within-city policy changes over time, we replace year fixed effects in equations (1) and (2) with city-year fixed effects. Table G.1 reports the results from a CEM -based DID analysis, and Figure G.2 plots the dynamic treatment effects. The results are largely robust, though less statistically significant. A closer look at Figure G.2 suggests that a sanction increases pollution abatement spending and reduces wastewater emissions following a sanction in year *t*.

5.6 Repeated Sanctions in One Year

In the main analysis, we construct a DID variable ($D_{i,t}$) indicating years during and after which a firm received at least one sanction. We do not differentiate between a single sanction during a year and multiple sanctions received during the same year. As we show in Table I.1 of the appendix, among the 201 firms with water-related sanctions and the 130 firms with air-related sanctions, about a third received repeated sanctions within a year. Do repeated sanctions in a year affect firms differently than single sanctions in a year? We include an interaction term between the DID indicator ($D_{i,j}$) and a dummy variable indicating repeated sanctions for a firm. The results are in Table I.2 of the appendix: the lack of statistical significance of this interaction term in all model specifications suggest that there is no additional effect associated with repeated sanctions.

6. Why Firms Only Respond to Water-related Sanctions?

In our main analysis as well as in robustness checks, we consistently find that firms only respond to water-related sanctions. We do not observe an increase in air related environmental spending and a decrease in air pollutant emissions, after a firm received an air-related sanction. Why are there such different firm responses between water and air-related sanctions?

6.1 Visibility and Traceability

Some might consider this result counter-intuitive, because compared to water pollution, air pollution is often more visible and tends to afflict urban areas (Cao and Prakash 2012). Thus, if

---

[24] The categorization in Chinese can be retrieved at: https://www.mee.gov.cn/gkml/hbb/bgt/201712/W020171201468746002498.pdf.

[25] Nationally aggregated data on total outputs and wastewater/waste air emissions is published by the National Bureau of Statistics of China (https://www.epsnet.com.cn/index.html#/Home). The aggregated data is based on annual surveys of all industrial firms above the designated size (which was 5 million yuan or more in annual main business revenue before 2011 and 20 million yuan or more after 2011). These firms account for over 90% of the industrial GDP.

visibility affects firm responses, it would be more likely that air-related sanctions affect firms' behaviors more. However, the difference in visibility often is from the perspective of the public, not local regulators. Unlike the public, regulators have access to professional monitoring equipment and data. Traceability – how well one can trace the source of pollution back to a particular firm – matters more for local regulators. As far as we know, there is no consensus on whether water pollution is more traceable than air pollution or vice versa. There are many mediating factors, e.g., the traceability of water pollution often depends on how stationary the water is.

To test whether traceability explains our finding, we conduct an indirect test by differentiating between nationally key monitored firms from rest of the firms in our sample. The assumption here, which we think is reasonable, is that all else equal, pollutants from nationally key monitored firms should be more traceable because the central government has required the installation of more monitoring devices in these firms. Therefore, if traceability matters, we should expect more response from a nationally key monitored firm. We estimate the following model:

$$ln(y_{i,t}) = \beta_0 + \beta_1 D_{i,t} + \beta_2 D_{i,t} \times State\ key_{i,t} + \beta_3 X_{i,t} + \delta_i + \gamma_t + \epsilon_{i,t} \qquad (3)$$

$State\ key_{i,t}$ is a binary indicator for nationally key monitored firms. The results are in Table 2: there is no additional effect associated with nationally key monitored firms, suggesting that traceability is not a reason why firms only respond to water-related sanctions.

Table 2: State Key-Monitoring Firms

Panel A: Water-Related Sanctions				
Model	**(1)**	**(2)**	**(3)**	**(4)**
DV	**Spending**	**Wastewater**	**COD**	**NH**
	logged	*logged*	*logged*	*logged*
$D_{i,j}$	0.32^{*}	-0.62	-0.24	-0.78^{***}
	(0.19)	(0.49)	(0.19)	(0.27)
$D_{i,j} \times State\ Key_{i,j}$	1.06	1.00	-0.70	0.31
	(1.09)	(1.19)	(0.48)	(0.53)
N of Treated Firms	125	169	168	128
R^2	0.91	0.90	0.96	0.93
Panel B: Air-Related Sanctions				
Model	**(1)**	**(2)**	**(3)**	**(4)**
DV	**Spending**	**Wastewater**	**COD**	**NH**
	logged	*logged*	*logged*	*logged*
$D_{i,j}$	0.04	-0.14	0.14	0.22
	(0.23)	(0.15)	(0.29)	(0.27)
$D_{i,j} \times State\ Key_{i,j}$	-0.12	-0.09	-0.19	-0.18
	(0.24)	(0.17)	(0.29)	(0.32)
N of Treated Firms	79	111	100	100
R^2	0.97	0.99	0.89	0.93
Firm FEs	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes
Output (log)	Yes	Yes	Yes	Yes

a. * significant at 10%; ** significant at 5%; *** significant at 1%.
b. Standard errors are clustered at the firm level.
c. $D_{i,j}$ takes the value of 1 in and after the year in which a firm was punished.
d. The results are based on the CEM approach.

6.2. Provincial Policy Priorities

After ruling out pollutant visibility and traceability, our favorite explanation is that the discrepancy in firm responses to water-based sanctions vs. air-based sanctions may have to do with the priority of water pollution reduction on Hunan's political agenda. Compared to air pollution, water pollution is much more severe in Hunan. The provincial government prioritizes water pollution reduction. For instance, in 2013, the province defined the protection and remediation of the Xiang River as its No. 1 Key Project (一号重点工程).

First, Hunan locates along the central to southern reaches of the Yangtze River. It is hard to exaggerate the importance of rivers and lakes for the province. Based on the 2018 data from the Hunan Statistical Yearbook, there are 5,341 rivers in Hunan. The total of all watershed areas in the province is 178,692 square kilometers, 84.37% of the total provincial surface area. At the same time, Hunan is home to an abundance of mineral resources and heavy manufacturing. Intensive mining activities have contaminated local soils and watersheds. The consequences are severe. An analysis of soil and brown rice samples suggests that mining activities led the mean concentrations of cadmium and mercury to exceed the national standard for food safety (Zeng et al. 2015). A local NGO focusing on water quality, Green Hunan, has organized campaigns and protests to foster public awareness and demanded government intervention in projects with high potential for water pollution (Du 2016).

Compared with severe water pollution, Hunan is far from being a hotspot of air pollution in the country: see Figure J.2 of the appendix for 2005 SO2 data from NASA satellite. This is probably the reason why in the 11th Five-year Plan (2006-2010), the SO2 reduction target imposed by the central government for Hunan is relatively low: a 9% reduction target for Hunan is much lower than those for Beijing, Shanghai, and Hebei (all > 15%, see Table J.4 in appendix). In contract to a relative low target to reduce air pollution for Hunan, water pollution reduction has always been a priority for the central government and the provincial government. Table J.5 of the online appendix shows that out of the 10 targets established by the provincial government for the 12th Five-year Plan (2011-2015), 6 are specifically about addressing water pollution, 3 for air pollution, and 1 for industrial solid waste.

Another source of evidence that reflects a much more prioritized water pollution reduction is the annual provincial government work report. This is the most important document for a provincial government to communicate its policy priorities to the public. We have downloaded the environmental protection section of the provincial government work reports, 2011-2015. Our online appendix J has a detailed analysis of these texts. Here we only highlight a few findings: in the 2011 and 2012 reports, there is no mentioning of air pollution at all; there is only one sentence on air pollution reduction in the 2013 report. At the same time, large sections of the annual provincial government work reports are devoted to water pollution issues.

Such governmental priority on water pollution in Hunan could be due to public attention. We measure public attention to pollution issues by using Baidu Search Volume Index (SVI, similar to Google SVI). We use the provincial-level monthly averages of SVI to measure public attention to air vs. water pollution in Hunan. Figure I.6, I.7, and I.8 of the appendix shows that the public in Hunan pay more attention to water pollution as reflected in Baidu SVI.[26]

[26] Online appendix J provides a more detailed discussion on the three points covered in this section: 1) why water pollution is such an important issue for Hunan; 2) the central government's policy emphasis on water pollution for Hunan and evidence from Hunan provincial government's annual reports; 3) the public in Hunan pay more attention to water pollution as reflected in Baidu search volume index (SVI).

7. Exploring Mechanisms

In this section, we explore the mechanisms through which sanctions come to affect firms' water-related environmental performance. We evaluate two potential mechanisms: media disclosure and sanction severity.

7.1 Media Disclosure

In accordance with OGI regulations, local regulators have disclosed sanctions to the public through different channels. Since IPE records the source from which it collected sanction information, we are able to examine whether firms respond differently to specific disclosure sources. Publicizing violations and shaming violators have become a trend among regulatory agencies across countries. Scholars have just started to define the concept of regulatory shaming (Yadin 2019) and evaluate the effectiveness of this regulatory mechanism. In the US context, Johnson (2020) estimates that a single press release could achieve the same improvement in compliance as 210 inspections regarding workplace safety and health.[27]

Is publicizing sanctions and violations effective in improving environmental performance? To answer this question, we separate sanctions in the data based on the channels of disclosure, differentiating between routine disclosure on local environmental agencies' websites and disclosure via public news and media. Sanctions disclosed on environmental agencies' websites often take the forms of official enforcement orders and documents with sanction information embedded, for which one needs to go to the website and devote time and effort to read through the text and data to extract sanction information. On the contrary, by providing direct and succinct information to a general audience, dissemination through public media name-and-shames the violators in public.

To test whether media disclosure conditions the effect of sanctions, we estimate models of the following basic form:

$$ln(y_{i,t}) = \beta_0 + \beta_1 D_{i,t} + \beta_2 D_{i,t} \times M_i + \beta_3 X_{i,t} + \delta_i + \gamma_t + \epsilon_{i,t}\ , \qquad (4)$$

where M_i is a binary indicator, taking the value of 1 if the sanction information was publicized in newspapers (or on news websites) or official press releases; it is 0 for cases where information about sanctions was made available only by routine disclosure on environmental regulatory agencies' websites. The results are reported in Table 3. We do not find media disclosure to have any additional effect on firms' environmental performance. Thus, firms responded to water-related sanctions largely because of sanctions themselves, not due to public media disclosure. The result coincides with Seligsohn, Liu, and Zhang (2018) that publicizing pollution information has no measurable effect on environmental outcomes in Chines prefecture cities.[28]

---

[27] Recent studies show that compliance with social norms plays an important role in securing contracts in the global supply chain due to consumer and activist pressure (Distelhorst and Locke 2018). Thus, the additional reputational costs brought by public media disclosure might incentivize firms to improve their environmental performance.

[28] Though their focus is on whether increasing city level government information disclosure improves city environmental outcomes.

Table 3: Testing the Mechanism of Media Disclosure

Model	(1)	(2)	(3)	(4)
DV	Spending	Wastewater	COD	NH
	logged	*logged*	*logged*	*logged*
$D_{i,j}$	0.39^{*}	-0.48	-0.28	-0.81^{***}
	(0.21)	(0.48)	(0.18)	(0.26)
$D_{i,j} \times M_i$	-0.07	-1.84	0.02	1.08
	(0.72)	(2.33)	(0.20)	(1.33)
N of Treated Firms	125	169	168	128
R^2	0.91	0.90	0.96	0.93
Firm FEs	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes
Output (log)	Yes	Yes	Yes	Yes

a. * significant at 10%; ** significant at 5%; *** significant at 1%.
b. Standard errors are clustered at the firm level.
c. $D_{i,j}$ takes the value of 1 in and after the year in which a firm was punished.
d. The results are based on the CEM approach.

7.2 Sanction Severity

Next, we examine whether firm response is sensitive to the level of sanction severity. Our empirical approach is to separate sanctions based on severity. We sort sanctions into three types: correction of violation, fines, and suspension of production.[29] With violation correction, regulators require that violators correct their behaviors without imposing additional costs. Fines are direct penalties imposed on violators. In our data, the mean value of fines is ¥ 53,859 (with a minimum of ¥ 220 and a maximum of ¥ 300,000), which was very small compared to the mean output value of ¥27.51 million for our sample firms. Suspension of production is the severest of these sanctions, often imposing significant direct economic losses on firms. We therefore treat suspension of production as severe sanctions and correction and fines as light sanctions.

Table 4: Testing the Mechanism of Sanction Severity

Model	(1)	(2)	(3)	(4)
DV	Spending	Wastewater	COD	NH
	logged	*logged*	*logged*	*logged*
$D_{i,j}$	0.54	-0.42	-0.25	-0.77^{**}
	(0.33)	(0.56)	(0.22)	(0.39)
$D_{i,j} \times S_i$	0.08	0.07	0.06	0.13
	(0.46)	(1.02)	(0.44)	(0.53)
N of Treated Firms	96	133	132	99
R^2	0.90	0.90	0.96	0.93
Firm FEs	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes
Output (log)	Yes	Yes	Yes	Yes

a. * significant at 10%; ** significant at 5%; *** significant at 1%.
b. Standard errors are clustered at the firm level.
c. $D_{i,j}$ takes the value of 1 in and after the year in which a firm was punished.
d. The results are based on the CEM approach.

In Table 4, we report the regression results of models specified by a modified version of Equation (4), in which we substitute S_i for M_i. Like M_i, S_i is binary variable indicating severe sanctions (suspension of production). Severe penalties do not entail significantly additional

[29] Sanctions that do not fit in one the three types here are excluded from the analysis in Table 4.

effects: the estimated coefficients for the interaction terms are relatively small in magnitude and none is statistically significant. This again seems to suggest that it was mostly that mere action of sanctioning that serves warning purposes to violators that changed firm behaviors.

In the above analysis, we group together violation correction and fines and define them as light punishment. As a robustness check, in Appendix H, we estimate the effects of correction, fines, and suspension of production separately using three alternative coding rules. The results, as shown in Table H.1, H.2, and H.3, are similar to what we find in Table 4, indicating that penalties incurring high costs on violators were no different from those incurring low costs.

8. Testing Spillover Effects

Finally, we explore potential spillover effects of sanctions. Shimshack and Ward (2005) argue that only looking at sanctioned facilities' responses may seriously underestimate the efficacy of sanctions because an unsanctioned firm might also respond after observing similar firms were sanctioned. Here, we estimate both cross-region and cross-industry spillover effects.

We first categorize firms into three groups: (A) sanctioned firms, (B) in-county/in-industry sanctioned firms,[30] and (C) out-of-county/out-of-industry unsanctioned firms.[31] Like Shimshack and Ward (2005), we assume spillover is more likely to happen between a sanctioned firm and firms in same jurisdiction (county in our case) or in the same industry, because they are close geographically and similar in emission characteristics. Under this assumption, if spillover occurs, we should observe significant differences between in-county (or in-industry) unsanctioned firms and out-of-county (or out-of-industry) unsanctioned firms: the former increase more environmental spending and decrease more emissions than the latter. In this analysis, we consider group B (instead of sanctioned firms in group A) as the treatment group, and firms in group C as the control group to which we apply the same CEM approach as in the main analysis.

Table 5: Testing Spillover Effects

	In-County Unsanctioned vs. Out-of-County Unsanctioned				**In-Industry Unsanctioned vs. Out-of-Industry Unsanctioned**			
Model	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
DV	**Spending**	**Wastewater**	**COD**	**NH**	**Spending**	**Wastewater**	**COD**	**NH**
	logged	*logged*	*Logged*	*logged*	*logged*	*logged*	*logged*	*logged*
$D_{i,j}$	0.03	0.25	−0.04	−0.10	−0.09	−0.17	-0.31^{***}	0.10
	(0.05)	(0.16)	(0.07)	(0.10)	(0.07)	(0.26)	(0.08)	(0.11)
N of Treated Firms	2101	2758	2727	2192	1394	1876	1841	1414
R^2	0.96	0.89	0.94	0.96	0.95	0.85	0.93	0.94
Firm FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Output (log)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

a. * significant at 10%; ** significant at 5%; *** significant at 1%.
b. Standard errors are clustered at the firm level.
c. $D_{i,j}$ takes the value of 1 in and after the year in which a firm was punished.
d. The results are based on the CEM approach.

In Table 5, Columns (1) - (4) report the within-county spillover effects and Columns (5) – (8) the within -industry spillover effects. If spillovers exist, within-county or within-industry

[30] In-county/in-industry sanctioned firms are unsanctioned firms within the same county/industry of sanctioned firms each year.
[31] Out-of-county/out-of-industry unsanctioned firms are unsanctioned firms outside counties/industries with sanctioned firms each year.

unpunished firms should have greater response to punitive actions than other unsanctioned firms. However, we do not find strong spillover effects within the Hunan sample. The estimated coefficient of $D_{i,j}$ is only associated with COD in Column (7), suggesting that spillovers only exist between a sanctioned firm and unsanctioned firms in the same industry in the case of COD emissions.

9. Conclusion and Discussion

This paper evaluates the effectiveness of regulatory sanctions in changing non-compliant firms' environmental spending and emissions. We construct a unique firm-level panel data set of 6,529 major polluting firms of the Hunan province, China, for the period of 2011-2015. We combine CEM matching with a difference-in-differences (DID) design. We find that sanctions concerning water pollution increased firms' operating expenses in wastewater treatment devices and decreased water pollution. As for air pollution, we did not find any significant impact. This difference might be explained by Hunan's greater susceptibility to water pollution and its provincial government's emphasis on improving water quality.

This key empirical finding – effective water related sanctions and ineffective air related sanctions – is consistent across many robustness checks. It seems that the effectiveness of a regulatory sanction is not unconditional: in the case of Hunan, it depends on government policy priorities. In other words, firms pay attention to government regulatory priorities – improving water quality in the case of Hunan – and choose to respond accordingly. This is also consistent with what we find when we further explore whether a sanctioned firm's response is a function of sanction severity. Here, no such conditional effect is found, suggesting that immediate financial loss is probably not the major reason for a firm to increase environmental spending and reduce pollution.

We also test potential effects associated with media exposure of sanctions, a naming and shaming mechanism that exposes non-compliant firms to the public who care about pollution. We differentiate sanctions publicized by local media from those that were not. But we do not find a stronger effect associated with publicized sanctions, suggesting that there is no effective naming and shaming. Recently, Seligsohn, Liu, and Zhang (2018) also find that city government information disclosure has no measurable effect on environmental outcomes in Chinese cities. They argue that the experiments that have shown information to affect government performance have all involved mechanisms for citizens to influence their government, suggesting the lack of citizen input and influence on local governance as one potential explanation for the lack of media exposure effect. In other words, sanctioned firms might be named and shamed by the public, but popular grievances cannot translate into government actions. The public, as consumers, can still penalize sanctioned firms by boycotting their products. This California Effect is more likely to work for finished goods because consumers can link them to sanctioned firms. It might not work for intermediate goods such as wood, steel, raw sugar, because they, as inputs in the production of other goods, are not something consumers can choose directly. One interesting future research question therefore is whether the effect of media exposure is a function of the type of goods produced by a sanctioned firm.

We have also explored potential spillover effects of sanctions, either geographically (within the same county) or industry wise (within the same industry). Our results suggest no strong spillover effect within the Hunan sample. It would be interesting to further explore why there is no spillover. One potential explanation might be firms' very low overall chance of ever receiving a sanction. About 204 firms received a water-related sanction among a total of 6,529 firms over five years.[32] This translates into about a 0.6% yearly chance of receiving a sanction during 2011 to 2015. The chance of receiving an air-related sanction in a year is even lower: a total of 137 firms among 6,529 firms over five years translates into about a 0.4% chance.[33] The fact that sanctions are such rare events might make unsanctioned more opportunistic. Another explanation, which might further enhance the effect of an overall low chance of sanction, might be the lack of information dissemination or communication from the provincial EED so that even firms within the same county or the same industry were unaware of sanctions received by their peers.

Finally, future research should focus on a few issues that, despite our best efforts, still have not been fully addressed. For instance, some past studies have shown firm ownership to affect government enforcement of regulations (Wang et al. 2003; Wang and Wheeler 2005; Chen et al. 2014). We do not have a firm-level ownership variable to control for this potentially endogenous aspect of sanctions. However, we use a short panel of 5 years during which significant changes in firm ownership are unlikely to be prevalent, which makes it likely that firm ownership is a slow-moving or even time-invariant variable in our sample. This and the fact that we use firm fixed effects in all our model specifications can at least partially address the endogeneity concern of government sanctioning as a function of firm ownership. Furthermore, our study is based on only one province of China. To increase the external validity of our findings, future studies should investigate data in other provinces (and other countries) to test the effect of regulatory sanctions. While more research is certainly needed to better understand the effects of environmental regulatory instruments in a developing country context, we hope this paper has contributed to this new and exciting area of future research.

[32] Only three firms received water-related sanctions in more than one year.
[33] Only seven firms received air-related sanctions in more than one year.

References:

Almer, Christian and Goeschl, Timo. 2010. Environmental Crime and Punishment: Empirical Evidence from the German Penal Code. Land Economics 86(4): 707-726.

Becker, G. S. (1968). Crime and Punishment: An Economic Approach. *Journal of Political Economy*, *76*(2), 169–217.

Blackman, Allen and Li, Zhengyan and Liu, Antung A. 2018. Efficacy of Command-and-Control and Market-Based Environmental Regulation in Developing Countries. Annual Review of Resource Economics 10(1): 381-404.

Burgess, Robin, Matthew Hansen, Benjamin A. Olken, Peter Potapov, and Stefanie Sieber. 2012. "The Political Economy of Deforestation in the Tropics." Quarterly Journal of Economics 127 (4): 1707–54.

Cai, Xiqian, Yi Lu, Mingqin Wu, and Linhui Yu. 2016. Does environmental regulation drive away inbound foreign direct investment? Evidence from a quasi-natural experiment in China. Journal of Development Economics 123: 73–85.

Cao, Xun, and Aseem Prakash. 2012. Trade competition and environmental regulations: domestic political constraints and issue visibility. The Journal of Politics 74(1): 66-82.

Chen, Qiu, Min Maung, Yulin Shi, and Craig Wilson. 2014. Foreign direct investment concessions and environmental levies in China. International Review of Financial Analysis 36: 241 – 250.

Chen, Zhao, Matthew E. Kahn, Yu Liu, and Zhi Wang. 2018. The consequences of spatially differentiated water pollution regulation in China. Journal of Environmental Economics and Management 88: 468-485.

Dasgupta, Susmita, Benoit Laplante, Nlandu Mamingib, and Hua Wanga. 2001a. Inspections, pollution prices, and environmental performance: Evidence from China. Ecological Economics 36:487-98.

Daw, J. R., & Hatfield, L. A. (2018). Matching and Regression to the Mean in Difference-in-Differences Analysis. Health Services Research, 53(6), 4138–4156.

Distelhorst, G., & Locke, R. M. (2018). Does Compliance Pay? Social Standards and Firm-Level Trade. *American Journal of Political Science*, *62*(3), 695–711.

Doonan, Julie, Paul Lanoie, and Benoit Laplante. 2005. Determinants of environmental performance in the Canadian pulp and paper industry: An assessment from inside the industry. Ecological Economics 55(1): 73 – 84.

Du, J. (2016) With Network of River Watchers, Green Hunan Opens Second Front in China's War on Pollution. https://www.newsecuritybeat.org/2016/12/network-river-watchers-green-hunan-opens-front-chinas-war-pollution/, Accessed 30 May 2020.

Du, Y., & Takeuchi, K. (2019). Can climate mitigation help the poor? Measuring impacts of the CDM in rural China. Journal of Environmental Economics and Management, 95, 178–197.

Duflo, Esther, Michael Greenstone, Rohini Pande, and Nicholas Ryan. 2013. "Truth-Telling By Third-Party Auditors and the Response of Polluting Firms: Experimental Evidence from India." Quarterly Journal of Economics 128 (4): 1499–1545.

Economy, Elizabeth. 2004. The River Runs Black: The Environmental Challenge to China's Future. Cornell University Press.

Escobar, Ninel and Carlos Chávez. 2013. Monitoring, firms' compliance and imposition of fines: evidence from the Federal Industrial Inspection Program in Mexico City. Environment and Development Economics 18(6): 723–748.

Ferris, R. J., Jr, & Zhang, H. (2005). Environmental Law in the People's Republic of China: An Overview Describing Challenges and Providing Insights for Good Governance. In K. A. Day (Ed.), *China's Environment and the Challenge of Sustainable Development* (1st ed., pp. 66–101).

Gebel, M., & Voßemer, J. (2014). The Impact of Employment Transitions on Health in Germany. A Difference-in-Differences Propensity Score Matching Approach. Social Science & Medicine, 108(C), 128–136.

Girma, S., & Görg, H. (2007). Evaluating the Foreign Ownership Wage Premium Using a Difference-in-Differences Matching Approach. Journal of International Economics, 72(1), 97–112.

Gray, Wayne and Jay Shimshack. 2011. The Effectiveness of Environmental Monitoring and Enforcement: A Review of the Empirical Evidence. Review of Environmental Economics and Policy 5(1): 3-24.

Greenstone, Michael and Rema Hanna. 2014. "Environmental Regulations, Air and Water Pollution, and Infant Mortality in India." *American Economic Review*, 104(10): 3038-3072.

Hanna, Rema and Paulina Oliva. 2015. The effect of pollution on labor supply: Evidence from a natural experiment in Mexico City. Journal of Public Economics 122: 68-79.

Iacus, S. M., King, G., & Porro, G. (2017). Causal Inference without Balance Checking: Coarsened Exact Matching. Political Analysis, 20(1), 1–24.

Jiang, Tingsong and Warwick J. Mckibbin. 2002. Assessment of China's pollution levy system: an equilibrium pollution approach. Environment and Development Economics 7: 75–105.

Jin, Yanhong and Liguo Lin. 2014. China's provincial industrial pollution: The role of technical efficiency, pollution levy and pollution quantity control. Environment and Development Economics 19(1): 111-132.

Johnson, Matthew S. 2020. Regulation by Shaming: Deterrence Effects of Publicizing Violations of Workplace Safety and Health Laws. *American Economic Review*, 110 (6): 1866-1904.

Kahn, Matthew E., Pei Li, and Daxuan Zhao. 2015. "Water Pollution Progress at Borders: The Role of Changes in China's Political Promotion Incentives." American Economic Journal: Economic Policy 7(4): 223-42.

Karplus, Valerie J., and Mengying Wu. 2019. Smoke and Mirrors: Did China's Environmental Crackdowns Lead to Persistent Changes in Polluting Firm Behavior? Working paper.

King, G., & Nielsen, R. (2019). Why Propensity Scores Should Not Be Used for Matching. Political Analysis, 27(4), 435–454.

Konisky, David M., and Christopher Reenock. 2013. Compliance Bias and Environmental (In)Justice. Journal of Politics 75(2): 506–19.

Konisky, David M. and Manuel P. Teodoro. 2016. When Governments Regulate Governments. American Journal of Political Science 60(3): 559–574.

Kostka, Genia. 2015. Command Without Control: the Case of China's Environmental Target System. Regulation & Governance, 10(1), 58–74.

LaPlante, Benoit and Paul Rilstone. 1996. Environmental inspections and emissions of the pulp and paper industry in Quebec. Journal of Environmental Economics and Management 31:19-36.

Lin, Liguo. 2013. Enforcement of pollution levies in China. Journal of Public Economics 98: 32-43.

McRae, Shaun. 2015. "Infrastructure Quality and the Subsidy Trap." American Economic Review 105, (1): 35–66.

Maung, Min, Craig Wilson, and Xiaobo Tang. 2016. Political connections and industrial pollution: Evidence based on state ownership and environmental levies in China. Journal of Business Ethics 138(4):649-659.

Oliva, Paulina. 2015. "Environmental Regulations and Corruption: Automobile Emissions in Mexico City." Journal of Political Economy 123(3): 686-724.

O'Neill, S., Kreif, N., Grieve, R., Sutton, M., & Sekhon, J. S. (2016). Estimating causal effects: considering three alternatives to difference-in-differences estimation. Health Services and Outcomes Research Methodology, 16(1), 1–21.

Ripollone, J. E., Huybrechts, K. F., Rothman, K. J., Ferguson, R. E., & Franklin, J. M. (2018). Implications of the Propensity Score Matching Paradox in Pharmacoepidemiology. American Journal of Epidemiology, 187(9), 1951–1961.

Ryan, A. M., Kontopantelis, E., Linden, A., & Burgess, J. F., Jr. (2018). Now trending: Coping with non-parallel trends in difference-in-differences analysis. Statistical Methods in Medical Research, 28(12), 3697–3711.

Ryan, Nicholas. 2014. The Competitive Effects of Transmission Infrastructure in the Indian Electricity Market. working paper.

Seligsohn, D., Liu, M., & Zhang, B. (2018). The sound of one hand clapping: transparency without accountability. Environmental Politics, 27(5), 804–829.

Shen, Hongtao, and Zhou Yankun. 2017. Environmental law enforcement supervision and corporate environmental performance: Quasi-natural experimental evidence from environmental interviews. Nankai Business Review 20(6): 73-82.

Shimshack, Jay P. 2014. The Economics of Environmental Monitoring and Enforcement. Annual Review of Resource Economics 6(1): 339-360.

Shimshack, Jay P. and Ward, Michael B. 2005. Regulator Reputation, Enforcement, and Environmental Compliance. Journal of Environmental Economics and Management 50(3): 519-540.

Shi, Xinzheng and Zhufeng Xu. 2018. Environmental regulation and firm exports: Evidence from the eleventh Five-Year Plan in China. Journal of Environmental Economics and Management 89: 187-200.

Stuart, E. A. (2010). Matching Methods for Causal Inference: A Review and a Look Forward. Statistical Science, 25(1), 1–21.

Sun, Junxiu, Wang, Feng, Yin, Haitao, and Zhang, Bing. 2019. Money Talks: The Environmental Impact of China's Green Credit Policy. Journal of Policy Analysis and Management 38(3): 653-680.

Tanaka, Shinsuke. 2015. Environmental regulations on air pollution in China and their impact on infant mortality. Journal of Health Economics 42: 90-103.

van der Kamp, Denise. 2018. Can 'Police Patrols' Prevent Pollution? The Limits of Top-Down Bureaucratic Control in China. Working paper.

van der Kamp, Denise S. Forthcoming. Blunt force regulation and bureaucratic control: Understanding China's war on pollution. Governance.

Van Rooij, Benjamin. 2006. Implementation of Chinese Environmental Law: Regular Enforcement and Political Campaigns. Development and Change 37(1): 57-74.

Wang, Chunhua, JunJie Wub, and Bing Zhang. 2018. Environmental Regulation, Emissions and Productivity: Evidence from Chinese COD-emitting Manufacturers. Journal of Environmental Economics and Management 92: 54-73.

Wang, Hua. 2002. Pollution regulation and abatement efforts: evidence from China. Ecological Economics 41(1): 85 – 94.

Wang, Hua, and David Wheeler. 2005. Financial incentives and endogenous enforcement in China's pollution levy system. *Journal of Environmental Economics and Management* 49(1):174-196.

Wang, Hua, Nlandu Mamingi, Benoit Laplante and Susmita Dasgupta. 2003. Incomplete enforcement of pollution regulation: Bargaining power of Chinese factories. Environmental and Resource Economics 24(3):245-262.

Wang, Hua and David Wheeler. 2005. Financial incentives and endogenous enforcement in China's pollution levy system. Journal of Environmental Economics and Management 49(1):174-196.

Yadin, Sharon. 2019. Regulatory Shaming. *Environmental Law*, 49(2), 2019, 407–451.

Yin, Haitao, Zhang, Xuemei, and Wang, Feng. 2020. Environmental Regulations in China. PRINTED FROM the OXFORD RESEARCH ENCYCLOPEDIA: ENVIRONMENTAL SCIENCE.

Zhang, Bing, Xiaolan Chen, and Huanxiu Guo. 2018. Does central supervision enhance local environmental enforcement? Quasi-experimental evidence from China. Journal of Public Economics 164: 70–90.

Zeng, F., Wei, W., Li, M., Huang, R., Yang, F., & Duan, Y. (2015). Heavy Metal Contamination in Rice-Producing Soils of Hunan Province, China and Potential Health Risks. International Journal of Environmental Research and Public Health 12(12): 15584–1559.