Reciprocity and State Capacity: Evidence from a National Pollution Control Reform in China*

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Abstract

We study pollution control from the social contract perspective. Leveraging granular data on Chinese industrial firms, we find that firms with prior government support achieved greater emission reductions through costly adjustments than those without, following a 2007 reform that sharply elevated the government's pollution control incentives. We interpret these reductions as driven by the reciprocity between firms and the government, as (a) they intensified with regional reciprocity norms, and (b) the government, in turn, responded with future support. We further find that such reciprocity translated into significant declines in regional pollution concentrations, thereby strengthening the government's capacity for pollution control.

Keywords: pollution control, state capacity, social contract

JEL Codes: Q52, Q58, H11, H77, Z13, Z18

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1 Introduction

State capacity in modern societies has advanced remarkably over the past century. For example, few nations taxed more than 10% of GDP at the century's start, whereas 40% was not uncommon by its close (Besley, 2020). Yet, the capacity to control pollution still lags behind. Almost the entire global population (99%) remains exposed to unhealthy levels of pollution (World Bank, 2022*a*). How can a modern state enhance its capacity for pollution control?

One can approach this from two broad views on state effectiveness (Besley, 2020). The Hobbesian view (Hobbes, 1651), which underpins many environmental policies, advocates for coercive institutions such as laws and regulations, whose effectiveness can, however, be undermined by imperfect enforcement (Duflo et al., 2013), pollution displacement (Gibson, 2019), and motivation crowding (Bénabou and Tirole, 2003), among other factors. Conversely, the social contract view (Locke, 1690; Rousseau, 1762), rooted in Enlightenment thinkers, emphasizes reciprocal obligations between citizens and the state in achieving state effectiveness, as in the rise of the tax state (Levi, 1988; Slivinski and Sussman, 2019; Besley, 2020).

Recognizing the imperative of pollution control and the limitations of traditional approaches, we adopt the contractarian approach to examine whether a modern state can effectively reduce pollution by leveraging its reciprocal relationships with firms, the major contributors to pollution. Conceptually, this involves linking firms' environmental performance to the state's support for firms. However, doing so requires comprehensive records of firms' pollutant emissions and abatement strategies, which are often lacking in many states due to insufficient monitoring capacities; it also requires a consistent measure of the state's support for firms with sufficient cross-sectional variations, which is still scarce due to the heterogeneous preferences of local electorates and the limited power of many governments. These issues are further complicated by the dynamic nature of reciprocal obligations, which necessitates repeated observations of state-firm interactions. Finally, the state may strategically grant support to firms with high compliance, posing a significant challenge to identifying the causal relationship between reciprocity and pollution reduction.

To tackle these challenges, we employ granular firm-level data from China covering the period between 2001 and 2010. Thanks to China's strong bureaucracy in carrying out large-scale economic and environmental statistics, we have access to detailed records on the environmental performance, including various pollutant emissions and installed abatement facilities, of major polluters nationwide over an extended period. We also observe the subsidies that these firms received from the government, a typical form of government support. Given the central role of Chinese local governments in developing regional economies and their high discretion in resource allocation

(Maskin, Qian and Xu, 2000; Xu, 2011), we are able to consistently track these subsidies over years with sufficient variations across firms. We then bring the granular data to a natural experiment, which sharply increased local governments' pollution control incentives, for causal identification. Specifically, in 2007, the central government elevated pollution control to an unprecedented priority in the cadre evaluation system: local officials would face disqualification for promotion or other administrative penalties if they fail to reduce targeted pollutants, irrespective of performance in other areas. This reform thus created exogenous shocks to local officials' incentives to reduce pollution.

We proceed by adopting a difference-in-differences (DID) identification strategy to examine the role of reciprocity in motivating firms to reduce pollution. Specifically, we define treatment firms as firms receiving subsidies in the pre-reform period (2001-2006) and control firms as those that did not. We then compare their environmental performance in response to the reform. The identification relies crucially on the parallel trends assumption, which posits that the performance of these two types of firms should evolve in parallel absent the reform. We believe this assumption is plausible in our setting, given the exogeneity of the reform, and we will further support it through event studies. Our baseline analysis focuses on the emissions of the two key pollutants targeted by the reform: sulfur dioxide (SO_2) and chemical oxygen demand (COD), which are key air and water pollutants, respectively. We find that treatment firms experienced a 14.1% greater reduction in SO_2 emissions and an 18.7% greater reduction in COD emissions after the reform relative to control firms. These magnitudes are comparable to the effects of conventional regulations in the same context, suggesting the powerful and complementary role of reciprocity. We then leverage institutional knowledge on the reform's targets to design a placebo test: while the reform was sweeping, it only targeted key pollutants. We examine firms' emissions of non-targeted pollutants and find no differential changes. This largely rules out the concern that our baseline findings are driven by general changes in firms' greenness after 2007 that differed across groups.

We employ multiple methods to sharpen the identification, which can be categorized into two main clusters. The first focuses on minimizing covariate imbalance between treatment and control firms, although we note that perfect balance is not required by our DID strategy. We achieve this by: (a) including throughout our analysis a comprehensive set of firm fixed effects, industry $\times$ year fixed effects, and pre-reform firm characteristics interacted with year fixed effects, to remove heterogeneity across firms; (b) comparing firms within narrowly defined groups that share the same baseline characteristics and trend in the same way to further achieve covariate balance; and (c) adopting a reweighting scheme to directly achieve covariate balance. The second involves testing and allowing for potential non-parallel trends between treatment and control firms. We start with a standard event study, where we find no discernible pre-trends in emissions between treatment and control firms. We then relax the parallel trends assumption using: (a) a "partial identification" scheme that bounds post-reform trends; (b) a flexible regression specification with linear trends varying by firms, treatment groups, and even time periods; and (c) a within-treatment identification strategy exploiting additional variations in firms' capacity to cut emissions. Our findings remain highly consistent across all these specifications. We also address some implicit concerns such as anticipation and spillover effects. In addition to these identification checks, we further show that our findings are insensitive to alternative variable measures and inference procedures, robust to controlling for other concurrent policies, and not driven by marginal firms or specific industries and regions.

Having demonstrated the robustness of our baseline findings, we turn to examine how treatment firms achieved greater emission reductions in response to the reform. Conceptually, they can adjust by decreasing emissions per unit of output, which entails the adoption of clean technology, or by simply reducing total output. We leverage detailed abatement and production information in our data to examine these margins. First, we examine adjustments on the clean technology side. We find that treatment firms organized production in a way that consumed less coal and industrial water per unit of output. We also find that they installed more pollution abatement facilities. However, we observe little change in their green innovation. Next, we examine adjustments on the production side. We find a drop in total output, as well as drops in profitability, sales, and total factor productivity. These results suggest that treatment firms undertook costly actions to reduce emissions, which is consistent with a notion of reciprocity towards the government for the previous support received.

We interpret treatment firms' costly emission reductions as driven by their reciprocity towards the government. We support this mechanism in two ways. First, we examine whether the emission reductions varied by local norms of reciprocity between citizens and the state, as reciprocal obligations should be governed by such norms. We exploit three nationally representative surveys, each with different samples and questions, to construct measures of such norms. The first survey asks citizens about their willingness to pay taxes for universal healthcare. The second survey asks entrepreneurs about their willingness to donate in gratitude to the government. The third survey asks citizens about their willingness to repay even a small favor. Although these three questions were asked at different times, to different respondents, and with different connotations, they all capture, to some extent, a notion of reciprocity between citizens and the state. We then aggregate each of the answers to the city level after partialling out individual characteristics and create three standardized measures of norms of reciprocity. We find consistent patterns across these three measures: treatment firms' emission reductions were larger in cities with stronger norms of reciprocity. We also find similar economic magnitudes across different measures. These patterns thus suggest that treatment firms' greater emission reductions were likely driven by their reciprocity towards the government.

Second, we adopt a forward-looking approach to unpack the nature of treatment firms' greater emission reductions: if they were motivated by reciprocity, the government would, in turn, respond with additional support in the future, as reciprocity is by definition dynamic. We substantiate this idea by examining whether treatment firms' emission reductions are positively associated with their future government subsidies, which we indeed find to be the case. Additionally, we observe that treatment firms' emission reductions largely preceded the government's additional future subsidies, alleviating the concern that the emission reductions could be driven by concurrent subsidies received. Finally, we find that the additional future subsidies, while statistically significant, are relatively small in economic terms and can hardly compensate for treatment firms' large costs of emission reductions. This further rules out an alternative story of material exchanges and aligns precisely with the notion of reciprocity, where firms responded to the government's previous support even at a higher cost. These results, together with our previous results on the heterogeneity in treatment firms' emission reductions across areas with varying norms of reciprocity, provide compelling evidence on the reciprocity mechanism underlying treatment firms' greater emission reductions.

We rule out several alternative explanations for our findings. First, one may wonder if the greater emission reductions were driven by treatment firms' prior subsidies that eased their financial constraints, a key factor influencing firms' environmental decisions. We believe this is unlikely due to the relatively small size of prior subsidies. We also construct various measures of firms' financial constrains and find that our results remain virtually unchanged when including these measures, suggesting that differential financial constraints do not explain our findings. Second, one may wonder if the greater emission reductions were caused by enhanced regulatory enforcement. To investigate this, we use administrative data on environmental punishment to measure regulatory enforcement and find no evidence that treatment firms experienced differential regulatory enforcement. Finally, one may question if treatment firms were also more likely to be politically connected firms, which cut emissions largely out of material exchanges with politicians. While we lack direct measures of political connections in our firm-level data, we note that treatment firms constitute a large fraction (39%), whereas political connections are typically concentrated in a small set of firms due to the nature of the relationships. We also formally address this concern in two ways. First, we analyze a sample of public firms, for which we have standard measures of their political connections (e.g., board connections with politicians), and find no change in emission reductions when controlling for political connections. Second, we turn to our full sample and test whether the emission reductions were smaller in cities with recent turnovers of local leaders, which would pose a negative shock to local firms' political connections. We do not find smaller effects in cities with such political turnovers.

Finally, we conduct a regional-level analysis to illuminate the broader impacts of the reciprocity between firms and the government. This is crucial because firm-level findings alone may not fully capture regional pollution dynamics, especially if firm-level reductions were offset by intra-city pollution displacement or if treatment firms contributed minimally to regional pollution. We do so by aggregating the firm-level treatment to the city level, collecting new data on citywide air and water pollution, and exploiting within-city variations net of provincial shocks for identification. For air pollution, we find that cities with higher fractions of treatment firms witnessed greater declines in citywide SO_2 concentrations derived from satellite observations. We confirm the robustness of this finding through an event study, a placebo test using ozone concentrations, an alternative outcome summing total emissions in a city, and a firm size-weighted treatment measure. For water pollution, we similarly find that cities with higher fractions of treatment firms witnessed greater declines in citywide COD concentrations, which are measured using inverse distance-weighted average COD readings from monitoring stations downstream of a city (as water pollutants only flow from high elevations to low elevations). We also confirm the robustness of this finding through an event study, a placebo test using upstream readings, a simple average of downstream readings without weighting, and a firm size-weighted treatment measure. These aggregate impacts are consistent and substantial in magnitudes, suggesting that the reciprocal relationships between individual firms and the government collectively strengthened the government's capacity for pollution control on a regional scale.

Our paper contributes to several strands of literature. First, we contribute to a nascent literature studying state capacity from the social contract perspective. While Enlightenment thinkers long emphasized the role of reciprocity between citizens and the state in shaping state effectiveness, this view has been mostly applied to a few specific aspects of state capacity, particularly taxation (Levi, 1988; Levi and Sacks, 2009; Doerrenberg and Peichl, 2018; Slivinski and Sussman, 2019; Besley, 2020), which is already well-developed in many modern states. A recent work by Caprettini and Voth (2023) broadens this discourse by examining reciprocity in wartime mobilization, showing that U.S. New Deal spending boosted citizens' patriotism during World War II. Similarly, Qian and Tabellini (2024) explore a type of "negative reciprocity" in this context, showing that racial discrimination eroded support for the government among excluded groups following the Pearl Harbor attack. Our paper extends this literature by first applying the contractarian view of the state to pollution control, an underdeveloped area pertinent to many modern states.

In highlighting the role of reciprocity in strengthening state capacity, our paper joins a broader literature on the origins of state capacity. Starting from Tilly (1990)'s intriguing view that "war made states", various determinants of state capacity have been explored, including endogenous investments by state actors (Besley and Persson, 2009, 2011), institutional constraints on elite power (Acemoglu and Robinson, 2013), local knowledge of policy enforcers (Balán et al., 2022), monitoring costs within state territories (Mastrorocco and Teso, 2023), and the separation of administration from politics (Aneja and Xu, 2024). Our paper offers a complementary view on state effectiveness from the social contract perspective.

Second, we contribute to a growing literature on the challenges of implementing environmental regulations, particularly in weak states. A common obstacle to such regulations is imperfect enforcement, as noted by Duflo et al. (2013), Greenstone and Hanna (2014), Duflo et al. (2018), and Deng and Axbard (2021). Even when regulations succeed locally, their aggregate impacts remain uncertain, as firms may shift production to unregulated regions (Chen et al., 2018; Gibson, 2019; Chen et al., 2023*a*; Zhang and Zhao, 2023). Additionally, there could also be motivation crowding (Bénabou and Tirole, 2003; Falk and Kosfeld, 2006). The contractarian approach explored in our paper can be a promising means of overcoming these challenges.

Third, we shed new light on the political economy of pollution control in China (Kahn, Li and Zhao, 2015; Chen, Li and Lu, 2018; He, Wang and Zhang, 2020; Wu and Cao, 2021). The literature generally concurs that integrating emission reduction targets into the cadre evaluation system effectively incentivized local governments to reduce pollution, as evidenced by notable improvements in environmental quality over the past decade (Greenstone et al., 2021). However, an underexplored question remains: once local governments had incentives to control pollution, how did they achieve this in such a short timeframe? Our findings suggest a new mechanism beyond the typical view of regulatory enforcement: local governments leveraged reciprocal relationships with firms to mobilize compliance.[1]

Finally, there is a recent literature exploring two-way exchange relationships between firms and governments (Faccio and Hsu, 2017; Lei, 2021; Szeidl and Szucs, 2021; Chen et al., 2023*b*). For instance, Lei (2021) shows that Chinese firms benefiting from preferential loans and tax breaks helped local governments retain tax revenues under a central-local tax-sharing rule. We differ in the nature of the relationships: ours extend the notion of reciprocity rooted in familial and social structures to the state, with positive impacts on social welfare and state effectiveness, whereas relationships in this literature can be best depicted by cooperative behaviors in repeated games purely

[1] Our findings do not negate the effectiveness of regulatory enforcement in China; rather, they suggest that, given China's strong state apparatus, both methods can be effective.

driven by material payoffs, whose societal implications can be obscure.

The remainder of this paper is structured as follows. Section 2 introduces the institutional background. Section 3 describes the main data and variables. Section 4 discusses the identification strategy. Section 5 presents the baseline results and robustness checks. Section 6 explores the mechanisms. Section 7 investigates the aggregate impacts. Section 8 concludes.

2 Institutional Background

2.1 Foundations of Reciprocity between Firms and the Government

Reciprocity is a social norm of responding to kindness with kindness, even if it is costly (Fehr and Gächter, 2000; Sobel, 2005).[2] This concept has its biological roots in cooperative behaviors among species (Trivers, 1971; Bowles and Gintis, 2011). It can also develop from socialization (Bisin and Verdier, 2001; Tabellini, 2008), and evolve with institutions and policies (Bisin and Verdier, 2024; Persson and Tabellini, 2021). Historically, reciprocity has guided the formation of familial and social structures by offering competitive advantages in survival and resource competition (Gouldner, 1960). More recently, it has been applied to the social contract view of the state, where reciprocal obligations between citizens and the state are key to state effectiveness (Locke, 1690; Rousseau, 1762; Levi, 1988; Besley, 2020). When applying this concept to firm-government relationships, it becomes crucial to ask whether firms have obligations beyond profit maximization. The modern view of corporate social responsibility suggests that they do (Besley and Ghatak, 2007; Kitzmueller and Shimshack, 2012; Liang and Renneboog, 2017).

How could China's institutional features accommodate this notion of reciprocity and hone firm-government relationships? China's regionally decentralized authoritarian (RDA) regime (Xu, 2011) allows the central government to exert tight personnel control over local governments through performance evaluations centered on economic growth (Li and Zhou, 2005; Jia, Kudamatsu and Seim, 2015; Landry, Lü and Duan, 2018).[3] Local governments, incentivized by these evaluations, run the bulk of self-contained regional economies that facilitate competition (Maskin, Qian and Xu, 2000). This regime effectively places local governments in fierce economic tournaments and ensures a favorable environment for firms to thrive, despite the flux of individual officials who come and go (Xu, 2011; Bai, Hsieh and Song, 2019), and the weaknesses in the rule of law and governance quality (Allen, Qian and Qian, 2005;

[2] This, however, differs fundamentally from cooperative behaviors in repeated interactions, which arise due to pure material payoffs (Fehr and Gächter, 2000).

[3] There is also a literature stressing the role of connections with upper-level leaders (Shih, Adolph and Liu, 2012; Jia, Kudamatsu and Seim, 2015; Meyer, Shih and Lee, 2016).

Huang, 2008). This institutional cohesiveness provides reassurance to firms that the government would reciprocate by maintaining a conducive environment—not only benefiting themselves but also society as a whole—should they comply, thereby fostering reciprocity between firms and the government in the long-term.[4]

2.2 The National Pollution Control Reform in 2007

China's spectacular growth over the past three decades has exacted a heavy toll on the environment (Zheng and Kahn, 2017; Greenstone et al., 2021). In response, the central government elevated environmental protection to national strategic objectives in the Tenth Five-Year Plan (FYP) in 2001,[5] mandating a 10% reduction in key pollutants like SO_2 and COD by 2005.[6] However, this target was devolved to local governments without effectively aligning their incentives and was largely unmet by 2005.[7] A similar 10% reduction target was reiterated in the Eleventh Five-Year Plan (2006-2010), but emissions continued to rise in 2006.[8] These failures are unsurprising given the distorted incentives created by the cadre evaluation system (Jia, 2017).

In 2007, the central government implemented a landmark policy shift, announcing that reductions in targeted pollutants would weigh heavily in the performance evaluations of local officials. Those who failed to meet these targets would face disqualification from promotion or other penalties, regardless of performance in other areas—a mechanism known as the "one-vote veto".[9] The central government anticipated that by integrating these reduction targets into the cadre evaluation system, local governments would be incentivized to prioritize pollution control. This reform effectively aligned local governments' incentives, and the overall reduction targets of the Eleventh Five-Year Plan were ultimately met by the end of 2010 (Kahn, Li and Zhao, 2015; Chen, Li and Lu, 2018). In our subsequent analysis, we focus on firms' interactions with local governments, given the central role of local governments in managing regional economies and the limited attention of the central government.[10] We then treat this reform as an exogenous shock to local governments' incentives for pollution

[4]This does not exclude pure material exchanges between firms and governments in China (Lei, 2021; Chen et al., 2023*b*), which could co-exist with the type of reciprocity we examine. However, we can empirically rule out this concern in our specific context of pollution control.

[5]China's Five-Year Plans are comprehensive policy blueprints that set economic, social, and environmental goals for the nation over five-year periods. Originating from the era of central planning, these plans remain critical instruments for guiding the country's development, shaping major policy initiatives, and driving long-term strategic objectives.

[6]See https://www.gov.cn/gongbao/content/2002/content_61775.htm

[7]For example, those who failed to meet the target were merely required to "introspect and propose corrective plans". See https://www.gov.cn/gongbao/content/2002/content_61775.htm

[8]See https://www.gov.cn/govweb/jrzg/2007-02/03/content_517034.htm

[9]See http://www.gov.cn/zwgk/2007-06/03/content_634545.htm

[10]In addition, as we discuss in the following section, local governments, particularly city-level governments, play a major role in allocating government subsidies, a typical form of support for firms.

control and examine how, once incentivized, they leveraged reciprocal relationships with local firms to reduce pollution.

2.3 Government Subsidies

We focus on government subsidies to firms in this paper, a common fiscal tool used to support activities that align with broader economic, social, or environmental objectives (Criscuolo et al., 2019; Branstetter, Li and Ren, 2023; Juhász, Lane and Rodrik, 2023). Government subsidies offer direct financial assistance to firms, with the government exercising considerable discretion in selecting recipients. In China, local governments, particularly city-level governments, are the primary granting authorities of subsidies (Nie, Li and Li, 2022).[11] The remaining subsidies come from the central authorities such as the Ministry of Finance, but prefecture-level governments still play a vital role in overseeing firms' applications. Their approval and endorsement are generally required before these applications can be considered at higher levels.

What are the types of subsidies and how are they allocated in China? Branstetter, Li and Ren (2023) conduct a textual analysis of Chinese public firms' financial statements that disclose the types of subsidies received. They categorize these subsidies into the following types according to their intended purposes: R&D and innovation, industrial and equipment upgrading, employment stabilization and promotion, environment protection, and other general business subsidies.[12] However, these subsidies can be easily redirected or repurposed by the recipient for different uses, not necessarily the specific purpose intended by the government (Branstetter, Li and Ren, 2023). Regarding the allocation of government subsidies, a common view in the literature is that local governments have preference for state-owned enterprises, large firms, and productive firms (Nie, Li and Li, 2022; Branstetter, Li and Ren, 2023). We will control for these factors, along with other firm characteristics, throughout our analysis to minimize differences between firms with and without such subsidies, although our identification does not rely on random allocation of these subsidies.

[11] Here we use the term "city" to denote administrative units below the provincial level but above the county level, which are officially called prefecture-level administrative units. As of 2010, there are 333 such units, including 283 prefecture-level cities (*dijishi*), 17 regions (*diqu*), 30 autonomous prefectures (*zizhizhou*) and 3 leagues (*meng*). To avoid confusion, we simply use "city" to encompass all these 333 prefecture-level administrative units, whose divisions can be found in Figure 5.

[12] Our firm-level data (Annual Survey of Industrial Firms) does not differentiate between these types of subsidies and only report total subsidies. Given that subsidy funds are likely fungible (Branstetter, Li and Ren, 2023), we believe it is reasonable to focus on total subsidies.

3 Data and Variables

We assemble various datasets in our empirical analysis. Our baseline firm-level analysis mainly utilizes two comprehensive firm-level datasets from 2001 to 2010, which provide us detailed information on firms' environmental performance, government subsidies, and baseline covariates. We also collect a couple of other datasets to disentangle the mechanisms and explore regional pollution concentrations.

3.1 Firm-Level Environmental Performance

We measure firms' environmental performance using the China Environmental Statistics Database (CESD), which is collected and maintained by the Ministry of Environmental Protection (MEP). This data covers industrial firms whose emissions of key pollutants fall within the top 85% of total emissions in a county.[13] It records basic firm characteristics (e.g., firm name, identifier, location), emissions of targeted pollutants (SO_2, COD) and non-targeted pollutants (e.g., dust, NH_3), resource consumption (e.g, water use, coal use), and pollution abatement facilities (e.g., waste gas and water treatment). Such rich information allows us to examine firms' emission reductions, as well as the adjustments they made to achieve the reductions. Specifically, we focus on the two targeted pollutants: SO_2 and COD, in our baseline analysis, which are key air and water pollutants, respectively. We also use the two non-targeted pollutants (dust and NH_3) to conduct a placebo test, and the information on resource consumption and abatement facilities to investigate the adjustments made by firms.

While this data is generally considered to be of high reliability (Zhang, Chen and Guo, 2018; He, Wang and Zhang, 2020), it is worth noting that the emissions are self-reported by each polluting firm, which may raise concerns about data quality. However, the reported data is subject to rigorous scrutiny by both local and upper-level government officials.[14] Government officials, in turn, cannot use the data as the basis for imposing administrative penalties on polluting firms.[15] This largely reduces firms' incentives to misreport. We also attempt to detect anomalies in the emission data by applying the widely used Benford's law, which prescribes the frequency distribution of the first digits in naturally occurring datasets (Marchi and Hamilton, 2006; Judge and Schechter, 2009). As shown in Appendix Figures A1, A2, and A3, we find no

[13]See https://www.mee.gov.cn/gkml/zj/wj/200910/W020070917492947218703.pdf

[14]For instance, the Environmental Protection Bureaus at various levels are required to conduct on-site inspections, document reviews, and other effective methods to review and verify the data provided by firms. See https://www.gov.cn/gongbao/content/2007/content_786259.htm

[15]In fact, this data is solely used for statistical purposes. See https://www.gov.cn/flfg/2010-02/02/content_1526177.htm. The Ministry of Environmental Protection further stipulates that administrative penalties for polluting firms should be based on online or on-site monitoring data. See https://www.gov.cn/gongbao/content/2023/content_5754536.htm

discernable deviations from this law in terms of firms' SO_2 and COD emissions.[16] We further confirm our firm-level findings in our subsequent regional-level analysis using pollution concentrations derived from satellite observations and monitoring stations, which is essentially immune to misreporting.

3.2 Firm-Level Government Subsidies and Covariates

We construct measures of government support for a firm as well as firm-level covariates using the Annual Survey of Industrial Firms (ASIF), which is collected and maintained by the National Bureau of Statistics (NBS). During our baseline sample period (2001-2010), it covers private industrial firms with annual sales over 5 million RMB, all state-owned industrial firms before 2007, and state-owned industrial firms with annual sales over 5 million RMB during 2007-2010. These firms span more than 40 two-digit industries and account for approximately 90% of total industrial output of China. The variables surveyed include both basic information (e.g., firm name, identifier, address, ownership) and balance sheet information (e.g.,government subsidies, total assets, fixed assets, sales, profits). While the NBS has rigorous procedures to ensure data quality, it is well-known that this data contains some outliers that apparently violate general accounting principles (e.g., firms with total assets smaller than fixed assets). We thus follow the convention in the literature to drop these outliers (Cai and Liu, 2009; Feenstra, Li and Yu, 2014).

The key variable of interest in this dataset is government subsidies, which we use as a measure of government support for firms. As discussed in the institutional background, these subsidies represent a typical and direct form of government assistance. In our baseline difference-in-difference estimation, we classify firms into treatment and control groups according to whether they received any subsidies during the pre-reform period (2001-2006). We also consider the frequency and amount of subsidies received during this period as measures of treatment intensity. Two remaining issues regarding the subsidies are noteworthy. First, the ASIF data only provides the total amount of subsidies without specifying the types. However, this concern should be mitigated by the fungibility of subsidy funds (Branstetter, Li and Ren, 2023). Second, the ASIF data lacks subsidy information for the years 2008-2010. Since we our treatment is defined using subsidy information from earlier years, this gap does not affect our baseline analysis. We supplement the missing subsidies using another comprehensive firm-level dataset (the National Tax Survey Database) when this information

[16]Specifically, Benford's law implies: $P(\text{first digit is } n) = \log(1 + \frac{1}{n})$, where n represents integers from 1 to 9. In Appendix Figure A1, we show that the frequency distribution of the first digits in our emission data closely resembles the distribution implied by this law. We also split our sample based on firms' treatment status (treatment or control) and time period (before or after the reform), and still find no deviations from this law (Appendix Figure A2 and A3).

is needed to examine the government's future support.

3.3 Other Data

We also collect a variety of other datasets for various uses, including: (1) the China's Administrative Registration Database, which we use to identify conglomerates when exploiting within-treatment variations for identification; (2) firm patent data obtained from the China National Intellectual Property Administration (CNIPA), which we use to measure firms' innovation; (3) the China General Social Survey (CGSS), the China Private Enterprise Survey (CPES), and the Chinese Social Survey (CSS), which are used to extract citizens' attitudes on reciprocity and to construct measures of city-level norms of reciprocity; (4) the National Tax Survey Database (NTSD), which we use to supplement the missing subsidies in the ASIF dataset when examining the government's future support; (5) local governments' annual work reports from official government websites, which we use to construct measures of city-level emission reduction targets; (6) administrative data on environmental punishment collected from local Environmental Protection Bureaus (EPB), which we use to measure regulatory enforcement; (7) public firm data collected from the China Stock Market & Accounting Research Database (CSMAR), which we use to measure firms' political connections; (8) local leader data (mayors and party secretaries) collected from government official websites and Baidu Baike (China's equivalent of Wikipedia), which we use to measure political turnovers; (9) city-level SO_2 concentrations from the National Aeronautics and Space Administration (NASA) and COD concentrations derived from monitoring station readings published by the China Environmental Yearbooks, which we use to measure regional pollution dynamics; and (10) city-level economic controls (GDP per capita, fiscal revenue, population, and industrial production) obtained from the City Statistical Yearbooks, as well as weather controls (temperature, precipitation, wind direction, wind speed, and dew point temperature) derived from the National Oceanic and Atmospheric Administration (NOAA), which are used as covariates in our regional-level analysis. We leave the detailed discussion of these datasets to where they first appear in our subsequent analysis.

3.4 Sample Construction and Summary Statistics

We merge the two firm-level datasets, the CESD and the ASIF, to generate an unbalanced firm-level panel from 2001 to 2010 through the following steps. First, we follow the literature to remove observations that apparently violate general accounting principles, namely, those with total assets smaller than liquid assets, fixed assets, or net fixed assets (Cai and Liu, 2009; Feenstra, Li and Yu, 2014). Second, we conduct an initial matching according to firm identifier and year. Third, we conduct an additional matching according to firm name and year. Fourth, we combine the data obtained in the previous two steps and drop duplicate observations. We then generate our final firm-level sample by excluding the following observations: (1) firms registered in the four direct-controlled municipalities (*Beijing*, *Shanghai*, *Tianjin*, and *Chongqing*), which face a different governance structure and align closely with the central government;[17] and (2) firms with observations missing in the entire pre-reform or post-reform period, to ensure within-firm comparisons before and after the reform. Finally, we winsorize all continuous variables at the 1% and 99% percentiles to reduce the influence of outliers.

After imposing the above restrictions, we obtain an unbalanced panel from 2001 to 2010, with 23,609 unique firms and 143,524 firm-year observations. Table 1 presents summary statistics for the final sample used in our firm-level analysis, including the outcome variables (firms' emissions of SO_2 and COD), the treatment indicator (firms that received any pre-reform government subsidies), and other covariates (initial year values). On average, 38.7% of firms received subsidies during the pre-reform period. In Appendix Figure A4, we present the distribution of pre-reform government subsidies received by firms in this sample. Panel (a) shows the top 10 industries receiving the highest share of total subsidies, which are concentrated in heavy industries. Panel (b) shows the frequency of subsidies: approximately two-thirds of the firms received no subsidies, 14.5% received subsidies once, 7.9% received subsidies twice, and 12.4% received subsidies three or more times. Panel (c) shows the unconditional distribution of subsidies normalized by sales, which is right-skewed with an average of approximately 0.4%. Panel (d) shows the distribution of subsidies normalized by sales, conditional on receiving subsidies, which is also right-skewed with an average of approximately 1.3%. Overall, these subsidies are relatively small compared to the size of the firms.

4 Identification Strategy

4.1 Baseline Specification

We adopt a difference-in-differences (DID) strategy for causal identification. Specifically, we use the 2007 reform, which integrated emission reduction targets into the cadre evaluation system, as an exogenous shock to local governments' incentives to control pollution. Local firms' emission reductions were, in turn, affected by the prior support they received from the government, which we measure using pre-reform government subsidies. Our DID strategy thus compares pollutant emissions of firms

[17] Our results still remain consistent if we do not exclude these observations.

with differential pre-reform government subsidies before and after 2007, which takes the following form:

$$Emission_{ijt} = \beta Treat_i \times Post_t + \mathbf{X}_i \times \lambda_t + \mu_i + \eta_{jt} + \varepsilon_{ijt} \tag{1}$$

where the unit of observation is a firm-year, and the sample period spans the years 2001 to 2010. i, j, and t denote firm, industry, and year, respectively. $Emission_{ijt}$ denotes measures of firms' pollutant emissions. As the raw emissions are right-skewed and could be zero-valued, we apply the inverse hyperbolic sine transformation and check robustness using raw emissions (Bellemare and Wichman, 2020; Chen and Roth, 2023).[18] $Treat_i$ is a dummy variable equal to 1 if firm i received any subsidies from 2001 to 2006, and 0 otherwise. We use this binary treatment in our baseline specification and check robustness using the frequency or amount of subsidies received as treatment intensity measures. $Post_t$ is a dummy variable equal to 1 for 2007 and later years, and 0 otherwise. μ_i denotes firm fixed effects, controlling for time-invariant firm characteristics. η_{jt} denotes (2-digit) industry $\times$ year fixed effects, controlling for all factors, including those time-varying, at the industry level, such as differential regulatory standards or emission dynamics across industries. To further filter out firm-level confounders, we include a set of pre-reform firm characteristics $\mathbf{X}_i$ interacted with year fixed effects λ_t, which we discuss later on.

The coefficient of interest is β, which captures the differential changes in emissions after the reform between treatment and control firms. We expect β to be negative, implying that firms with pre-existing subsidies from the government are more willing to reduce emissions following the reform relative to those without. However, we also note that β, by construction, reveals only the relative emission changes between treatment and control firms in our sample and may not be informative about regional-level pollution dynamics. Therefore, we also conduct a regional-level analysis in later sections by aggregating the firm-level treatment and examining the impacts on regional pollution concentrations. For statistical inference in our firm-level analysis, we cluster the standard errors at the firm level in our baseline specification (Abadie et al., 2023), and assess robustness using alternative inference procedures, including: (1) clustering at alternative levels; (2) correcting for spatial correlation (Conley, 1999); and (3) conducting randomization inference (Young, 2019).

[18]The inverse hyperbolic sine transformation of a variable x is: $IHS(x) = \ln(x + \sqrt{x^2+1})$, which approximates the logarithm transformation but is well defined at zero (Bellemare and Wichman, 2020). In Appendix Table A5, we show that our results are robust to using the raw pollutant emissions, which alleviates the concerns about log-like transformations raised by the recent econometric literature (Chen and Roth, 2023).

4.2 Threats to Identification

Our DID strategy relies crucially on the parallel trends assumption, which posits that the emissions of treatment and control firms would have evolved in parallel in the absence of the reform. We believe this assumption is likely to hold due to exogeneity of the reform. However, pre-existing covariate imbalance between treatment and control firms may still raise concerns about the plausibility of this assumption. We take several strategies to mitigate this concern, although we note that perfect balance is not required in DID designs.[19] The inclusion of firm fixed effects μ_i and industry $\times$ year fixed effects η_{jt} already filters out time-invariant heterogeneity across firms and industry-level shocks that may correlate with our treatment measures and drive firms' differential responses to the reform. We also experiment with more disaggregated (4-digit) industry $\times$ year and city $\times$ year fixed effects in robustness checks, but refrain from doing so in our baseline regressions to avoid overspecification (Miller, Shenhav and Grosz, 2023).[20]

To further control for potential covariate imbalance, we include in $\mathbf{X}_i$ an extensive set of pre-reform firm-level covariates: size, age, patents, profitability, sales, exports, ownership status, employees, capital, and total factor productivity (TFP), all interacted with year fixed effects λ_t.[21] We choose to be conservative by considering all these covariates throughout our firm-level analysis and check robustness using the Double LASSO method to guide covariate selection (Belloni, Chernozhukov and Hansen, 2014). We also nonparametrically adjust for covariate imbalance by including decile bin fixed effects of $\mathbf{X}_i$ interacted with year fixed effects λ_t.[22] This ensures that we are comparing firms within narrowly defined groups that have the same baseline characteristics and trend in the same way. As a final attempt, we present results using the Entropy Balancing method, which achieves covariate balance by reweighting observations and obviates the need for balance checks (Hainmueller, 2012).

Despite the above attempts to achieve covariate balance, unobserved time-varying heterogeneity between treatment and control firms remains a potential threat to the plausibility of the parallel trends assumption. As it is inherently difficult to completely mute this concern in observational settings, we instead adopt four additional strategies to directly assess the plausibility of this assumption in our setting and flexibly account for possible violations of this assumption.

[19] The parallel trends assumption allows for covariate imbalance as long as its impact on outcomes, if any, remains constant over time.

[20] As shown by Miller, Shenhav and Grosz (2023), overspecifying a model with disaggregated fixed effects may induce non-random selection of groups into the identifying sample when treatment only varies within some groups.

[21] We use the initial values of these covariates in our sample period interacted year fixed effects to avoid the bad control issue (Angrist and Pischke, 2009). See Table 1 for detailed description of these covariates.

[22] One exception is ownership status, which is a time-invariant dummy variable.

First, we estimate a fully dynamic event study version of equation (1) to check for the existence of pre-trends in our setting:

$$Emission_{ijt} = \sum_{k=2001,k\neq 2006}^{k=2010} \beta_k Treat_i \times Year_k + \mathbf{X}_i \times \lambda_t + \mu_i + \eta_{jt} + \varepsilon_{ijt} \tag{2}$$

where we replace the post-reform indicator $Post_t$ in our baseline specification with a set of year dummies, with the year prior to the reform omitted as the reference year. The coefficient β_k thus captures the difference in emissions between year k and the reference year among treatment firms, relative to control firms. If the β_ks are small and statistically insignificant for $k < 2007$, then one can be confident about the plausibility of (counterfactual) parallel trends in the post-reform period. However, the pre-trends test could be underpowered, and conditioning on passing the test could lead to distortions of both estimation and inference (Freyaldenhoven, Hansen and Shapiro, 2019; Roth, 2022).[23] We address these concerns using several other strategies below that relax the parallel trends assumption.

Second, we adopt a partial identification scheme following Rambachan and Roth (2023), who use pre-trends to bound post-reform violations of parallel trends, thus providing partial identification for the treatment effects under non-parallel trends. Rambachan and Roth (2023) provides two bounding methods: "bounding relative magnitudes" and "smoothness restrictions". Specifically, let ϕ_t denote the difference in emission trends between treatment and control firms in year t, then the two methods can then be described as follows:

$$|\phi_{t+1} - \phi_t| \leq \bar{M} \cdot \max_{s<2007} |\phi_{s+1} - \phi_s|, \ \ \forall t >= 2007 \tag{3}$$

$$|(\phi_{t+1} - \phi_t) - (\phi_t - \phi_{t-1})| \leq M, \ \ \forall t \tag{4}$$

where the first imposes that the maximum post-reform violations of parallel trends between consecutive periods can not exceed $\bar{M}$ times the maximum pre-reform violations of parallel trends; the second allows the slope of the differential trends to change by no more than M between consecutive periods. We adopt both methods and flexibly vary the parameters $\bar{M}$ and M to check the sensitivity of our results to various types of violations of the parallel trends assumption.

Third, to the extent that there exist differential linear trends in emissions across firms, we can directly control for them. We start with including firm-specific linear time trends, which is a demanding specification that may absorb much of the treatment variation (Goodman-Bacon, 2021). Alternatively, we follow Alder, Shao and Zili-

[23]Conditioning on passing the pre-trends test may introduce bias because the dataset in which we fail to detect a significant pre-trend could be a selected sample from the true data generating process (Roth, 2022).

botti (2016) to estimate the following specification, which allows the trends to vary by treatment groups, and further vary differentially after the reform:

$$\begin{aligned} Emission_{ijt} &= \beta Treat_i \times Post_t + \gamma Treat_i \times (t - 2001) \\ &\quad + \delta Treat_i \times Post_t \times (t - 2007) + \mathbf{X}_i \times \lambda_t + \mu_i + \eta_{jt} + \varepsilon_{ijt} \end{aligned} \tag{5}$$

where $Treat_i \times (t - 2001)$ allows for a linear time trend specific to treatment firms and $Treat_i \times Post_t \times (t - 2007)$ further introduces a linear trend break after the reform for these firms. The latter term partially filters out additional shocks that treatment firms could possibly be exposed to in the post-reform period, which may explain their differential responses. This method roughly corresponds with the "smoothness restrictions" method in Rambachan and Roth (2023), but could provide point identification of the treatment effects.

Fourth, we exploit within-treatment variations for identification, thus allowing for arbitrary differential trends between treatment and control firms. Specifically, we define measures of firms' capacity to reduce emissions and then compare firms with high versus low reduction capacity within the same treatment group. Our estimating equation takes the following form:

$$\begin{aligned} Emission_{ijt} &= \beta Treat_i \times Post_t \times HighReductionCapacity_i + Treat_i \times \lambda_t \\ &\quad + HighReductionCapacity_i \times \eta_{jt} + \mathbf{X}_i \times \lambda_t + \mu_i + \eta_{jt} + \varepsilon_{ijt} \end{aligned} \tag{6}$$

where $HighReductionCapacity_i$ is a dummy variable indicating firms with high reduction capacity, constructed based on firms' emission history and organization structure that could affect their ability to respond to the reform.[24] The coefficient β on the triple interaction term $Treat_i \times Post_t \times HighReductionCapacity_i$ captures the additional emission reductions among high capacity treatment firms relative to low capacity treatment firms.[25] The inclusion of $Treat_i \times \lambda_t$ filters out all treatment-specific heterogeneity, both time-invariant and time-varying. We also interact $HighReductionCapacity_i$ with industry-year fixed effects η_{jt} to allow high capacity firms to have independent industry-level trends in emissions.

Finally, we assess the plausibility of some implicit assumptions imposed by our DID strategy, as violations of them may also bias the estimates. The first is the stable unit treatment value assumption (SUTVA), which rules out spillovers between treatment and control firms. We empirically quantify the magnitude of spillovers by including the average treatment status of all other firms around a firm in our baseline

[24] See Section 5.1.5 for a detailed description of how we construct these measures.

[25] Note here the lower order interaction terms, $Treat_i \times Post_t$, $Post_t \times HighReductionCapacity_i$, and $Treat_i \times HighReductionCapacity_i$ cannot be included, as they are collinear with the fixed effects in this equation.

specification. The second is the no-anticipation assumption, which implies that treatment firms should not react to the reform before its implementation. As violations of this assumption are observationally equivalent to violations of the parallel trends assumption, we can test for this using the same dynamic specification as equation (2). Alternatively, we can check robustness by discarding observations just prior to the reform that may be contaminated by potential anticipation effects.

5 Results

5.1 Firm-Level Emission Reductions

5.1.1 Baseline Estimates on Targeted Pollutants

Table 2 presents the estimates of β using the baseline DID specification (equation (1)), showing that treatment firms (those with pre-reform government subsidies) exhibited greater emission reductions after the reform compared to control firms (those without pre-reform government subsidies). We examine SO_2 emissions in columns (1)-(3) and COD emissions in columns (4)-(6), with the raw emissions transformed using the inverse hyperbolic sine function. Columns (1) and (4) adopt the simplest specification, which includes only firm fixed effects, year fixed effects, and the interaction between the treatment indicator and post-reform indicator. Columns (2) and (5) replace the year fixed effects with more granular (2-digit) industry $\times$ year fixed effects to filter out industry-level shocks such as differential emission dynamics across industries. Columns (3) and (6) further augment the model by including a set of pre-reform firm characteristics interacted with year fixed effects to account for time-varying heterogeneity across firms. We obtain fairly stable estimates across these specifications. The point estimates decrease slightly in magnitude but remain negative and statistically significant at the 1% level. In our preferred specifications in columns (3) and (6), the estimates indicate that treatment firms experienced a 14.1% greater reduction in SO_2 emissions and an 18.7% greater reduction in COD emissions after the reform compared to control firms. These findings indicate that firms receiving prior government support were more willing to reduce emissions in response to the government's pollution control needs compared to firms without such support.

To better benchmark the role of government support in motivating firms to reduce emissions, we reference studies that examine the effects of conventional environmental regulations on firm-level emissions within the same context. For example, Fan et al. (2019) find that the environmental regulations stipulated by the Eleventh Five-Year Plan decreased firms' SO_2 and COD emissions by 9.9% and 7.3%, respectively. Zhang and Zhao (2023) find that the regional environmental regulation in Beijing and its surrounding areas decreased SO_2 emissions by 17.6%. Zhang, Chen and Guo (2018) find that the central monitoring of local emissions, aimed at enhancing local environmental regulations, reduced firms' COD emissions by 26.8%. Our estimates on SO_2 and COD emissions are thus comparable to the effects of these regulations. However, as we elaborate in the mechanism section, our findings on emission reductions are driven by a fundamentally different mechanism, namely, the reciprocity between firms and the government, which is potentially immune to the various drawbacks of conventional regulations, such as imperfect enforcement (Duflo et al., 2013; Greenstone and Hanna, 2014; Duflo et al., 2018; Deng and Axbard, 2021) and pollution displacement (Chen et al., 2018; Gibson, 2019; Chen et al., 2023*a*; Zhang and Zhao, 2023).

5.1.2 Event Study

As we discuss in Section 4.2, our DID strategy relies on the parallel trends assumption. To test the plausibility of this assumption in our setting and study the dynamics of emission reductions, we estimate an event study specification (equation (2)) that includes leads and lags of the reform. As shown in Figure 1, there are no discernible differential trends in emissions prior to the reform. The coefficient estimates on the leads are small and statistically insignificant. Furthermore, F-tests for the joint significance of the coefficient estimates on the leads yield p-values of 0.67 for SO_2 emissions and 0.68 for COD emissions. These results thus lend strong support to the plausibility of the parallel trends assumption. In contrast, the coefficient estimates on the lags, or the dynamic treatment effects, are negative and statistically significant, with the effect magnitudes gradually increasing over time. This suggests that prior government support played a significant role in motivating firms to reduce emissions following the reform.

The insignificant pre-trends in the event study plots, however, may not necessarily guarantee the absence of counterfactual differential trends in the post-reform period. In addition, the pre-trends test may fail to reject due to low power, and conditioning on passing such a test may further bias our estimates on the treatment effects (Freyaldenhoven, Hansen and Shapiro, 2019; Roth, 2022). To address these concerns, we follow Rambachan and Roth (2023) to adopt a partial identification scheme that does not rely on the parallel trends assumption. As described in equations (3) and (4), Rambachan and Roth (2023) partially identify the treatment effects under two types of restrictions on violations of parallel trends. The "bounding relative magnitudes" method assumes that the maximal post-reform violations of parallel trends between consecutive periods cannot be larger than $\bar{M}$ times the maximal pre-reform violations of parallel trends. The "smoothness restrictions" method allows the slope of the differential trends to change by no more than M between consecutive periods, with $M = 0$

corresponding to linear treatment-specific time trends.

In panels (a) and (c) of Appendix Figure A5, we plot the robust 95% confidence sets for the treatment effects in the first year following the reform (β_{2007} in the event study equation (2)), using the "bounding relative magnitudes" method. We follow Rambachan and Roth (2023) to set the upper bound of $\bar{M}$ to be 1, which allows for similar confounding shocks before and after the reform that may contribute to differential trends. We see that the negative treatment effects on SO_2 and COD emissions hold up to $\bar{M} = 1$ and 0.5, respectively. In panels (b) and (d), we plot the results using the "smoothness restrictions" method and follow Biasi and Sarsons (2022) to set the upper bound of M to be equal to the standard error of the coefficient of interest (β_{2007}). We see that the negative treatment effects on SO_2 and COD emissions hold up to M = 172% and 56% of the standard errors of β_{2007}, respectively.[26] These exercises suggest that our findings are robust to a considerable degree of violations of parallel trends.

5.1.3 Non-Targeted Pollutants as Placebo

One unique feature of the 2007 reform is that it did not uniformly target all pollutants; instead, it focused on key pollutants such as SO_2 and COD. This provides us a unique opportunity to test whether our baseline findings are driven by general changes in firms' greenness that differed between treatment and control firms after 2007, by examining the emissions of non-targeted pollutants. Specifically, we use industrial dust and ammonia nitrogen (NH_3-N) as placebo air and water pollutants, respectively. Because these pollutants were not targeted by the reform, we should expect to see null or much smaller effects on these pollutants. We estimate the baseline specification with these two pollutants as outcomes, and report the results in Appendix Table A1. Consistent with our conjecture, the coefficient estimates are small and statistically insignificant. In Appendix Figure 2, we further plot the dynamic effects using an event study specification with leads and lags of the reform. Reassuringly, the coefficient estimates on the leads and lags are small and statistically insignificant.

5.1.4 Covariate Balance and Flexible Trends

We show in Figure 3 that our results are robust to adjusting for potential covariate imbalance and differential trends between treatment and control firms, as discussed in Section 4.2. We plot the baseline estimates from equation (1) in row (1) for reference and the estimates from variants of the baseline specification in the remaining rows

[26] To get these percentages, we first obtain the standard errors of β_{2007} in equation (2) (0.029 for SO_2 and 0.036 for COD). As the "breakdown values" (where the confidence sets include zero) of M are approximately 0.05 for SO_2 (panel (c)) and 0.036 for COD (panel (d)), the percentages can be calculated as follows: $0.05/0.029 \approx 172\%$ and $0.02/0.036 \approx 56\%$.

(2)-(8). Specifically, in row (2), we add more granular 4-digit industry $\times$ year fixed effects and city $\times$ year fixed effects to further filter out unobserved heterogeneity across firms. In row (3), we select the most relevant controls through the Double LASSO method (Belloni, Chernozhukov and Hansen, 2014) to better parametrically adjust for potential covariate imbalance. In row (4), we instead use decile bin fixed effects of $\mathbf{X}_i$ interacted with year fixed effects λ_t to nonparametrically adjust for potential covariate imbalance. In row (5), we reweight observations to achieve covariate balance using the Entropy Balancing method (Hainmueller, 2012). In row (6), we include firm-specific linear time trends to allow for differential linear time trends across firms. To avoid overspecifying our equation, we alternatively try treatment-specific linear time trends in row (7), and further allow for a linear trend break after the reform in row (8) following Alder, Shao and Zilibotti (2016). The estimates from these alternative specifications have no substantial changes compared to the baseline estimates, implying that our findings are unlikely to be confounded by potential covariate imbalance or differential trends.

5.1.5 Exploiting Within-Treatment Variations for Identification

To further relax the parallel trends assumption, we leverage within-treatment variations for identification, as outlined in Section 4.2. Specifically, we leverage firms' differential emission reduction capacity within the same treatment group as an additional source of identifying variations. This enables us to control for all heterogeneity between treatment and control firms by including treatment $\times$ year fixed effects. We sort firms into high and low reduction capacity categories using two criteria: (1) whether pre-reform emissions exceed the sample median, as it would be harder for clean firms to further cut emissions compared to dirty firms. To better capture recent emission trends and minimize short-term fluctuations, we consider emissions over the three years preceding the reform when constructing this indicator; (2) whether a firm was part of a multi-division conglomerate in 2006, as conglomerates are better at resource sharing and risk diversification across internal firms (Stein, 1997, 2003; Maksimovic and Phillips, 2002, 2007; Giroud and Mueller, 2015), allowing these firms to take costly actions to cut emissions.[27] Following Chen et al. (2023*a*), we identify conglomerates using data from China's Administrative Registration Database. We then construct a dummy variable $HighReductionCapacity_i$ indicating firms with high reduction capacity, based on either of the two criteria, and estimate equation (6). As seen from Appendix Table A2, the coefficient estimates on $Treat_i \times Post_t \times HighReductionCapacity_i$ are negative and statistically significant, suggesting that our findings are unlikely

[27] To avoid pollution displacement across internal firms, we only consider conglomerates that contain other non-polluting firms.

driven by differential trends between treatment and control firms.

5.1.6 Addressing Concerns about Spillover and Anticipation

We address two implicit concerns that may bias our DID estimates. First, we examine whether there are any spillover effects between treatment and control firms. To this end, we add to the baseline specification the interaction between $Spillover_i$ and $Post_t$ to capture the spillover effects, where $Spillover_i$ denotes the strength of spillovers from other firms to firm i.[28] Following Avis, Ferraz and Finan (2018), we use the number of other treatment firms in a firm's neighborhood to proxy for $Spillover_i$, where the neighborhood is defined either by the firm's postal code or by a 5 km radius around the firm. As shown in Appendix Table A3, the spillover effects are small and statistically insignificant. In contrast, the direct treatment effects are barely affected. Second, we address the concern regarding the anticipation effect. That is, treatment firms may react to the reform prior to its implementation. The absence of pre-trends in our previous event study plots largely mitigates this concern. In Appendix Table A4, we further repeat our baseline estimation after excluding observations in the two years before the reform, as well as treatment firms that were first treated during this period. The results remain consistent with the baseline estimates.

5.1.7 Additional Robustness Checks

We test the sensitivity of our findings to alternative specifications. In Appendix Table A5, we show that our findings are robust to alternative variable measures. In columns (1)-(2) of panel (a), we use the raw values of SO_2 and COD emissions to alleviate the concern that log-like transformations may be sensitive to variable units (Chen and Roth, 2023). In columns (3)-(4) of panel (a), instead of using total emissions, we use emissions per unit of output to capture the intensity of emissions. In columns (1)-(2) of panel (b), we measure the intensity of the treatment using the average subsidy-to-assets ratio in the pre-reform period. In columns (3)-(4) of panel (b), we maintain the binary nature of the treatment, but redefine it using a dummy variable indicating firms that received subsidies for at least 3 years in the pre-reform period. Our findings remain consistent across these specifications.

In Appendix Figure A6, we show that our findings are robust to alternative inference procedures. In row (1) of panels (a)-(b), we plot the baseline estimates for reference. In rows (2)-(4) of panels (a)-(b), we correct for within-cluster correlation using three alternative clustering levels: at the city level, at the 2-digit industry level, and two-way clustering at both the city and 2-digit industry levels. In rows (5)-(8) of

[28] Specifically, we estimate: $Pollution_{ijt} = \beta^{Direct} Treat_i \times Post_t + \beta^{Spillover} Spillover_i \times Post_t + \mathbf{X}_i \times \lambda_t + \mu_i + \eta_{jt} + \varepsilon_{ijt}$, where β^{Direct} captures the direct effects and $\beta^{Spillover}$ captures the spillover effects.

panels (a)-(b), we correct for spatial correlation using Conley standard errors (Conley, 1999), allowing for correlation of observations within distances of 100 km, 200 km, 400 km, and 800 km.[29] In panels (c)-(d), we perform randomization inference, which is robust to high-leverage observations and complex error structures (Young, 2019).[30] Our coefficient estimates remain highly significant across these procedures.

In Appendix Table A6, we show that our findings are robust to controlling for a variety of other concurrent environmental policies that may differentially affect treatment and control firms. Specifically, we consider: (1) the SO_2 emission regulation under the 1998 Two Control Zones (TCZ) policy (Cai et al., 2016; Chen, Li and Lu, 2018); (2) the air quality regulation for host and neighboring cities during the 2008 Beijing Olympics (Chen et al., 2013; He, Fan and Zhou, 2016); and (3) the water pollution regulation at provincial borders and key waters in the Eleventh Five-Year Plan (Kahn, Li and Zhao, 2015). We control for the confounding effects of these policies by including the distance from each firm to the targeted areas, interacted with year fixed effects. Our findings remain virtually unchanged.

Finally, in Appendix Table A7, we show that our findings are not driven by some marginal firms. We weight the regressions using pre-reform firm size (total assets or sales) and obtain similar estimates. In Appendix Figure A7, we show that our findings are not driven by some particular industries or regions. We repeat our baseline estimation after excluding each industry or province individually and find no large deviations from the baseline estimates.

5.2 Firm Adjustments

Having demonstrated the robustness of our baseline findings, we examine how firms achieved emission reductions in this section. To understand firms' margins of adjustments, on can decompose firm i's emissions e_i into two components: $e_i = \frac{e_i}{y_i} \times y_i$, where $\frac{e_i}{y_i}$ captures emissions per unit of output, or emission intensity, and y_i denotes output (Fan et al., 2019). Then by taking the logarithm and total differential we can decompose the emission reductions as: $\Delta \log e_i = \Delta \log \frac{e_i}{y_i} + \Delta \log y_i$, where $\Delta \log \frac{e_i}{y_i}$ implies a reduction in emission intensity, typically achieved through clean technology adoption (e.g., resource recycling, green innovation, and pollutant treatment), and $\Delta \log y_i$ simply represents output cuts. We examine these margins in Table 3, leveraging detailed information on clean technology adoption and production in our firm-level data.

[29] We also allow for serial correlation across all years when implementing spatial correlation correction, but our findings are insensitive to this.

[30] Following the recommendation of Young (2019), We conduct 10,000 random permutations of the treatment while maintaining the same probability of treatment as in the original sample. We then compare the true coefficient estimates to the coefficient estimates generated by the permutations. Notably, none of the permutations yield coefficient estimates that exceed our true estimates in absolute terms.

In panel (a), we examine various types of clean technology adoption. Columns (1)-(2) examine coal and industrial water use per unit of output, as the consumption of these inputs is typically associated with the emissions of key air and water pollutants.[31] We find that treatment firms experienced a greater reduction in coal and industrial water use per unit of output after the reform than control firms, possibly achieved through resource recycling. Column (3) examines firms' green innovation using the green patent data obtained from the China National Intellectual Property Administration (CNIPA).[32] We find a precisely estimated zero effect, possibly because green innovation usually takes longer time. Column (4) examines firms' installation of pollution abatement facilities. We find that treatment firms installed more pollution abatement facilities after the reform than control firms. In panel (b), we examine firms' adjustments on the production side. As expected, we find a negative impact on output in column (1). We also examine other performance indicators, such as profitability, sales, and total factor productivity (TFP), in the remaining columns.[33] We find similar negative effects on these outcomes, consistent with emission reductions negatively affecting firms' economic performance (Shapiro and Walker, 2018; He, Wang and Zhang, 2020; Liu, Tan and Zhang, 2021).

Overall, the findings in this section reveal a clear pattern: the emission reductions observed in the previous sections were achieved through costly adjustments, such as clean technology adoption and output cuts. We further disentangle the mechanisms underlying these costly actions taken by firms to reduce emissions in the following section.

6 Mechanisms

Our findings so far reveal that firms receiving prior government subsidies made significant efforts to reduce emissions following the 2007 pollution control reform. We interpret this as driven by the reciprocity between firms and the government, where those firms cut emissions to reciprocate the government facing pollution control challenges. We validate this mechanism in two ways: (1) from the firms' perspective, their reciprocal behaviors should be governed by local norms of reciprocity, implying greater emission reductions in areas with stronger such norms; (2) from the government's perspective, if the firms' actions were driven by reciprocity, the government would likely respond with future support, as reciprocity is by definition dynamic. We

[31]Coal combustion is one of the key sources of SO_2 emissions. Industrial water is used in many processes of production, which can lead to higher wastewater discharge and, consequently, higher water pollutant emissions.

[32]We define green patents as those patents aimed for reducing emissions during production (Fan et al., 2019).

[33]We calculate firms' TFP using the Olley and Pakes (1996) method.

also rule out several alternative explanations for our findings

6.1 Heterogeneity in Reductions by Norms of Reciprocity

In this section, we test the reciprocity mechanism from the firms' perspective. As Besley (2020) articulates, in a reciprocal social contract, the obligations between citizens and the state are primarily governed by informal norms of reciprocity rather than formal contracts. This suggests that if treatment firms were reducing emissions to reciprocate the government, we should observe greater emission reductions in areas where the norms of reciprocity between citizens and the state are stronger. Therefore, we can validate this mechanism by examining the heterogeneity in emission reductions across cities with varying norms of reciprocity.

To substantiate the idea, we construct three measures of the norms of reciprocity between citizens and the state at the city level using different survey data. Our first measure is derived from the 2011 China General Social Survey (CGSS), which includes the question: "Are you willing to pay higher taxes to improve the level of universal healthcare?"[34] The answer is one of the following: very willing, somewhat willing, neutral, somewhat unwilling, very unwilling. We create an individual-level dummy variable indicating "very willing" for subsequent aggregation.

Our second measure comes from the 2002 China Private Enterprise Survey (CPES), which asks private entrepreneurs the following question: "what is the most important reason for your donation to charitable causes?" The answer is one of the following: to contribute to society, to reciprocate the government, to repay the elders and villagers, to maintain good relations with the local community, to enhance the reputation of one's own business, actually forced to contribute, or other reasons.[35] We create an individual-level dummy variable indicating "to reciprocate the government" for subsequent aggregation.

Our third measure is based on the 2006 Chinese Social Survey (CSS),[36] which asks: "To what extent do you agree with the saying that even a small favor must be re-

[34]The CGSS is a nationally representative survey launched by Renmin University in China in 2003, aimed at understanding citizen attitudes and behaviors on various social and economic issues. We use the 2011 wave because this question related reciprocity is not present in earlier waves. We believe this is reasonable given the relatively stable nature of cultural norms over short time (Persson and Tabellini, 2021). We also validate our findings using questions from two other surveys available in pre-reform years.

[35]The CPES is a biennial, nationally representative survey launched by multiple research institutions and universities in China in 1993. It aims at understanding various aspects of private enterprises in China, including their development, business environment, entrepreneurial behavior, and the challenges they face. We use the 2002 wave as this is the earliest wave in our pre-reform period that includes this question.

[36]The CSS is a nationally representative survey launched by the Chinese Academy of Social Sciences in 2005. It aims at understanding various aspects of labor and employment, family and social life, and social attitudes among the general public across the country.

paid?" The answer is one of the following: strongly agree, somewhat agree, uncertain, somewhat disagree, strongly disagree. This question can be viewed as a measure of reciprocity towards the general public, including the government. We create an individual-level dummy variable indicating "strongly agree" for subsequent aggregation.

We then aggregate each of these individual-level dummy variables to the city level to construct three city-level measures of reciprocity norms, $Norm_c$, after partialling out individual characteristics, such as age, gender, education, and income, although our results are not sensitive to these characteristics. To facilitate comparisons across different survey questions, we further standardize $Norm_c$ to have a mean of zero and a standard deviation of one. We then include the triple interaction $Treat_i \times Post_t \times Norm_c$ as well as all possible double interactions in our baseline regressions to examine the heterogeneity in emission reductions across cities with varying norms of reciprocity.

Table 4 presents the results. The coefficient estimates on $Treat_i \times Post_t \times Norm_c$ are negative and statistically significant across all three measures, with similar and substantial economic magnitudes, meaning that treatment firms cut more emissions in areas with stronger norms of reciprocity. This finding thus depicts a strong pattern of reciprocity between firms and the government, which likely drove the emission reductions. In the following section, we further bolster this argument from the government's perspective by examining the government's future responses.

6.2 Future Responses of the Government

In this section, we adopt a forward-looking approach to understand the nature of the costly emission reductions identified above by focusing on future responses of the government. If, as we propose, the reductions were driven by treatment firms' reciprocity towards the government for the prior subsidies, we would expect the government to, in turn, respond with additional support in the future. This is because reciprocal behaviors, by definition, imply dynamic interactions (Fehr and Gächter, 2000; Sobel, 2005). Therefore, we can test the reciprocity mechanism by examining whether treatment firms' emission reductions are positively associated with future government subsidies.

To examine future government subsidies, we use an extended period of the Annual Survey of Industrial firms (ASIF) data covering 2001-2014.[37] However, this data lacks records on government subsidies for 2008-2010, so we supplement it with another comprehensive firm-level dataset: the National Tax Survey Database (NTSD).[38]

[37] We stop at 2014 as this is the latest year for which the ASIF is available to us.

[38] The NTSD is jointly collected by the State Administration of Taxation and the Ministry of Finance of China. It contains detailed information on firms' taxation, financing, operations, and performance, and has been widely used to study firm behaviors (Liu and Mao, 2019; Chen et al., 2023*c*). The dataset has a

By matching the two datasets via common firm identifiers, we establish records of government subsidies from 2001 to 2014.[39] We then define firms' emission reductions as follows:

$$Reduction_i = 0.5 \times \frac{\overline{SO2_{01-06}} - \overline{SO2_{07-10}}}{\overline{SO2_{01-06}}} + 0.5 \times \frac{\overline{COD_{01-06}} - \overline{COD_{07-10}}}{\overline{COD_{01-06}}} \quad (7)$$

where $\overline{SO2_{01-06}}$ and $\overline{COD_{01-06}}$ respectively denote average SO_2 and COD emissions in the pre-reform period (2001-2006); $\overline{SO2_{07-10}}$ and $\overline{COD_{07-10}}$ respectively denote average SO_2 and COD emissions in the post-reform period (2007-2010). Thus, $Reduction_i$ represents the equally weighted average of the fractional reductions in SO_2 and COD emissions. We use this measure as our benchmark and assess robustness using two alternative measures.[40] We then examine the relationship between treatment firms' emission reductions and future subsidies by estimating the following equation:

$$\begin{aligned} Subsidy_{ijt} = {} & \beta_1 Treat_i \times Reduction_i \times Period_{07-10} \\ & + \beta_2 Treat_i \times Reduction_i \times Period_{11-14} \\ & + \gamma_1 Reduction_i \times Period_{07-10} + \gamma_2 Reduction_i \times Period_{11-14} \\ & + \delta_1 Treat_i \times Period_{07-10} + \delta_2 Treat_i \times Period_{11-14} \\ & + \mathbf{X}_i \times \lambda_t + \mu_i + \eta_{jt} + \varepsilon_{ijt} \end{aligned} \quad (8)$$

where $Subsidy_{ijt}$ denotes measures of government subsidies, either in terms of incidence (presence of subsidies) or in terms of intensity (amount of subsidies). To understand the timing of the government's responses, we create two time period indicators $Period_{07-10}$ and $Period_{11-14}$, which denote the post-reform period (2007-2010) and the future period (2011-2014), respectively. The coefficients of interest are β_1 and β_2, which capture the differential subsidies received by treatment firms with varying emission reductions in different periods.

Table 5 presents the results. Columns (1) and (4) adopt our baseline measure of emission reductions. Two patterns are noteworthy. First, the government did not respond simultaneously to treatment firms' emission reductions, which makes sense as the pollution control target was only fulfilled in 2010. This finding also alleviates the concern that treatment firms' emission reductions were sustained by additional subsidies received in the same period. Second, the government responded by providing additional subsidies in the future period (2011-2014) to reward treatment firms' previ-

large sample of approximately 700,000 firms of various sizes, covering a full range of sectors (including agriculture, mining, manufacturing, building, and services) and areas in China.

[39]The matching rates of the ASIF with the NTSD in 2008, 2009, and 2010 are 74%, 87%, and 86%, respectively.

[40]To facilitate comparisons across different measures, we further standardize these measures to have a mean of zero and a standard deviation of one in the regressions.

ous emission reductions, consistent with the reciprocity interpretation of the emission reductions. In the remaining columns, we adopt two alternative measures of emission reductions by altering the way we normalize the reduction levels for robustness, and find similar results.[41] In Appendix Table A8, we further address the concern that $Reduction_i$ could be endogenous by replacing it with exogenous reduction targets at the city level, which we extract from government annual work reports.[42] The results remain consistent.

To facilitate visual exploration of the timing of the government's responses, we estimate the following event study specification:

$$\begin{aligned} Subsidy_{ijt} = & \sum_{k=2001,\ k\neq 2006}^{k=2014} \beta_k Treat_i \times Reduction_i \times Year_k \\ & + \sum_{k=2001,\ k\neq 2006}^{k=2014} \gamma_j Reduction_i \times Year_k \\ & + \sum_{k=2001,\ k\neq 2006}^{k=2014} \delta_k Treat_i \times Year_k + \mathbf{X}_i \times \lambda_t + \mu_i + \eta_{jt} + \varepsilon_{ijt} \end{aligned} \tag{9}$$

where β_k is the coefficient of interest, capturing the differential subsidies received by treatment firms with varying emission reductions across years. Figure 4 plots the estimates for β_k, which remain small and statistically insignificant until 2010 when the pollution control target was fulfilled. The estimates turn positive and statistically significant after 2010, suggesting that the government's responses exactly coincided with the fulfillment of the pollution control target. These patterns are in line with a dynamic and mutually beneficial interaction between firms and the government, reinforcing the reciprocity mechanism.

Overall, by documenting the government' benevolent responses in the future, combined with our previous findings on the heterogeneity in reductions across areas with varying norms of reciprocity, we provide compelling evidence that treatment firms' emission reductions were likely driven by their reciprocity towards the government.[43]

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[41]Specifically, in columns (2) and (5) of Table 5, we use the emissions in 2006 to normalize the reduction levels by defining $Reduction_i$ as: $0.5 \times \left(\overline{SO2_{06}} - \overline{SO2_{07-10}}\right) / \overline{SO2_{06}} + 0.5 \times \left(\overline{COD_{06}} - \overline{COD_{07-10}}\right) / \overline{COD_{06}}$. In columns (3) and (6) of Table 5, we use the reduction levels without any normalization by defining $Reduction_i$ as: $0.5 \times \left(\overline{SO2_{01-06}} - \overline{SO2_{07-10}}\right) + 0.5 \times \left(\overline{COD_{01-06}} - \overline{COD_{07-10}}\right)$.

[42]These reports are drafted by city-level governments and issued at the beginning of each year, containing detailed and well-structured development policies. They have been widely used to examine local governments' policies priorities (Jiang, Meng and Zhang, 2019). We collect these reports from the official government websites of each city and create a dummy variable $ReductionTarget_c$ indicating cities with a specific reduction target in their 2008 reports. We then replace $Reduction_i$ with $Reduction_i$ and re-estimate equation (8).

[43]One alternative explanation for future government subsidies could be that firms cut emissions purely for material gains if they anticipated more future subsidies. To address this concern, we con-

In the following sections, we address several alternative explanations for our baseline findings.

6.3 Alternative Explanations

In this section, we examine several alternative explanations that may generate observationally equivalent results as our baseline findings, including financial constraints, regulatory enforcement, and political connections. We show that these explanations do not drive our baseline findings.

6.3.1 Financial Constraints

Given the substantial costs of emission abatement as we show earlier, one possible explanation for treatment firms' additional emission reductions could be attributed to prior government subsidies that may reduce their financial constraints, an important factor affecting firms' environmental decisions (Levine et al., 2018; Xu and Kim, 2022; Aghion et al., 2024). While this explanation is plausible on its own, we believe it is less likely to drive our findings considering the minimal amount of the subsidies. As we show in Appendix Figure A4, prior government subsidies accounted for less than 1.3% of firm sales on average. To further rule out this possibility, we directly include firms' financial constraints at the end of 2006, interacted with year fixed effects, in our baseline regressions. Drawing on the literature on financial constraints (Kaplan and Zingales, 1997; Whited and Wu, 2006; Manova and Yu, 2016) and considering data availability in our ASIF dataset, we construct three measures of financial constraints: (1) cash flow, defined as operating cash flow over total assets; (2) liquidity, defined as current assets minus current liabilities over total assets; and (3) leverage, defined as current liabilities over current assets. The first two are negatively related to financial constraints and the last one is positively related to financial constraints. As shown in Appendix Table A9, our results remain virtually unchanged when flexibly controlling for financial constraints.

duct a simple cost-benefit analysis as follows: Table 3 shows that, on average, the cost of emission reductions for treatment firms is 18.7% of pre-reform profits. Table 5 shows that the additional future subsidies associated with a one standard deviation increase in emission reductions amount to 38.8% of pre-reform subsidies, which further translates into 38.8% $\times$ 8.6% = 3.3% of pre-reform profits (the average pre-reform subsidy-to-profit ratio is 8.6%). Thus, to compensate for the cost of emission reductions, a firm would need to increase emission reductions by more than 5 standard deviations, which is nearly impossible. This suggests that the cost of firms' emission reductions generally far exceeds the additional future subsidies received. This disparity between the cost and benefit also aligns precisely with the definition of reciprocity, which involves mutual exchanges that may not necessarily be equal (Fehr and Gächter, 2000; Sobel, 2005).

6.3.2 Regulatory Enforcement

The greater emission reductions observed among treatment firms could also result from enhanced regulatory enforcement independent of any reciprocal behaviors towards the government. Specifically, local governments may choose to target these subsidized firms for regulatory enforcement if they felt more familiar with these firms or if doing so could lower the cost of environmental regulations on firms. To rule out this possibility, we test whether treatment firms faced greater regulatory enforcement following the reform. Following Kong and Liu (2024) and Axbard and Deng (2024), we use administrative data on environmental punishment from local Environmental Protection Bureaus as measures of regulatory enforcement. The data contain detailed records on each punishment, including date, location, firm name, punishment type, amount of fines, and reason for punishment. We aggregate these records to the firm-year level and match them with our main data based on firm name and year. We then create four measures of regulatory enforcement: (1) number of punishment; (2) presence of punishment; (3) suspension of production; and (4) amount of fines.[44] As shown in Appendix Table A10, the coefficient estimates on $Treat_i \times Post_t$ are consistently small and statistically insignificant across all four measures, suggesting that treatment firms did not experience differential regulatory enforcement following the reform.

6.3.3 Political Connections

A third alternative explanation could be that treatment firms were also politically connected, which reduced emissions to cater to politicians' needs or to gain future economic benefits.[45] While we lack direct measures of political connections in our ASIF dataset, we note that treatment firms accounted for more than 30% of all firms, contradicting the notion that political connections are typically concentrated in a small set of firms (Shleifer and Vishny, 1993; Faccio, 2006; Akcigit, Baslandze and Lotti, 2023). We then adopt two strategies to further rule out this possibility. First, we focus on the public firms in our sample,[46] for which we can construct two measures of their political connections in 2006 following the literature: (1) the fraction of politically connected board directors (Giannetti, Liao and Yu, 2015);[47] and (2) entertainment and travel cost

[44] In China, suspension of production is often viewed as more severe punishment for firms than fines (Axbard and Deng, 2024).

[45] We want to explicitly distinguish this from our reciprocity argument, not only because this is a different mechanism but also because it often distorts allocative efficiency (Goldman, Rocholl and So, 2013; Schoenherr, 2019), generating social costs that may outweigh the benefits of emission reductions.

[46] These firms accounts for 2% of all firms. Their pre-reform emissions of SO_2 and COD represent approximately 10.4% and 8.1% of all firms' pre-reform SO_2 and COD emissions, respectively.

[47] Specifically, we define board directors as politically connected if they are current or former government officials.

(ETC) (Cai, Fang and Xu, 2011; Fang et al., 2022).[48] As shown in Appendix Table A11, treatment firms' emission reductions have no substantial changes when we flexibly control for the potential impacts of political connections. To address the concern that the publicly listed firms may not be representative of the full sample, we also examine whether the emission reductions were lower in areas with political turnovers just prior to the reform, as these turnovers would disrupt local firms' political connections (Fang et al., 2022). We focus on the turnovers of the top two leaders (party secretaries and mayors),[49] and count the number of turnovers in 2006, excluding cases where the same person moved from one position to another. As shown in Appendix Table A12, our baseline findings on emission reductions are unaffected by political turnovers.

7 Aggregate Impacts

Having established the role of reciprocity between firms and governments in reducing pollutant emissions at the firm level, we now turn to its broader implications for regional pollution concentrations. To this end, we conduct a city-level analysis by aggregating the treatment to the city level and gathering new data on city-level pollution concentrations. This aggregate analysis is imperative, as firm-level findings alone may not adequately capture regional pollution dynamics, if reductions at the firm-level were offset by intra-city pollution displacement or the contribution of treatment firms to regional pollution was minimal. Reassuringly, our analysis reveals that cities with higher fractions of treatment firms witnessed greater reductions in SO_2 and COD concentrations, thereby demonstrating the role of reciprocity in strengthening local governments' capacity for pollution control on a broader scale.

7.1 Air Pollution

We use satellite observations to measure city-level SO_2 concentrations due to the lack of ground-based data in our study period (Zhang, Chen and Guo, 2018).[50] We collect the data from the National Aeronautics and Space Administration (NASA), which records monthly SO_2 concentrations at the $0.5° \times 0.625°$ (around 50 km $\times$ 60 km) grid level since 1980.[51] We then aggregate the data to the city $\times$ year level and

[48] A firm's entertainment and travel costs encompass expenses for dining, gifts, travel, and other activities, which are widely used to measure relationship building with local officials (Cai, Fang and Xu, 2011; Fang et al., 2022).

[49] Local firms may have connections with other lower-level officials for whom we do not have data. But as the top two leaders possess personnel control over subordinate officials, our measure can also be viewed as negative shocks to connections with lower-level officials.

[50] Before 2013, ground-based monitoring station readings for specific pollutants such as SO_2 were unavailable; only an air pollution index (API) for some major cities was reported.

[51] Specifically, we use the product M2TMNXAER in the Modern-Era Retrospective Analysis for Research and Applications version 2 (MERRA-2) released by the NASA, which can be found at

estimate the following specification:

$$\log(SO2_{cpt}) = \beta Treat_c \times Post_t + \lambda_c + \delta_{pt} + \varepsilon_{cpt} \quad (10)$$

where c, p, and t denote city, province, and year, respectively. $SO2_{cpt}$ denotes SO_2 concentrations in city c and year t. $Treat_c$ is the average firm-level treatment in city c. We use the simple average as our benchmark and assess robustness using firm size-weighted average. As shown in panel (a) of Figure 5, $Treat_c$ varies substantially across cities, with a mean of 0.35. $Post_t$ indicates years after the 2007 reform. We include city fixed effects λ_c and province $\times$ year fixed effects δ_{pt} to exploit only within city variation net of provincial shocks for identification. We also include in some specifications city-level economic controls (GDP per capita, fiscal revenue, population, and industrial production) and flexible weather controls (temperature, precipitation, wind direction, wind speed, and dew point temperature) (Deschênes and Greenstone, 2011).[52] Standard errors are clustered at the city level to account for within-city correlation.

The results are presented in columns (1)-(3) of Table 6. Column (1) employs the baseline specification without any controls, while columns (2)-(3) gradually add the economic and weather controls. The estimates are negative, statistically significant, and highly stable across these columns, indicating that cities with higher fractions of treatment firms experienced greater reductions in SO_2 concentrations following the reform. The event study plot in panel (a) of Figure 6 further confirms this reduction pattern and shows no pre-trends prior to the reform. To gauge the economic magnitude, consider the estimate in column (3) (coef.=-0.088, s.e.=0.027). As the mean of $Treat_c$ is 0.35, this estimate indicates that moving from a city with no treatment firms to the average city would result in a 3.1% ($\approx 0.088 \times 0.35$) greater reduction in SO_2 concentrations following the reform.

Robustness—We conduct several tests to check the robustness of our results. First, we conduct a placebo test using ozone (O_3) concentrations collected from the Copernicus Climate Change Service, which was not targeted by the reform, as a placebo outcome.[53] As shown in column (1) of Appendix Table A13 and panel (b) of Figure

https://disc.gsfc.nasa.gov/datasets/M2TMNXAER_5.12.4summary?keywords=SO2

[52]The economic controls are collected from the City Statistical Yearbooks. To avoid bad control issues, we include their pre-reform averages interacted with year fixed effects. The weather controls are constructed using the daily weather station readings from the National Oceanic and Atmospheric Administration (NOAA). To aggregate the station readings to the city level, we use the nearest station readings within 100 km of the city centroid for wind speed and wind direction, and the inverse-distance weighted average of station readings within 200 km of the city centroid for other weather variables (Deschênes and Greenstone, 2011). To further allow for nonlinear weather effects (Deschênes and Greenstone, 2011), we do not directly include these weather controls; rather, we control for the share of days in a year falling into each of the 10 quantiles derived from the overall daily weather distribution, except for precipitation. As the distribution of daily precipitation is highly right-skewed, we simply use its annual average.

[53]See https://cds.climate.copernicus.eu/cdsapp#!/dataset/satellite-ozone-v1?tab=overview for this

6, the impact on this placebo outcome is small and statistically insignificant. Second, we use industrial SO_2 emissions collected from the City Statistical Yearbooks as an alternative outcome measure, which is the total emissions from all firms in a city, including those outside our firm sample. This is a major source of atmospheric SO_2 and a key indicator for assessing whether local governments met emission reduction targets (Chen, Li and Lu, 2018).[54] The results are shown in column (2) of Appendix Table A13 and panel (c) of Figure 6. The patterns are similar to those found in our baseline specification. Third, we weight the firm-level treatment by pre-reform firm size to account for the varying contributions of firms to city-level pollution concentrations. The results are shown in column (3) of Appendix Table A13 and panel (c) of Figure 6, which are also similar to those found in our baseline specification. Overall, these estimates consistently suggest that the reciprocal relationships between firms and the government significantly improved local air quality.

7.2 Water Pollution

To measure city-level COD concentrations, we collect data on surface water quality at the monitoring station level from the China Environmental Yearbooks. The data includes approximately 500 monitoring stations from 2004 to 2010. Panel (b) of Figure 5 shows the spatial distribution of these stations. Given that this distribution can be quite sparse in some areas, we compute city-level COD concentrations utilizing the COD readings from all downstream stations within 100 km of each city. This approach is based on the premise that water pollutants generated by a city are likely to affect only downstream station readings, considering that water flows from higher to lower elevations (He, Wang and Zhang, 2020; Dias, Rocha and Soares, 2023). We then estimate the following specification similar to equation (10):

$$\log(COD_{cpt}^{Downstream,\ <100\ km}) = \beta Treat_c \times Post_t + \lambda_c + \delta_{pt} + \varepsilon_{cpt} \tag{11}$$

where c, p, and t denote city, province, and year, respectively. $COD_{cpt}^{Downstream,\ <100\ km}$ represents the average COD readings in year t from stations that are both downstream and within 100 km of the centroid of city c, weighted by the inverse of the distance from each station to the centroid of city c. We apply the inverse distance weights as stations closer to the city are more likely to be influenced by the city's polluting activities, but our results remain robust without such weights. $Treat_c$ denotes the average firm-level treatment in city c. $Post_t$ indicates years after the 2007 reform. We

data. It reports monthly ozone concentrations at the 0.5° × 0.5° (around 50 km × 50 km) grid level, which we aggregate to the city × year level.

[54]China's industrial SO_2 emissions accounted for more than 80% of total SO_2 emissions during our study period, with the remainder coming from residential sources See https://www.mee.gov.cn/gkml/sthjbgw/qt/200910/t20091031_180759.htm

include city fixed effects λ_c and province $\times$ year fixed effects δ_{pt} to strengthen identification. We also include city-level economic controls (GDP per capita, fiscal revenue, population, and industrial production) and weather controls (temperature, precipitation, wind direction, wind speed, and dew point temperature) in some specifications, which are constructed analogously to those in our previous analysis of air pollution. Standard errors are clustered at the city level to account for within-city correlation.

The results are presented in columns (4)-(6) of Table 6, with economic and weather controls added gradually. In line with our previous findings on air pollution, the estimates are negative, statistically significant, and highly stable across specifications, indicating that cities with higher fractions of treatment firms experienced greater reductions in COD concentrations following the reform. The event study plot in panel (a) of Figure 7, albeit with less precision due to smaller sample size,[55] further confirms this reduction pattern and rules out pre-trends concerns. To gauge the economic magnitude, consider the estimate in column (6) (coef.=-0.617, s.e.=0.284). As the mean of $Treat_c$ is 0.35, this estimate indicates that moving from a city with no treatment firms to the average city would result in a 21.6% ($\approx 0.617 \times 0.35$) greater reduction in COD concentrations following the reform.[56]

Robustness—We conduct several tests to check the robustness of our results. First, we construct a placebo outcome $COD_{cpt}^{Upstream,\ <100\ km}$ using the average COD readings from upstream stations, since water pollutants are unlikely to flow to these stations with higher elevations. As shown in column (4) of Appendix Table A13 and panel (b) of Figure 7, the impact on this placebo outcome is small and statistically insignificant. Second, we remove the inverse distance weight when calculating average COD readings. The results are shown in column (5) of Appendix Table A13 and panel (c) of Figure 7. The patterns are similar to those found in the baseline specification. Third, we weight the firm-level treatment by pre-reform firm size to account for heterogeneity in firms' contribution to city-level pollution concentrations. The results are shown in column (6) of Appendix Table A13 and panel (c) of Figure 6, which are also similar to those found in the baseline specification. In sum, these estimates consistently suggest that the reciprocal relationships between firms and the government also significantly improved local water quality.

[55] This is because the distribution of monitoring stations in Northwest China was highly sparse in our study period.

[56] The significantly larger impact on city-level COD concentrations may seem puzzling given the similar effects on firm-level emissions, but can be well reconciled with the following facts: first, water pollutants tend to be more localized, whereas air pollutants are more dispersible; second, air generally has a faster self-purification rate compared to water bodies. Hence, even with similar pollution reductions at the firm level, the decrease in city-level water pollution concentrations might be much more pronounced.

8 Concluding Remarks

Pollution is a critical global concern, with countries making significant but uneven progress in combating it over the past decades. On one end of the spectrum, advanced countries like the United States have made considerable strides in reducing pollution through stringent regulations (Shapiro and Walker, 2018); on the other end, developing countries such as Brazil, India, and even China have struggled with pollution control due to limited regulatory capacity (Jayachandran, 2022), contributing to the vast majority of pollution-related mortalities (World Bank, 2022*b*). This stark disparity highlights the need for alternative approaches to environmental governance in weak states.

Building on a notion of reciprocity that originally evolved within families and societies, we explore whether the state could leverage reciprocal obligations with citizens to reduce pollution, in the context of China's 2007 national pollution control reform. We establish that firms benefiting from prior government support indeed undertook costly efforts to cut emissions in response to the reform. We corroborate the reciprocity mechanism by showing that the emission reductions were more pronounced in regions with stronger norms of reciprocity, and that the government, in turn, reciprocated with additional support in the future. Aggregately, such reciprocity translated into substantial declines in regional pollution concentrations.

Our findings have significant implications for pollution control globally. By creating and fostering a norm of reciprocity with its citizens, the state can compensate for insufficient regulatory capacity and economize on enforcement costs. However, we also note that this follows a dynamic process and that there could be critical junctures reversing the virtuous circle. Therefore, maintaining the cohesiveness of institutions that effectively constrain elite power is also crucial (Besley, 2020; Persson and Tabellini, 2021; Bisin and Verdier, 2024).

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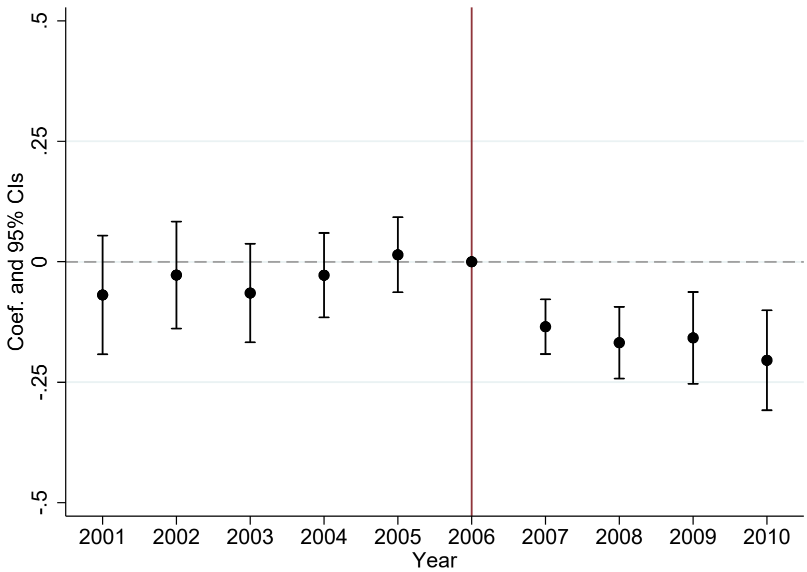
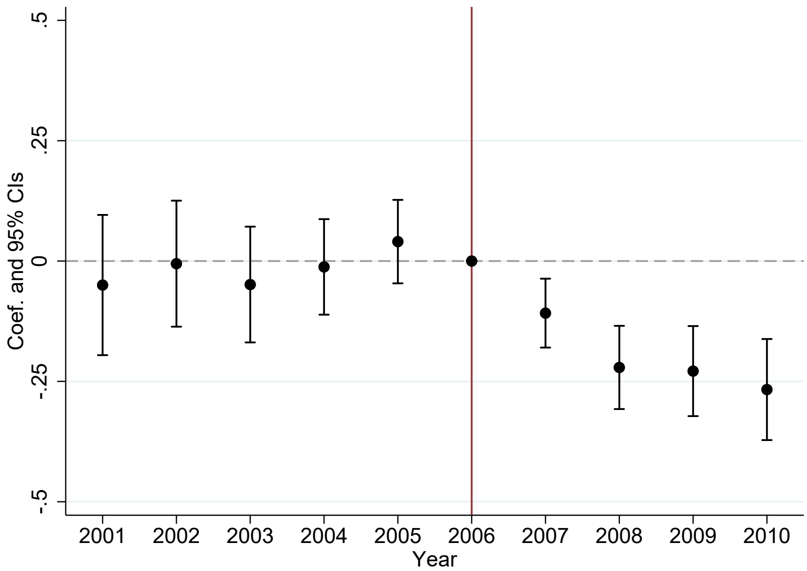
Figures and Tables

Figure 1: Dynamics of Firm-Level Emissions

(a) SO_2 Emissions



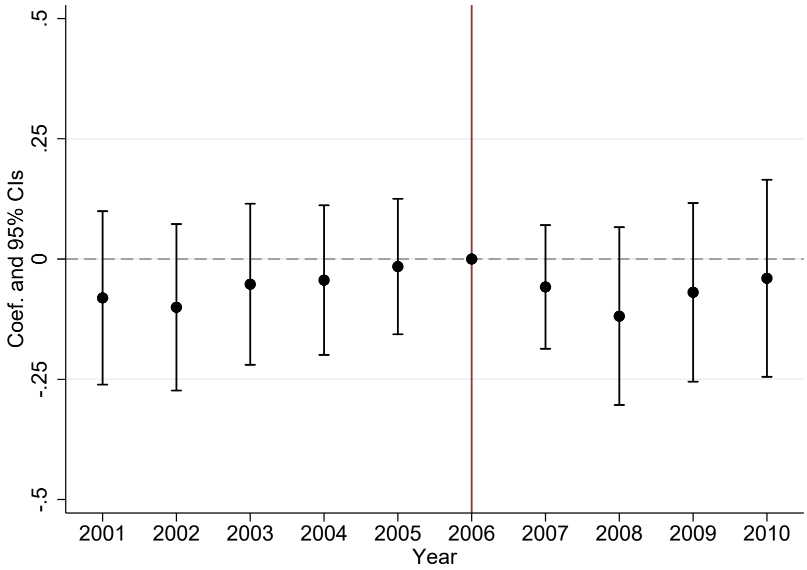
(b) COD Emissions



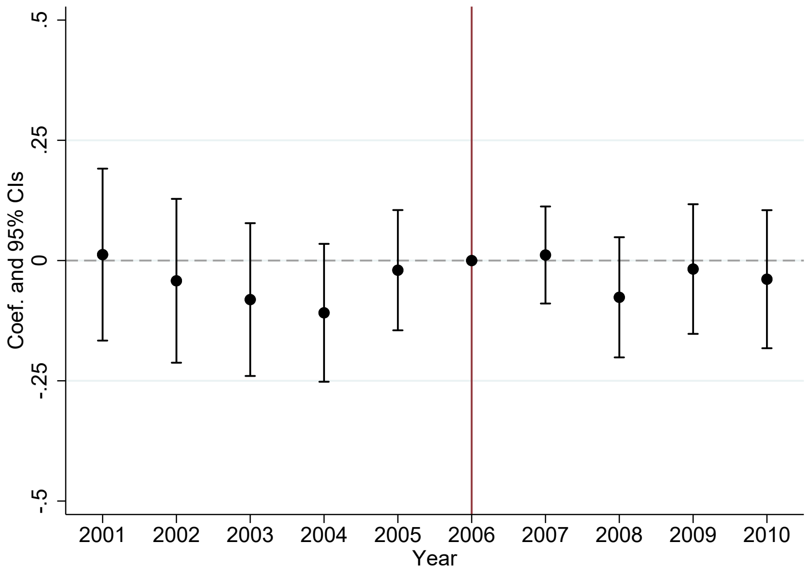
Notes: The figure compares the emissions of key pollutants (SO_2 and COD) between treatment and control firms over years using an event study specification (equation (2)). The year prior to the national pollution control reform in 2007 is omitted as the reference year. Treatment firms are firms that received any subsidies in the pre-reform period (2001-2006) and control firms are those that did not. SO_2 and COD denote the emissions of sulfur dioxide and chemical oxygen demand, respectively, and are transformed using the inverse hyperbolic sine (IHS) function to deal with zero values. The standard errors used to construct the 95% confidence intervals, indicated by the spikes, are clustered at the firm level.

Figure 2: Non-Targeted Pollutants as Placebo

(a) Dust Emissions



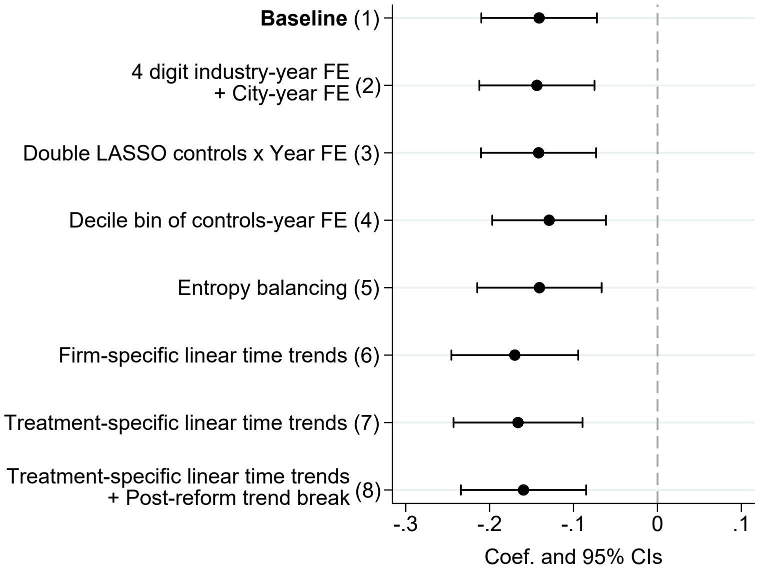
(b) NH_3-N Emissions



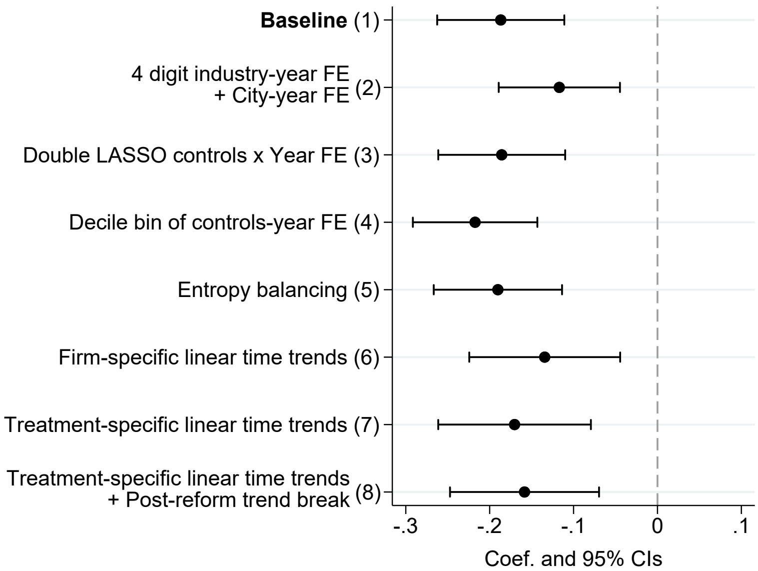
Notes: The figure compares the emissions of non-targeted pollutants (Dust and NH_3-N) between treatment and control firms over years using an event study specification (equation (2)). The year prior to the national pollution control reform in 2007 is omitted as the reference year. Treatment firms are firms that received any subsidies in the pre-reform period (2001-2006) and control firms are those that did not. Dust and NH_3-N denote the emissions of industrial dust and ammonia nitrogen, respectively, and are transformed using the inverse hyperbolic sine (IHS) function to deal with zero values. The standard errors used to construct the 95% confidence intervals, indicated by the spikes, are clustered at the firm level.

Figure 3: Robustness to Covariate Balancing and Flexible Trends Adjustment

(a) SO_2 Emissions



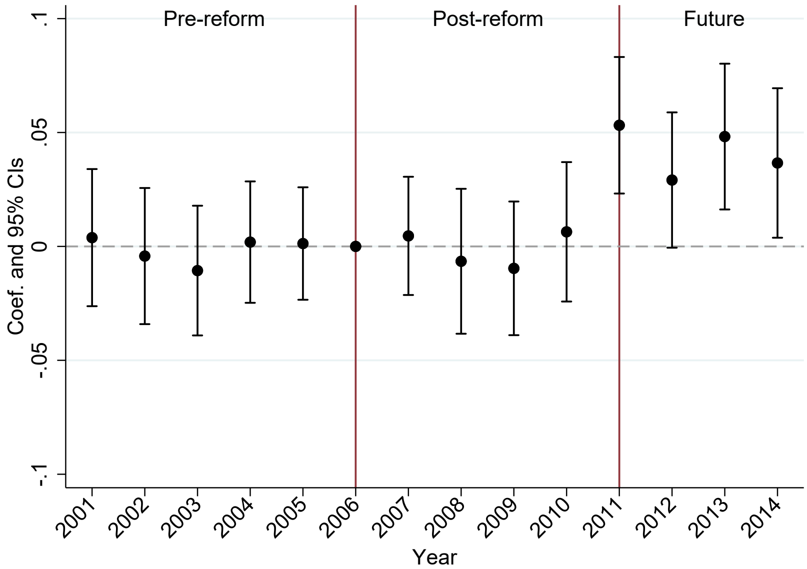
(b) COD Emissions



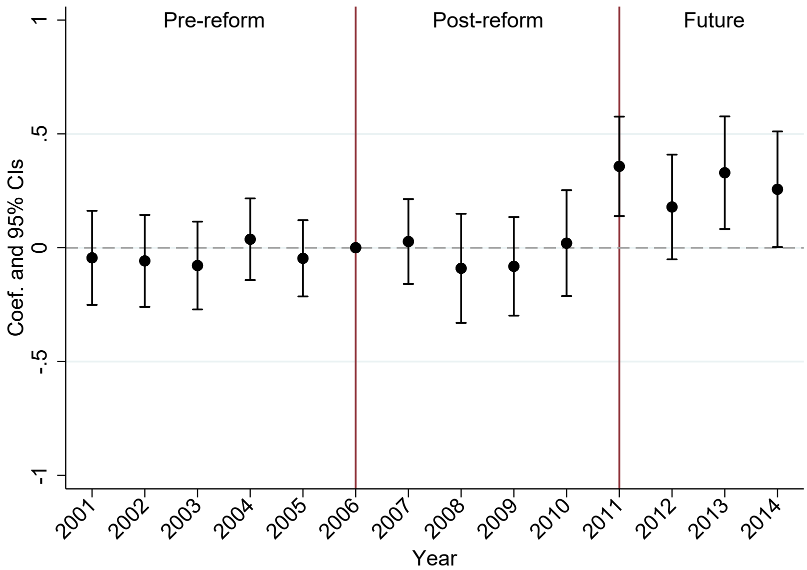
Notes: This figure shows the robustness of our baseline results to covariate balancing and flexible trends adjustment. SO_2 and COD denote the emissions of sulfur dioxide and chemical oxygen demand, respectively, and are transformed using the inverse hyperbolic sine (IHS) function to deal with zero values. Row (1) shows the baseline estimates for reference. Rows (2)-(8) shows the estimates from variants of the baseline specification. Specifically, row (2) adds more granular 4-digit industry $\times$ year fixed effects and city $\times$ year fixed effects. Row (3) selects the most relevant controls through the Double LASSO method. Row (4) uses decile bin fixed effects of covariates interacted with year fixed effects to nonparametrically adjust for potential covariate imbalance. Row (5) reweights observations to achieve covariate balance using the Entropy Balancing method. Row (6) includes firm-specific linear time trends to allow for differential linear time trends across firms. Row (7) instead uses treatment-specific linear time trends. Row (8) further allows for a linear trend break after the reform on the basis of row (7). The standard errors used to construct the 95% confidence intervals, indicated by the spikes, are clustered at the firm level.

Figure 4: Emission Reductions and Future Subsidies

(a) Subsidy Incidence



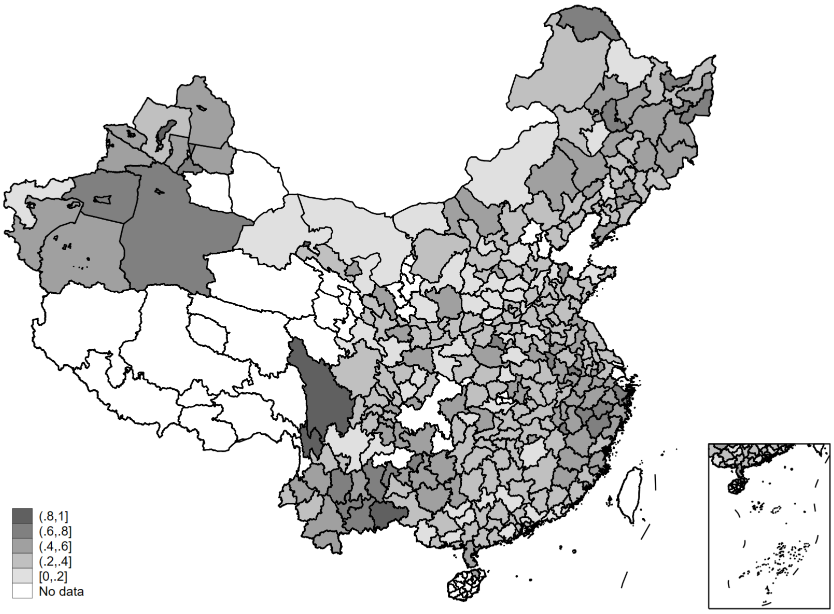
(b) Subsidy Amount



Notes: The figure compares the subsidies received between treatment and control firms with varying emission reductions over years using an event study specification (equation (9)). The year prior to the national pollution control reform in 2007 is omitted as the reference year. Treatment firms are firms that received any subsidies in the pre-reform period (2001-2006) and control firms are those that did not. We define emission reductions as the average of the fractional reductions in SO_2 and COD emissions in the post-reform period relative to pre-reform period. See equation (8) for details. Subsidy incidence is a dummy variable equal to 1 for positive subsidies and 0 otherwise. Subsidy amount is the inverse hyperbolic sine (IHS) transformation of firms' subsidies. The standard errors used to construct the 95% confidence intervals, indicated by the spikes, are clustered at the firm level.

Figure 5: City-Level Treatment and Water Quality Monitoring Stations

(a) City-Level Treatment



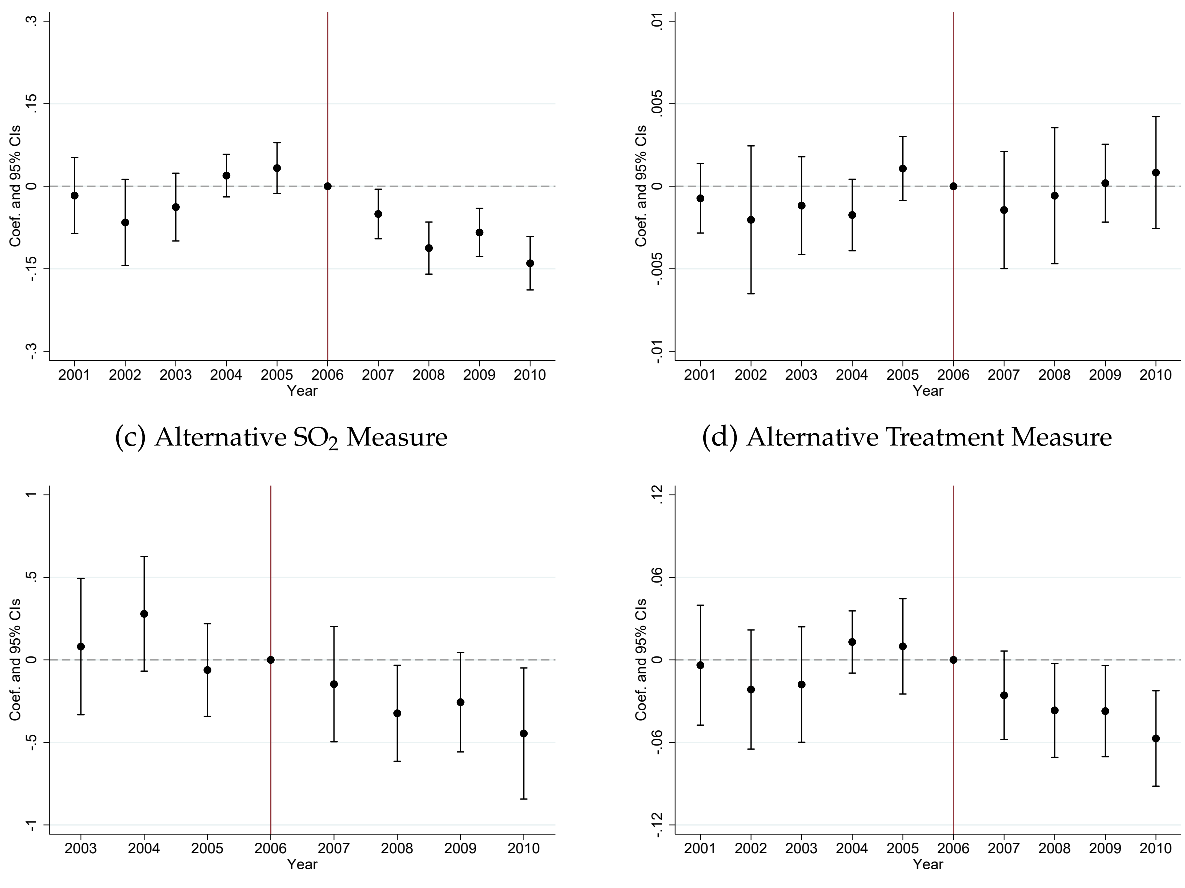
(b) Water Quality Monitoring Stations

Notes: Panel (a) shows the distribution of city-level average of firm-level treatment, with the latter being a dummy variable equal to 1 if firms received any subsidies in the pre-reform period (2001-2006), and 0 otherwise. Panel (b) shows the distribution of the water quality monitoring stations used to construct city-level COD (chemical oxygen demand) concentrations.

Figure 6: Dynamics of City-Level SO_2 Concentrations

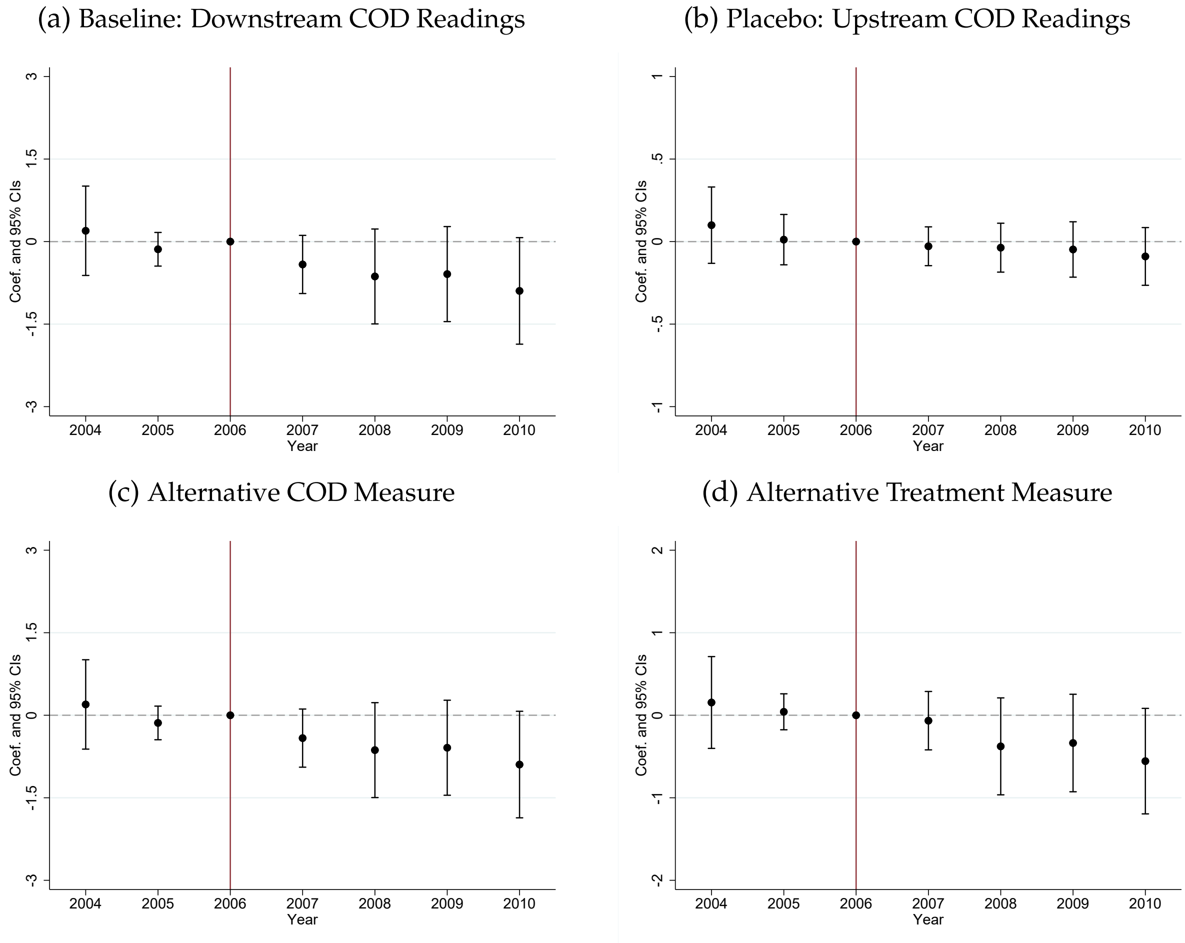
(a) Baseline (b) Placebo: O_3 Concentrations

(c) Alternative SO_2 Measure (d) Alternative Treatment Measure



Notes: The figure compares city-level SO_2 (sulfur dioxide) concentrations across cities with varying fractions of treatment firms over years using an event study variant of equation (10). The year prior to the national pollution control reform in 2007 is omitted as the reference year. Treatment firms are firms that received any subsidies in the pre-reform period (2001-2006). In panel (a), the outcome variable is the logarithm of city-level SO_2 concentrations derived from satellite observations. In panel (b), the outcome is the logarithm of O_3 (ozone) concentrations derived from satellite observations, which is used as a placebo outcome as it was not targeted by the reform. In panel (c), the outcome is the logarithm of industrial SO_2 emissions calculated by summing up emissions from all firms in a city, including those outside our firm sample. In panel (d), the outcome is still the logarithm of SO_2 concentrations derived from satellite observations, but when we compare cities with varying fractions of treatment firms, we weight the fractions using pre-reform firm size. The standard errors used to construct the 95% confidence intervals, indicated by the spikes, are clustered at the city level.

Figure 7: Dynamics of City-Level COD Concentrations



(a) Baseline: Downstream COD Readings

(b) Placebo: Upstream COD Readings

(c) Alternative COD Measure

(d) Alternative Treatment Measure

Notes: The figure compares city-level COD (chemical oxygen demand) concentrations across cities with varying fractions of treatment firms over years using an event study variant of equation (10). The year prior to the national pollution control reform in 2007 is omitted as the reference year. Treatment firms are firms that received any subsidies in the pre-reform period (2001-2006). In panel (a), the outcome variable is the inverse distance-weighted average COD readings from monitoring stations downstream and within 100 km of a city (as water pollutants only flow from high to low). In panel (b), the outcome is the inverse distance-weighted average COD readings from monitoring stations upstream and within 100 km of a city, which is used as a placebo outcome as water pollutants cannot flow from low to high. In panel (c), the outcome is the simple average COD readings from monitoring stations downstream and within 100 km of a city. In panel (d), the outcome is still the inverse distance-weighted average COD readings from monitoring stations downstream and within 100 km of a city, but when we compare cities with varying fractions of treatment firms, we weight the fractions using pre-reform firm size. The standard errors used to construct the 95% confidence intervals, indicated by the spikes, are clustered at the city level.

Table 1: Summary Statistics

Variable	Obs	Mean	Stdev	P25	P50	P75
SO_2	143,524	9.734	3.623	8.987	10.541	11.845
COD	143,524	8.525	3.724	7.155	9.245	10.985
Treat	143,524	0.387	0.487	0	0	1
Size	143,524	10.646	1.553	9.512	10.496	11.62
Age	143,524	2.025	1.081	1.099	1.946	2.773
Patents	143,524	0.076	0.393	0	0	0
Profitability	143,524	0.042	0.201	0	0.017	0.062
Sales	143,524	10.589	1.403	9.538	10.446	11.468
Exports	143,524	3.195	4.930	0	0	8.937
SOE	143,524	0.175	0.380	0	0	0
Employees	143,524	5.585	1.198	4.754	5.489	6.315
Capital	143,524	9.532	1.758	8.395	9.474	10.638
TFP	143,524	8.668	1.380	7.767	8.595	9.536

Notes: This table shows the summary statistics for the main variables used in our baseline firm-level analysis. The sample period is 2001-2010. SO_2 and COD denote the emissions of sulfur dioxide and chemical oxygen demand, respectively, and are transformed using the inverse hyperbolic sine (IHS) function to address zero values. Treat is a dummy variable equal to 1 if firms received any subsidies in the pre-reform period (2001-2006), and 0 otherwise. The remaining variables are firm-level covariates measured in the initial year of our sample. Specifically, size is the logarithm of firms' total assets. Age is the logarithm of firm age. Patents is the IHS transformation of firms' patents. Profitability is firms' profits normalized by total assets. Sales is the logarithm of firms' sales. Exports is the logarithm of firms' exports. SOE is a dummy variable equal to 1 if a firm is state-owned, and 0 otherwise. Employees is the logarithm of firms' employees. Capital is the logarithm of firms' net fixed assets. TFP is firms' total factor productivity calculated using the Olley and Pakes (1996) method.

Table 2: Effect on Firm-Level SO_2 and COD Emissions

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. var.:	SO_2	SO_2	SO_2	COD	COD	COD
Treat × Post	-0.239***	-0.217***	-0.141***	-0.385***	-0.402***	-0.187***
	(0.034)	(0.033)	(0.035)	(0.037)	(0.037)	(0.039)
Firm FE	X	X	X	X	X	X
Year FE	X			X		
Industry-year FE		X	X		X	X
Controls × Year FE			X			X
Observations	143,524	143,520	143,520	143,524	143,520	143,520
R-squared	0.773	0.776	0.777	0.723	0.730	0.733

Notes: This table compares the emissions of key pollutants (SO_2 and COD) between treatment and control firms before and after the national pollution control reform in 2007. The sample period is 2001-2010. Treat is a dummy variable equal to 1 if firms received any subsidies in the pre-reform period (2001-2006), and 0 otherwise. Post is a dummy variable equal to 1 for 2007 and later years, and 0 otherwise. SO_2 and COD denote the emissions of sulfur dioxide and chemical oxygen demand, respectively, and are transformed using the inverse hyperbolic sine (IHS) function to deal with zero values. Industry denotes 2-digit industries. Controls are firm-level covariates listed in the summary statistics. The standard errors reported in parentheses are clustered at the firm level. * denotes significance at the 10% level. ** denotes significance at the 5% level. *** denotes significance at the 1% level.

Table 3: Firm Adjustments

	(1)	(2)	(3)	(4)
Panel (a): Clean technology adoption				
Dep. var.:	Coal intensity	Water intensity	Green patents	Abatement facilities
Treat × Post	-0.041***	-0.068***	-0.000	0.124***
	(0.004)	(0.013)	(0.001)	(0.009)
Observations	136,341	142,946	143,520	143,520
R-squared	0.749	0.855	0.358	0.775
Panel (b): Production				
Dep. var.:	Output	Profit	Sales	TFP
Treat × Post	-0.016*	-0.187**	-0.018*	-0.021***
	(0.010)	(0.088)	(0.010)	(0.003)
Observations	143,085	137,488	131,038	135,092
R-squared	0.927	0.527	0.920	0.770
Firm FE	X	X	X	X
Industry-year FE	X	X	X	X
Controls × Year FE	X	X	X	X

Notes: This table examines how firms adjusted in response to the national pollution control reform in 2007. The sample period is 2001-2010. Treat is a dummy variable equal to 1 if firms received any subsidies in the pre-reform period (2001-2006), and 0 otherwise. Post is a dummy variable equal to 1 for 2007 and later years, and 0 otherwise. The dependent variables capture firms' margins of adjustments and are transformed using either the logarithm or the inverse hyperbolic sine (IHS) when they could zero-valued. Specifically, Coal intensity denotes firms' coal use per unit of output. Water intensity denotes firms' industrial water use per unit of output. Green patents denotes firms' patents aimed at reducing emissions during production. Abatement facilities denotes firms' pollution abatement facilities. Output, profits, sales, and TFP denote firms' economic performance, with TFP representing total factor productivity calculated using the Olley and Pakes (1996) method. Industry denotes 2-digit industries. Controls are firm-level covariates listed in the summary statistics. The standard errors reported in parentheses are clustered at the firm level. * denotes significance at the 10% level. ** denotes significance at the 5% level. *** denotes significance at the 1% level.

Table 4: Heterogeneity in Reductions by Norms of Reciprocity

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. var.:	SO_2	SO_2	SO_2	COD	COD	COD
Treat × Post × $Norm_1$	-0.080*			-0.111*		
	(0.046)			(0.059)		
Post × $Norm_1$	0.123***			0.018		
	(0.030)			(0.037)		
Treat × Post × $Norm_2$		-0.097**			-0.125***	
		(0.043)			(0.045)	
Post × $Norm_2$		0.170***			0.066**	
		(0.028)			(0.028)	
Treat × Post × $Norm_3$			-0.153**			-0.158*
			(0.066)			(0.082)
Post × $Norm_3$			0.296***			0.414***
			(0.048)			(0.065)
Treat × Post	-0.012	-0.044	-0.124	-0.139**	-0.154***	-0.175**
	(0.053)	(0.050)	(0.081)	(0.058)	(0.050)	(0.083)
Firm FE	X	X	X	X	X	X
Industry-year FE	X	X	X	X	X	X
Controls × Year FE	X	X	X	X	X	X
Observations	64,195	74,029	29,620	64,195	74,029	29,620
R-squared	0.789	0.768	0.808	0.740	0.739	0.750

Notes: This table shows the heterogeneity in emission reductions across cities with varying norms of reciprocity. Treat is a dummy variable equal to 1 if firms received any subsidies in the pre-reform period (2001-2006), and 0 otherwise. Post is a dummy variable equal to 1 for 2007 and later years, and 0 otherwise. SO_2 and COD denote the emissions of sulfur dioxide and chemical oxygen demand, respectively, and are transformed using the inverse hyperbolic sine (IHS) function to deal with zero values. $Norm_1$, $Norm_2$, and $Norm_3$ denote different measures of city-level norms of reciprocity, which are constructed using three nationally representative surveys. The first survey asks citizens about their willingness to pay taxes for universal healthcare. The second survey asks entrepreneurs about their willingness to donate in gratitude to the government. The third survey asks citizens about their willingness to repay a small favor. We then aggregate each of these questions to the city level after partialling out individual characteristics and create three standardized measures of norms of reciprocity. Industry denotes 2-digit industries. Controls are firm-level covariates listed in the summary statistics. The standard errors reported in parentheses are clustered at the firm level. * denotes significance at the 10% level. ** denotes significance at the 5% level. *** denotes significance at the 1% level.

Table 5: Emission Reductions and Future Subsidies

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. var.:	Subsidy incidence			Subsidy amount		
Treat × Period_{07-10} × Reduction_1	-0.001			0.008		
	(0.008)			(0.058)		
Treat × Period_{11-14} × Reduction_1	0.050***			0.388***		
	(0.012)			(0.092)		
Period_{07-10} × Reduction_1	0.001			-0.002		
	(0.003)			(0.022)		
Period_{11-14} × Reduction_1	0.001			-0.036		
	(0.007)			(0.055)		
Treat × Period_{07-10} × Reduction_2		0.006			0.009	
		(0.009)			(0.066)	
Treat × Period_{11-14} × Reduction_2		0.043***			0.311***	
		(0.013)			(0.100)	
Period_{07-10} × Reduction_2		0.002			0.012	
		(0.003)			(0.020)	
Period_{11-14} × Reduction_2		-0.001			-0.023	
		(0.007)			(0.056)	
Treat × Period_{07-10} × Reduction_3			-0.001			-0.024
			(0.005)			(0.039)
Treat × Period_{11-14} × Reduction_3			0.049***			0.347***
			(0.007)			(0.057)
Period_{07-10} × Reduction_3			-0.002			-0.011
			(0.002)			(0.013)
Period_{11-14} × Reduction_3			-0.006			-0.061**
			(0.004)			(0.030)
Treat × Period_{07-10}	-0.352***	-0.342***	-0.353***	-2.224***	-2.159***	-2.223***
	(0.005)	(0.006)	(0.005)	(0.039)	(0.042)	(0.037)
Treat × Period_{11-14}	-0.196***	-0.187***	-0.197***	-1.034***	-0.985***	-1.018***
	(0.008)	(0.009)	(0.007)	(0.062)	(0.069)	(0.057)
Firm FE	X	X	X	X	X	X
Industry-year FE	X	X	X	X	X	X
Controls × Year FE	X	X	X	X	X	X
Observations	147,208	123,098	169,474	147,208	123,098	169,474
R-squared	0.496	0.496	0.499	0.527	0.527	0.529

Notes: This table relates firms' emission reductions to future government subsidies. The sample period is 2001-2014. Treat is a dummy variable equal to 1 if firms received any subsidies in the pre-reform period (2001-2006), and 0 otherwise. Post is a dummy variable equal to 1 for 2007 and later years, and 0 otherwise. Subsidy incidence is a dummy variable equal to 1 for positive subsidies and 0 otherwise. Subsidy amount is the inverse hyperbolic sine (IHS) transformation of firms' subsidies. Period_{07-10} is a dummy variable equal to 1 for years 2007-2010, and 0 otherwise. Period_{11-14} is a dummy variable equal to 1 for years 2011-2014, and 0 otherwise. Reduction_1, Reduction_2, and Reduction_3 denote different measures of firms' emission reductions in the post-reform period (2007-2010). Specifically, $Reduction_1 = 0.5 \times \frac{\overline{SO2_{01-06}} - \overline{SO2_{07-10}}}{\overline{SO2_{01-06}}} + 0.5 \times \frac{\overline{COD_{01-06}} - \overline{COD_{07-10}}}{\overline{COD_{01-06}}}$. $Reduction_2 = 0.5 \times \left(\overline{SO2_{06}} - \overline{SO2_{07-10}}\right) / \overline{SO2_{06}} + 0.5 \times \left(\overline{COD_{06}} - \overline{COD_{07-10}}\right) / \overline{COD_{06}}$. $Reduction_3 = 0.5 \times \left(\overline{SO2_{01-06}} - \overline{SO2_{07-10}}\right) + 0.5 \times \left(\overline{COD_{01-06}} - \overline{COD_{07-10}}\right)$. Industry denotes 2-digit industries. Controls are firm-level covariates listed in the summary statistics. The standard errors reported in parentheses are clustered at the firm level. * denotes significance at the 10% level. ** denotes significance at the 5% level. *** denotes significance at the 1% level.

Table 6: Effect on City-Level SO_2 and COD Concentrations

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. var.:	SO_2	SO_2	SO_2	COD	COD	COD
Treat × Post	-0.085***	-0.081***	-0.088***	-0.649**	-0.604**	-0.617**
	(0.024)	(0.028)	(0.027)	(0.306)	(0.288)	(0.284)
City FE	X	X	X	X	X	X
Province-year FE	X	X	X	X	X	X
Economic controls × Year FE		X	X		X	X
Weather controls			X			X
Observations	2,710	2,710	2,653	666	638	631
R-squared	0.997	0.998	0.998	0.942	0.946	0.953

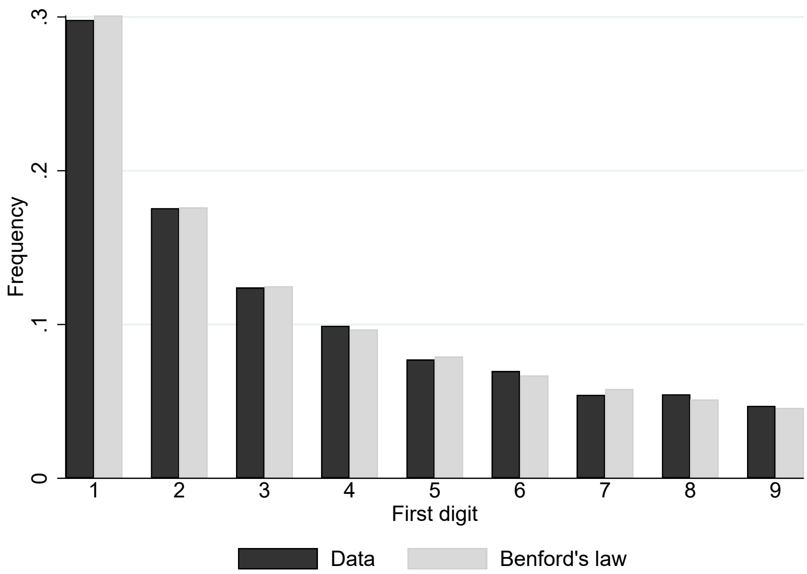
Notes: This table conducts a regional-level analysis by aggregating firm-level treatment to the city level and relating it to citywide SO_2 and COD concentrations. The sample period is 2001-2010 in columns (1)-(3) and 2004-2010 in columns (4)-(6) (due to data limitations). Treat is the city-level average of firm-level treatment, with the latter being a dummy variable equal to 1 if firms received any subsidies in the pre-reform period (2001-2006), and 0 otherwise. Post is a dummy variable equal to 1 for 2007 and later years, and 0 otherwise. SO_2 denotes the logarithm of city-level sulfur dioxide concentrations derived from satellite observations. COD denotes the inverse distance-weighted average chemical oxygen demand readings from monitoring stations downstream and within 100 km of a city (as water pollutants only flow from high to low). Economic controls include city-level GDP per capita, fiscal revenue, population, and industrial production, all measured in the pre-reform period. Weather controls include temperature, precipitation, wind direction, wind speed, and dew point temperature. The standard errors reported in parentheses are clustered at the city level. * denotes significance at the 10% level. ** denotes significance at the 5% level. *** denotes significance at the 1% level.

Online Appendix

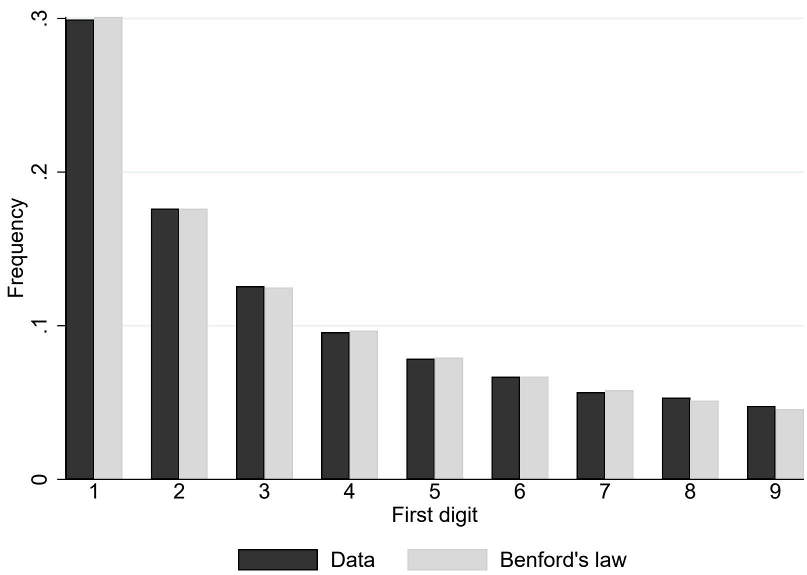
Additional Figures

Figure A1: Detecting Emission Anomalies using Benford's Law

(a) SO_2 Emissions



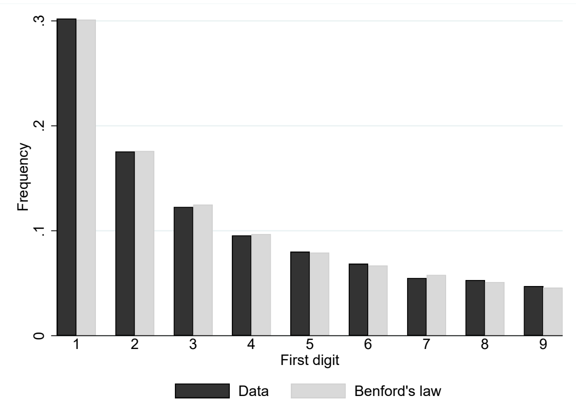
(b) COD Emissions



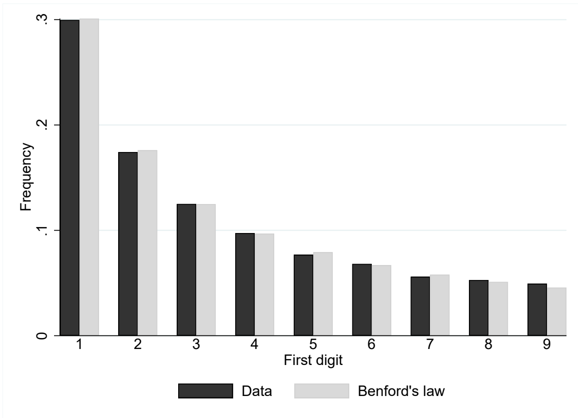
Notes: The figure shows the distribution of the first digits in our raw SO_2 (sulfur dioxide) and COD (chemical oxygen demand) emission data, and the distribution implied by the Benford's law (Marchi and Hamilton, 2006; Judge and Schechter, 2009). The Benford's law is widely used to detect anomalies in naturally occurring datasets, which gives: $P(\text{first digit is } n) = \log(1 + \frac{1}{n})$, where n represents integers from 1 to 9.

Figure A2: Detecting Anomalies in SO_2 Emissions using Benford's Law: Sample Splits

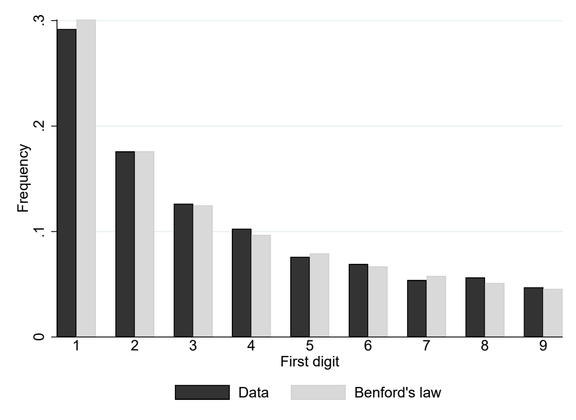
(a) Treatment and Pre-reform



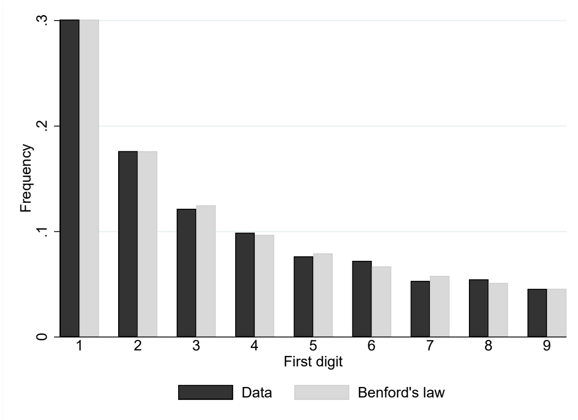
(b) Treatment and Post-reform



(c) Control and Pre-reform



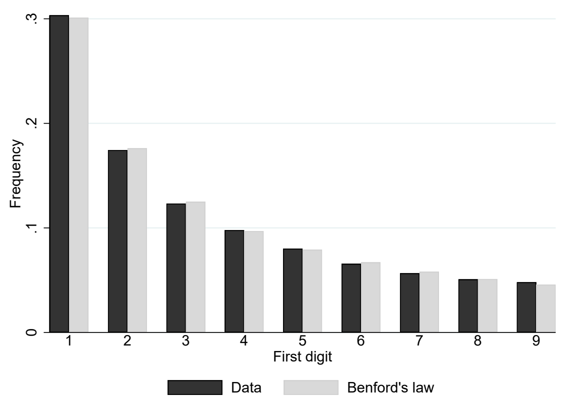
(d) Control and Post-reform



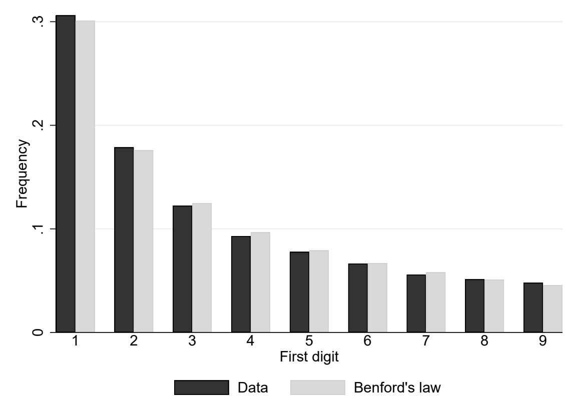
Notes: The figure shows the distribution of the first digits in our raw SO_2 (sulfur dioxide) emission data, and the distribution implied by the Benford's law (Marchi and Hamilton, 2006; Judge and Schechter, 2009), with the full sample split by treatment status (treatment versus control firms) and time period (pre- versus post-reform). The Benford's law is widely used to detect anomalies in naturally occurring datasets, which gives: $P(\text{first digit is } n) = \log(1 + \frac{1}{n})$, where n represents integers from 1 to 9. Treatment firms are firms that received any subsidies in the pre-reform period (2001-2006) and control firms are those that did not. Pre-reform period denotes the years 2001-2006 and post-reform period denotes the years 2007-2010.

Figure A3: Detecting Anomalies in COD Emissions using Benford's Law: Sample Splits

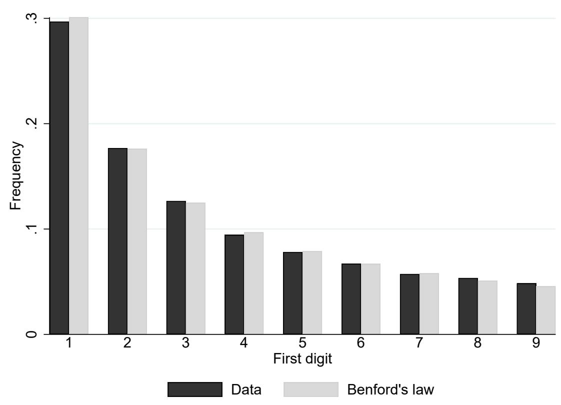
(a) Treatment and Pre-reform



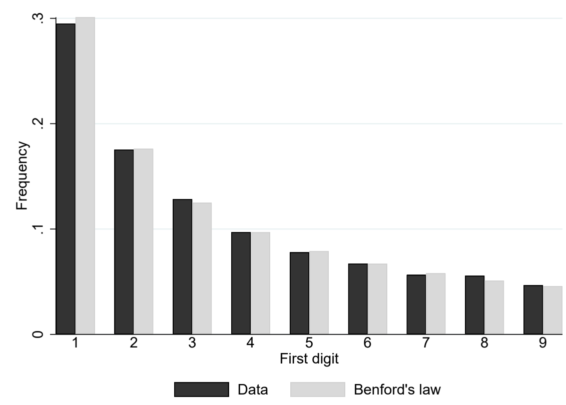
(b) Treatment and Post-reform



(c) Control and Pre-reform

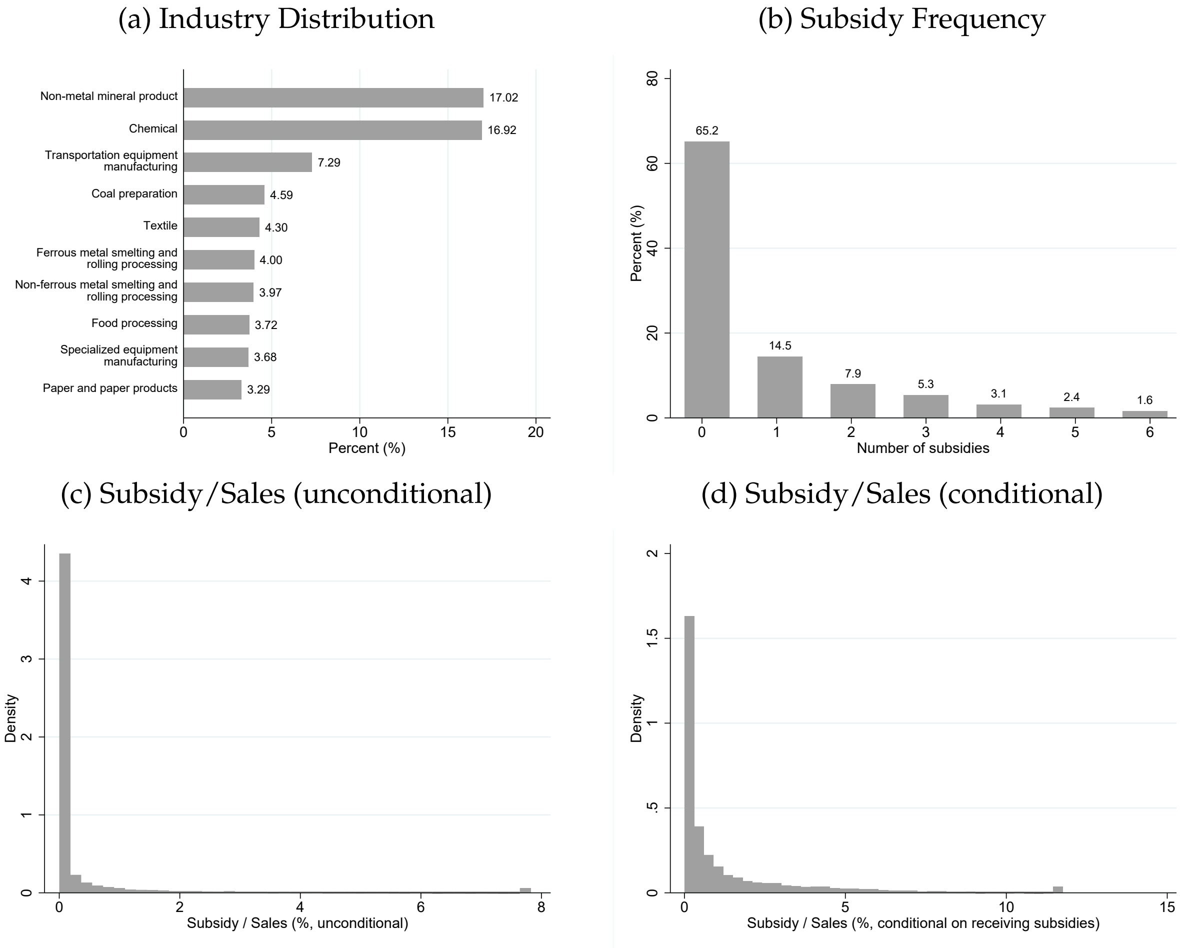


(d) Control and Post-reform



Notes: The figure shows the distribution of the first digits in our raw COD (chemical oxygen demand) emission data, and the distribution implied by the Benford's law (Marchi and Hamilton, 2006; Judge and Schechter, 2009), with the full sample split by treatment status (treatment versus control firms) and time period (pre- versus post-reform). The Benford's law is widely used to detect anomalies in naturally occurring datasets, which gives: $P(\text{first digit is } n) = \log(1 + \frac{1}{n})$, where n represents integers from 1 to 9. Treatment firms are firms that received any subsidies in the pre-reform period (2001-2006) and control firms are those that did not. Pre-reform period denotes the years 2001-2006 and post-reform period denotes the years 2007-2010.

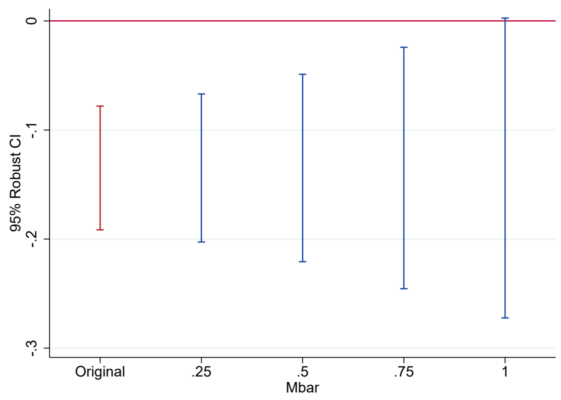
Figure A4: Distribution of Government Subsidies



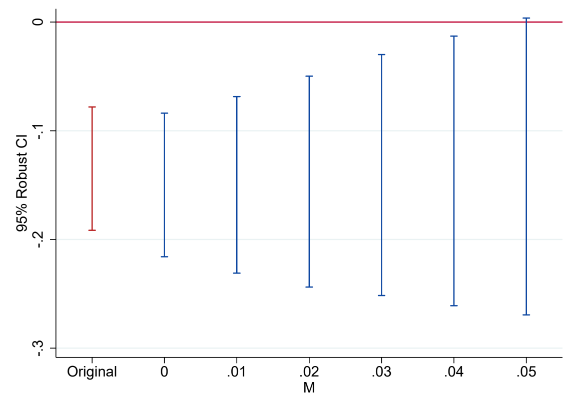
Notes: This figure shows the distribution of pre-reform government subsidies received by firms in our sample. Specifically, panel (a) shows the top 10 industries receiving the highest share of total subsidies. Panel (b) shows the frequency of subsidies. Panel (c) shows the unconditional distribution of subsidies normalized by sales, with an average of approximately 0.4%. Panel (d) shows the distribution of subsidies normalized by sales, conditional on receiving subsidies, with an average of approximately 1.3%.

Figure A5: Sensitivity to Violations of Parallel Trends Assumption

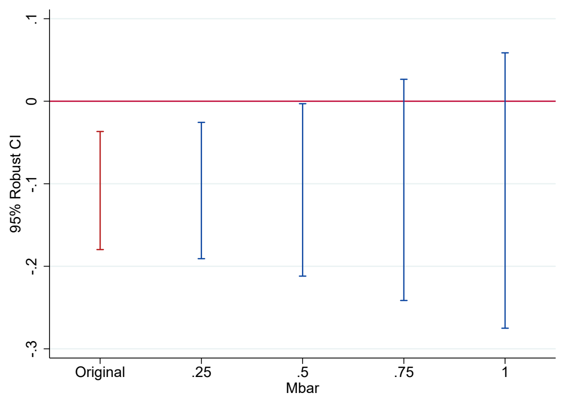
(a) SO_2: Bounding Relative Magnitudes



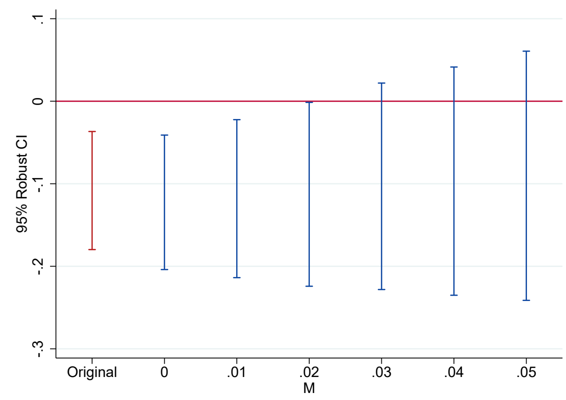
(b) SO_2: Smoothness Restrictions



(c) COD: Bounding Relative Magnitudes



(d) COD: Smoothness Restrictions

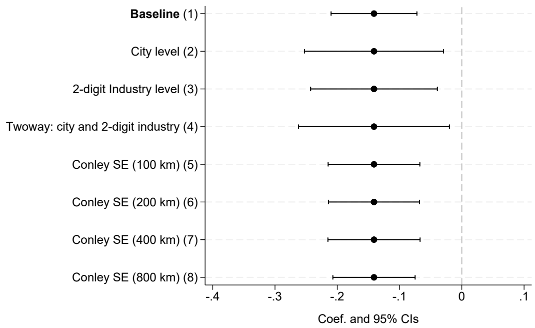


Notes: This figure shows the sensitivity of our baseline estimates to potential violations of parallel trends , using the partial identification scheme proposed by Rambachan and Roth (2023), who partially identify the treatment effects under two types of restrictions on violations of parallel trends. Specifically, panels (a) and (c) plot the robust 95% confidence sets for the treatment effects in the first year following the reform (β_{2007} in the event study equation (2)), using Rambachan and Roth (2023)'s "bounding relative magnitudes" method, which assumes that the maximal post-reform violations of parallel trends between consecutive periods cannot be larger than $\bar{M}$ times the maximal pre-reform violations of parallel trends. Panels (b) and (d) plot the robust 95% confidence sets for the treatment effects in the first year following the reform (β_{2007} in the event study equation (2)), using Rambachan and Roth (2023)'s "smoothness restrictions" method, which allows the slope of the differential trends to change by no more than M between consecutive periods, with $M = 0$ corresponding to linear treatment-specific time trends. The original estimates of β_{2007} in the event study equation (2) are also plotted (in red) for reference. SO_2 and COD denote the emissions of sulfur dioxide and chemical oxygen demand, respectively, and are transformed using the inverse hyperbolic sine (IHS) function to deal with zero values.

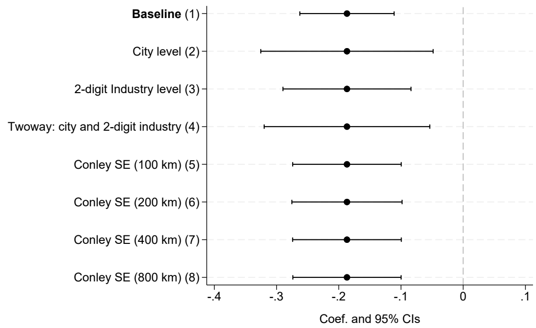
Figure A6: Alternative Inference Procedures

Alternative Standard Errors

(a) SO_2 Emissions

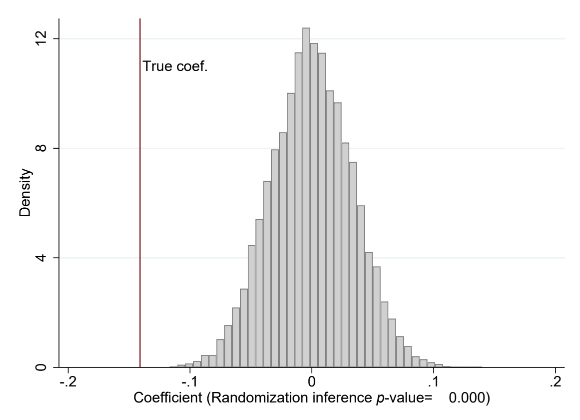


(b) COD Emissions

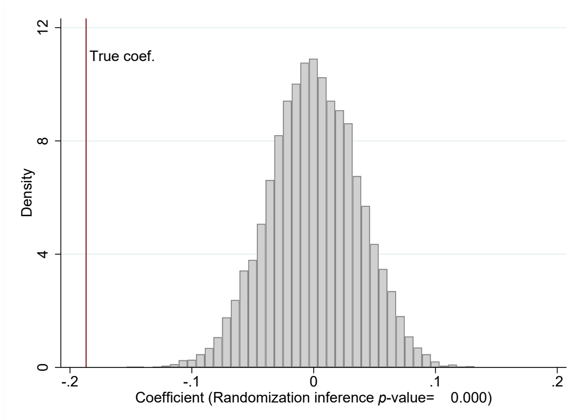


Randomization Inference

(c) SO_2 Emissions



(d) COD Emissions

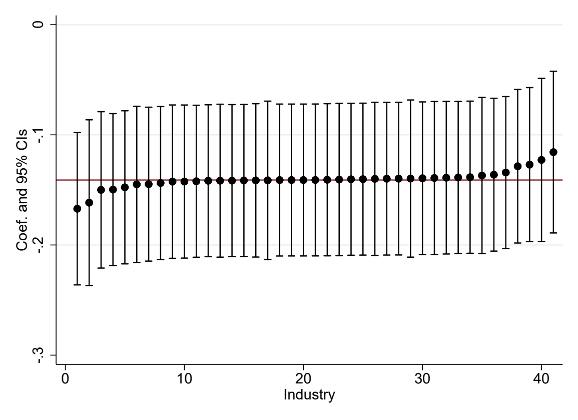


Notes: This figure shows the robustness of our baseline estimates to alternative inference procedures. In row (1) of panels (a)-(b), we plot the baseline estimates for reference. In rows (2)-(4) of panels (a)-(b), we correct for within-cluster correlation using three alternative clustering levels: at the city level, at the 2-digit industry level, and two-way clustering at both the city and 2-digit industry levels. In rows (5)-(8) of panels (a)-(b), we correct for spatial correlation using Conley standard errors (Conley, 1999), allowing for correlation of observations within distances of 100 km, 200 km, 400 km, and 800 km. We also allow for serial correlation across all years. In panels (c)-(d), we perform randomization inference, which is robust to high-leverage observations and complex error structures. Following the recommendation of Young (2019), We conduct 10,000 random permutations of the treatment while maintaining the same probability of treatment as in the original sample. We then compare the true coefficient estimates to the coefficient estimates generated by the permutations. Notably, none of the permutations yield coefficient estimates that exceed our true estimates in absolute terms.

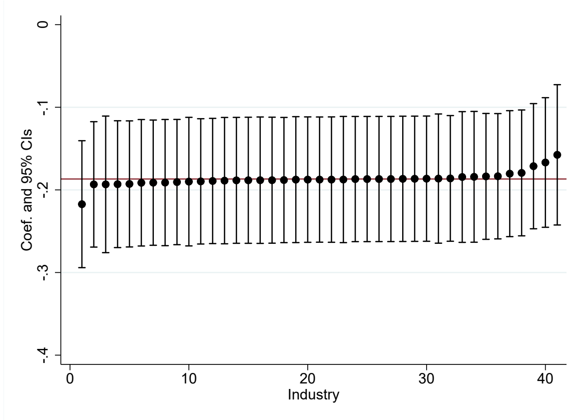
Figure A7: Leave-One-Out Estimates

Excluding an Industry Each Time

(a) SO_2 Emissions

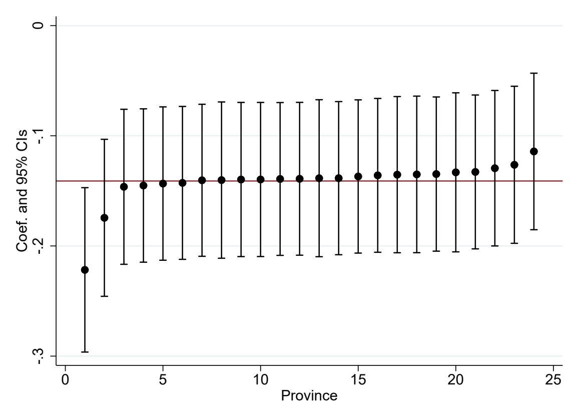


(b) COD Emissions

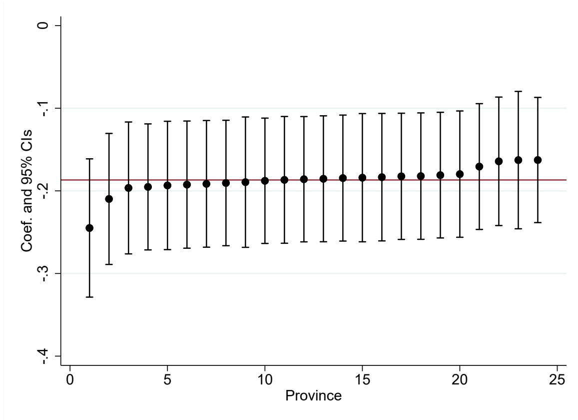


Excluding a Province Each Time

(c) SO_2 Emissions



(d) COD Emissions



Notes: This figures shows the robustness of our baseline estimates when excluding one industry or province each time. Panel (a) and (b) plot the coefficient estimates after excluding one 2-digit industry each time. Panel (c) and (d) plot the coefficient estimates after excluding one province each time. SO_2 and COD denote the emissions of sulfur dioxide and chemical oxygen demand, respectively, and are transformed using the inverse hyperbolic sine (IHS) function to deal with zero values. The standard errors used to construct the 95% confidence intervals, indicated by the spikes, are clustered at the firm level.

Additional Tables

Table A1: Non-Targeted Pollutants as Placebo

Dep. var.:	(1) Dust	(2) NH_3-N	(3) Raw Dust	(4) Raw NH_3-N
Treat × Post	-0.028	0.015	8,816.8	181.2
	(0.069)	(0.053)	(6,234.2)	(139.5)
Firm FE	X	X	X	X
Industry-year FE	X	X	X	X
Controls × Year FE	X	X	X	X
Observations	93,634	122,173	93,634	122,173
R-squared	0.885	0.718	0.804	0.739
Mean of dep. var.			101957.7	3430.2

Notes: This table compares the emissions of non-targeted pollutants (Dust and NH_3-N) between treatment and control firms before and after the national pollution control reform in 2007. The sample period is 2001-2010. Treat is a dummy variable equal to 1 if firms received any subsidies in the pre-reform period (2001-2006), and 0 otherwise. Post is a dummy variable equal to 1 for 2007 and later years, and 0 otherwise. Dust and NH_3-N denote the emissions of industrial dust and ammonia nitrogen, respectively, and are transformed using the inverse hyperbolic sine (IHS) function to deal with zero values in the first two columns. Industry denotes 2-digit industries. Controls are firm-level covariates listed in the summary statistics. The standard errors reported in parentheses are clustered at the firm level. * denotes significance at the 10% level. ** denotes significance at the 5% level. *** denotes significance at the 1% level.

Table A2: Exploiting Within-Treatment Variations for Identification

	(1)	(2)	(3)	(4)
Panel (a): High versus low pollution				
Dep. var.:	SO_2	SO_2	COD	COD
Treat × Post	-0.116*	-0.116*	-0.177**	-0.176**
× High pollution	(0.069)	(0.069)	(0.073)	(0.073)
Treat × Post	-0.029		-0.050	
	(0.058)		(0.061)	
Observations	143,501	143,501	143,515	143,515
R-squared	0.783	0.783	0.743	0.743
Panel (b): Single versus conglomerate				
Dep. var.:	SO_2	SO_2	COD	COD
Treat × Post	-0.228***	-0.226***	-0.218***	-0.214***
× Conglomerate	(0.080)	(0.080)	(0.083)	(0.083)
Treat × Post	-0.087**		-0.133***	
	(0.040)		(0.044)	
Observations	143,513	143,513	143,513	143,513
R-squared	0.778	0.778	0.734	0.734
Firm FE	X	X	X	X
Industry-year FE	X	X	X	X
Industry-year-group FE	X	X	X	X
Treat-year FE		X		X
Controls × Year FE	X	X	X	X

Notes: This table exploits within-treatment variations for identification. The sample period is 2001-2010. Treat is a dummy variable equal to 1 if firms received any subsidies in the pre-reform period (2001-2006), and 0 otherwise. Post is a dummy variable equal to 1 for 2007 and later years, and 0 otherwise. SO_2 and COD denote the emissions of sulfur dioxide and chemical oxygen demand, respectively, and are transformed using the inverse hyperbolic sine (IHS) function to deal with zero values. High pollution is a dummy variable indicating whether pre-reform emissions exceed the sample median. To better capture recent emission trends and minimize short-term fluctuations, we consider emissions over the three years preceding the reform when constructing this indicator. Conglomerate is a dummy variable indicating whether a firm was part of a multi-division conglomerate in 2006. To avoid pollution displacement across internal firms, we only consider conglomerates that contain other non-polluting firms. Industry denotes 2-digit industries. Controls are firm-level covariates listed in the summary statistics. The standard errors reported in parentheses are clustered at the firm level. * denotes significance at the 10% level. ** denotes significance at the 5% level. *** denotes significance at the 1% level.

Table A3: Ruling Out Spillover Effects

Dep. var.:	(1) SO_2	(2) SO_2	(3) COD	(4) COD
Treat × Post	-0.108***	-0.145***	-0.156***	-0.201***
	(0.039)	(0.036)	(0.042)	(0.040)
# treated neighbors within	-0.007		0.014	
the same postal code × Post	(0.012)		(0.012)	
# treated neighbors within		-0.010		-0.003
a 5 km radius× Post		(0.009)		(0.009)
Firm FE	X	X	X	X
Industry-year FE	X	X	X	X
# treated neighbors-year FE	X	X	X	X
Controls × Year FE	X	X	X	X
Observations	118,310	138,990	118,310	138,990
R-squared	0.767	0.778	0.739	0.733
Mean # treated neighbors	1.98	3.18	1.98	3.18
Mean # neighbors	5.56	8.64	5.56	8.64

Notes: This table shows the robustness of the baseline results when accounting for potential spillover effects between treatment and control firms. The sample period is 2001-2010. Treat is a dummy variable equal to 1 if firms received any subsidies in the pre-reform period (2001-2006), and 0 otherwise. Post is a dummy variable equal to 1 for 2007 and later years, and 0 otherwise. SO_2 and COD denote the emissions of sulfur dioxide and chemical oxygen demand, respectively, and are transformed using the inverse hyperbolic sine (IHS) function to deal with zero values. To capture the strength of spillovers, we use the number of other treatment firms in a firm's neighborhood as a proxy, where the neighborhood is defined either by the firm's postal code or by a 5 km radius around the firm. Industry denotes 2-digit industries. Controls are firm-level covariates listed in the summary statistics. The standard errors reported in parentheses are clustered at the firm level. * denotes significance at the 10% level. ** denotes significance at the 5% level. *** denotes significance at the 1% level.

Table A4: Addressing Concerns about the No-Anticipation Assumption

	(1)	(2)	(3)	(4)
	Excluding 2005 and 2006 observations		Excluding firms becoming treated at 2005 or 2006	
Dep. var.:	SO_2	COD	SO_2	COD
Treat × Post	-0.108**	-0.145***	-0.182***	-0.199***
	(0.047)	(0.051)	(0.041)	(0.044)
Firm FE	X	X	X	X
Industry-year FE	X	X	X	X
Controls × Year FE	X	X	X	X
Observations	104,694	104,694	128,289	128,289
R-squared	0.783	0.744	0.778	0.733

Notes: This table address concerns about the no-anticipation assumption. The sample period is 2001-2010. Treat is a dummy variable equal to 1 if firms received any subsidies in the pre-reform period (2001-2006), and 0 otherwise. Post is a dummy variable equal to 1 for 2007 and later years, and 0 otherwise. SO_2 and COD denote the emissions of sulfur dioxide and chemical oxygen demand, respectively, and are transformed using the inverse hyperbolic sine (IHS) function to deal with zero values. Columns (1)-(2) exclude observations in the two years before the reform. Columns (3)-(4) exclude treatment firms that were first treated at 2005 or 2006. Industry denotes 2-digit industries. Controls are firm-level covariates listed in the summary statistics. The standard errors reported in parentheses are clustered at the firm level. * denotes significance at the 10% level. ** denotes significance at the 5% level. *** denotes significance at the 1% level.

Table A5: Alternative Outcome and Treatment Measures

	(1)	(2)	(3)	(4)
Panel (a): Alternative outcome measures				
Dep. var.:	Raw SO_2	Raw COD	SO_2 intensity	COD intensity
Treat $\times$ Post	-12,427.733***	-4,577.171***	-0.064***	-0.060***
	(1,738.800)	(1,273.993)	(0.009)	(0.009)
Observations	143,520	143,520	143,085	143,085
R-squared	0.797	0.787	0.784	0.742
Mean of dep. var.	91222.266	52782.657		
Panel (b): Alternative treatment measures				
Dep. var.:	SO_2	COD	SO_2	COD
Treat intensity $\times$ Post	-0.117***	-0.093***		
	(0.027)	(0.033)		
1(Subsidy freq.>=3) $\times$ Post			-0.241***	-0.302***
			(0.049)	(0.052)
Observations	143,520	143,520	143,520	143,520
R-squared	0.777	0.733	0.777	0.733
Firm FE	X	X	X	X
Industry-year FE	X	X	X	X
Controls $\times$ Year FE	X	X	X	X

Notes: This table shows the robustness of the baseline results by using alternative variable measures. The sample period is 2001-2010. Treat is a dummy variable equal to 1 if firms received any subsidies in the pre-reform period (2001-2006), and 0 otherwise. Post is a dummy variable equal to 1 for 2007 and later years, and 0 otherwise. In panel (a), we use alternative outcome measures: columns (1)-(2) use the raw values of SO_2 and COD emissions while columns (3)-(4) use the inverse hyperbolic sine (IHS) transformation of the emissions per unit of output to capture the intensity of emissions. In panel (b), we use alternative treatment measures and maintain the same outcomes as in the baseline specification (the IHS transformation of SO_2 and COD emissions): columns (1)-(2) measure the intensity of the treatment using the average subsidy-to-assets ratio in the pre-reform period while columns (3)-(4) adopt a dummy variable indicating firms that received subsidies for at least 3 years in the pre-reform period. Industry denotes 2-digit industries. Controls are firm-level covariates listed in the summary statistics. The standard errors reported in parentheses are clustered at the firm level. * denotes significance at the 10% level. ** denotes significance at the 5% level. *** denotes significance at the 1% level.

Table A6: Ruling Out Confounding Policies

	(1)	(2)	(3)	(4)	(5)
Panel (a): Dep. var. = SO_2					
Treat × Post	-0.140***	-0.137***	-0.138***	-0.141***	-0.138***
	(0.036)	(0.036)	(0.036)	(0.036)	(0.036)
Observations	139,015	139,015	139,015	139,015	139,015
R-squared	0.776	0.776	0.776	0.776	0.777
Panel (b): Dep. var. = COD					
Treat × Post	-0.191***	-0.188***	-0.193***	-0.201***	-0.196***
	(0.039)	(0.039)	(0.039)	(0.039)	(0.039)
Observations	139,015	139,015	139,015	139,015	139,015
R-squared	0.730	0.730	0.730	0.731	0.731
Firm FE	X	X	X	X	X
Industry-year FE	X	X	X	X	X
Controls × Year FE	X	X	X	X	X
Dist. to TCZ city center × Year FE	X				X
Dist. to 2008 Olympics city × Year FE		X			X
Dist. to provincial boundaries × Year FE			X		X
Dist. to key waters × Year FE				X	X

Notes: This table shows the robustness of the baseline results after controlling for confounding policies that may influence firms' emission reductions and correlate with our treatment measure. The sample period is 2001-2010. Treat is a dummy variable equal to 1 if firms received any subsidies in the pre-reform period (2001-2006), and 0 otherwise. Post is a dummy variable equal to 1 for 2007 and later years, and 0 otherwise. SO_2 and COD denote the emissions of sulfur dioxide and chemical oxygen demand, respectively, and are transformed using the inverse hyperbolic sine (IHS) function to deal with zero values. The confounding policies we consider are: the SO_2 emission regulation under the 1998 Two Control Zones (TCZ) policy (column (1)), the air quality regulation for host and neighboring cities during the 2008 Beijing Olympics (column (2)), the water pollution regulation at provincial borders (column (3)) and key waters (column (4)) in the Eleventh Five-Year Plan, and all of them in column (5). We control for the confounding effects of these policies by including the distance from each firm to the targeted areas, interacted with year fixed effects. Industry denotes 2-digit industries. Controls are firm-level covariates listed in the summary statistics. The standard errors reported in parentheses are clustered at the firm level. * denotes significance at the 10% level. ** denotes significance at the 5% level. *** denotes significance at the 1% level.

Table A7: Weighted Regression

	(1)	(2)	(3)	(4)
	Pre-reform size as weight		Pre-reform sales as weight	
Dep. var.:	SO_2	COD	SO_2	COD
Treat × Post	-0.148***	-0.191***	-0.145***	-0.193***
	(0.036)	(0.039)	(0.036)	(0.039)
Firm FE	X	X	X	X
Industry-year FE	X	X	X	X
Controls × Year FE	X	X	X	X
Observations	143,520	143,520	143,520	143,520
R-squared	0.779	0.732	0.778	0.733

Notes: This table shows the robustness of the baseline results by using firm size as weights in the regressions. The sample period is 2001-2010. Treat is a dummy variable equal to 1 if firms received any subsidies in the pre-reform period (2001-2006), and 0 otherwise. Post is a dummy variable equal to 1 for 2007 and later years, and 0 otherwise. SO_2 and COD denote the emissions of sulfur dioxide and chemical oxygen demand, respectively, and are transformed using the inverse hyperbolic sine (IHS) function to deal with zero values. In columns (1) and (2), we use pre-reform average total assets as weights in the regression. In columns (3) and (4), we use pre-reform average sales as weights in the regression. Industry denotes 2-digit industries. Controls are firm-level covariates listed in the summary statistics. The standard errors reported in parentheses are clustered at the firm level. * denotes significance at the 10% level. ** denotes significance at the 5% level. *** denotes significance at the 1% level.

Table A8: Emission Reductions and Future Subsidies – Robustness

	(1)	(2)
Dep. var.:	Subsidy incidence	Subsidy amount
Treat × Period_{07-10} × Reduction target	-0.011	0.060
	(0.013)	(0.103)
Treat × Period_{11-14} × Reduction target	0.049**	0.534***
	(0.021)	(0.160)
Period_{07-10} × Reduction target	-0.010*	-0.109***
	(0.005)	(0.035)
Period_{11-14} × Reduction target	-0.076***	-0.694***
	(0.013)	(0.096)
Treat × Period_{07-10}	-0.358***	-2.314***
	(0.012)	(0.091)
Treat × Period_{11-14}	-0.226***	-1.315***
	(0.019)	(0.145)
Firm FE	X	X
Industry-year FE	X	X
Controls × Year FE	X	X
Observations	111,718	111,718
R-squared	0.503	0.532

Notes: This table relates firms' emission reductions to future government subsidies. The sample period is 2001-2014. Treat is a dummy variable equal to 1 if firms received any subsidies in the pre-reform period (2001-2006), and 0 otherwise. Post is a dummy variable equal to 1 for 2007 and later years, and 0 otherwise. Subsidy incidence is a dummy variable equal to 1 for positive subsidies and 0 otherwise. Subsidy amount is the inverse hyperbolic sine (IHS) transformation of firms' subsidies. Period_{07-10} is a dummy variable equal to 1 for years 2007-2010, and 0 otherwise. Period_{11-14} is a dummy variable equal to 1 for years 2011-2014, and 0 otherwise. Reduction target is a dummy indicating cities with a specific reduction target in their 2008 annual work reports. Industry denotes 2-digit industries. Controls are firm-level covariates listed in the summary statistics. The standard errors reported in parentheses are clustered at the firm level. * denotes significance at the 10% level. ** denotes significance at the 5% level. *** denotes significance at the 1% level.

Table A9: Alternative Explanation: Reduced Financial Constraint

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. var.:	SO_2	SO_2	SO_2	COD	COD	COD
Treat × Post	-0.139***	-0.138***	-0.138***	-0.172***	-0.173***	-0.172***
	(0.034)	(0.034)	(0.034)	(0.039)	(0.039)	(0.039)
Firm FE	X	X	X	X	X	X
Industry-year FE	X	X	X	X	X	X
Controls × Year FE	X	X	X	X	X	X
Cash flow × Year FE	X			X		
Liquidity × Year FE		X			X	
Leverage × Year FE			X			X
Observations	117,695	117,695	117,695	117,695	117,695	117,695
R-squared	0.777	0.777	0.777	0.722	0.722	0.722

Notes: This table shows the robustness of the baseline results when accounting for potential differences in firms' financial constraints. The sample period is 2001-2010. Treat is a dummy variable equal to 1 if firms received any subsidies in the pre-reform period (2001-2006), and 0 otherwise. Post is a dummy variable equal to 1 for 2007 and later years, and 0 otherwise. SO_2 and COD denote the emissions of sulfur dioxide and chemical oxygen demand, respectively, and are transformed using the inverse hyperbolic sine (IHS) function to deal with zero values. we include firms' financial constraints at the end of 2006, interacted with year fixed effects, in our baseline regressions. We construct three measures of financial constraints: (1) cash flow, defined as operating cash flow over total assets; (2) liquidity, defined as current assets minus current liabilities over total assets; and (3) leverage, defined as current liabilities over current assets. The first two are negatively related to financial constraints and the last one is positively related to financial constraints. Industry denotes 2-digit industries. Controls are firm-level covariates listed in the summary statistics. The standard errors reported in parentheses are clustered at the firm level. * denotes significance at the 10% level. ** denotes significance at the 5% level. *** denotes significance at the 1% level.

Table A10: Alternative Explanation: Regulatory Enforcement

	(1)	(2)	(3)	(4)
Dep. var.:	Punishment number	Punishment incidence	Suspension	Fines
Treat × Post	0.0005	0.0005	-0.0001	0.0025
	(0.0005)	(0.0005)	(0.0002)	(0.0024)
Firm FE	X	X	X	X
Industry-year FE	X	X	X	X
Controls × Year FE	X	X	X	X
Observations	143,520	143,520	143,520	143,520
R-squared	0.2453	0.2428	0.2370	0.2436
Mean of dep. var.	0.0011	0.0010	0.0002	0.0052

Notes: This table examines whether there were differential regulatory enforcement between treatment and control firms. The sample period is 2001-2010. Treat is a dummy variable equal to 1 if firms received any subsidies in the pre-reform period (2001-2006), and 0 otherwise. Post is a dummy variable equal to 1 for 2007 and later years, and 0 otherwise. We use administrative data on environmental punishment from local Environmental Protection Bureaus as measures of regulatory enforcement. Specifically, in column (1), we use the number of punishment. In column (2), we use a dummy variable indicating postive number of punishment. In column (3), we use a dummy variable indicating suspension of production. In column (4), we use the inverse hyperbolic sine transformation of the amount of fines. Industry denotes 2-digit industries. Controls are firm-level covariates listed in the summary statistics. The standard errors reported in parentheses are clustered at the firm level. * denotes significance at the 10% level. ** denotes significance at the 5% level. *** denotes significance at the 1% level.

Table A11: Testing Political Connections using Public Firms

	(1)	(2)	(3)	(4)	(5)	(6)
	Baseline	Controlling for connections		Baseline	Controlling for connections	
Dep. var.:	SO_2	SO_2	SO_2	COD	COD	COD
Treat × Post	-0.598**	-0.642**	-0.598**	-0.565**	-0.568**	-0.563**
	(0.269)	(0.267)	(0.269)	(0.271)	(0.273)	(0.271)
Firm FE	X	X	X	X	X	X
Industry-year FE	X	X	X	X	X	X
Controls × Year FE	X	X	X	X	X	X
Board connect. × Year FE		X			X	
ETC × Year FE			X			X
Observations	2,998	2,998	2,998	2,998	2,998	2,998
R-squared	0.779	0.781	0.780	0.775	0.776	0.776

Notes: This table tests the political connections mechanism by focusing on a sample of public firms and then including measures of political connections. The sample period is 2001-2010. Treat is a dummy variable equal to 1 if firms received any subsidies in the pre-reform period (2001-2006), and 0 otherwise. Post is a dummy variable equal to 1 for 2007 and later years, and 0 otherwise. SO_2 and COD denote the emissions of sulfur dioxide and chemical oxygen demand, respectively, and are transformed using the inverse hyperbolic sine (IHS) function to deal with zero values. Columns (1) and (4) repeat the baseline estimation. In columns (2) and (5), we control for board connections in 2006 interacted with year fixed effects, with board connections measured as the fraction of board directors who are current or former government officials. In columns (3) and (6), we control for entertainment and travel cost (ETC) in 2006 interacted with year fixed effects (ETC encompasses expenses for dining, gifts, travel, and other activities, which is widely used to measure relationship building with local officials). Industry denotes 2-digit industries. Controls are firm-level covariates listed in the summary statistics. The standard errors reported in parentheses are clustered at the firm level. * denotes significance at the 10% level. ** denotes significance at the 5% level. *** denotes significance at the 1% level.

Table A12: Effect Heterogeneity by Leader Turnover

	(1)	(2)	(3)	(4)
Dep. var.	SO_2	SO_2	COD	COD
Treat × Post × Turnover number	0.006		-0.023	
	(0.049)		(0.057)	
Post × Turnover number	-0.038		0.116***	
	(0.029)		(0.033)	
Treat × Post × Turnover incidence		0.034		0.079
		(0.065)		(0.070)
Post × Turnover incidence		-0.045		0.088***
		(0.028)		(0.032)
Treat × Post	-0.140***	-0.152***	-0.160***	-0.205***
	(0.044)	(0.044)	(0.046)	(0.047)
Firm FE	X	X	X	X
Industry-year FE	X	X	X	X
Controls × Year FE	X	X	X	X
Observations	141,187	141,187	141,187	141,187
R-squared	0.777	0.777	0.734	0.734

Notes: This table tests the political connections mechanism by examining whether the emission reductions were lower in areas with political turnovers just prior to the reform, as these turnovers would disrupt local firms' political connections. The sample period is 2001-2010. Treat is a dummy variable equal to 1 if firms received any subsidies in the pre-reform period (2001-2006), and 0 otherwise. Post is a dummy variable equal to 1 for 2007 and later years, and 0 otherwise. SO_2 and COD denote the emissions of sulfur dioxide and chemical oxygen demand, respectively, and are transformed using the inverse hyperbolic sine (IHS) function to deal with zero values. We focus on the turnovers of the top two leaders (party secretaries and mayors) and count the number of turnovers in 2006, excluding cases where the same person moved from one position to another. We then check whether our treatment effects vary with the number of turnovers in columns (1) and (3) or the incidence of turnovers (i.e., a dummy indicating positive number) in columns (2) and (4). Industry denotes 2-digit industries. Controls are firm-level covariates listed in the summary statistics. The standard errors reported in parentheses are clustered at the firm level. * denotes significance at the 10% level. ** denotes significance at the 5% level. *** denotes significance at the 1% level.

Table A13: Effect on City-Level Concentrations of SO_2 and COD: Robustness

	(1)	(2)	(3)	(4)	(5)	(6)
	Placebo	Industrial SO_2	Size-weighted treatment	Placebo	Unweighted COD	Size-weighted treatment
Dep. var.:	O_3	SO_2	SO_2	COD	COD	COD
Treat × Post	0.000	-0.364**	-0.035**	-0.087	-0.649**	-0.399*
	(0.001)	(0.173)	(0.016)	(0.076)	(0.306)	(0.215)
City FE	X	X	X	X	X	X
Province-year FE	X	X	X	X	X	X
Observations	2,054	2,096	2,710	1,344	666	666
R-squared	0.999	0.940	0.997	0.983	0.942	0.941

Notes: This table checks the robustness of our regional-level analysis, where we aggregate firm-level treatment to the city level and relate it to citywide SO_2 (sulfur dioxide) and COD (chemical oxygen demand) concentrations. The sample period is 2001-2010 in columns (1)-(3) and 2004-2010 in columns (4)-(6) (due to data limitations). Treat is the city-level average of firm-level treatment, with the latter being a dummy variable equal to 1 if firms received any subsidies in the pre-reform period (2001-2006), and 0 otherwise. We use the simple average in columns (1)-(2) and columns (4)-(5), and the pre-reform firm size weighted average in columns (3) and (6). Post is a dummy variable equal to 1 for 2007 and later years, and 0 otherwise. In column (1), the outcome is the logarithm of O_3 (ozone) concentrations derived from satellite observations, which is used as a placebo outcome as it was not targeted by the reform. In column (2), the outcome is the logarithm of industrial SO_2 emissions calculated by summing up emissions from all firms in a city, including those outside our firm sample. In column (3), the outcome is the logarithm of SO_2 concentrations derived from satellite observations. In column (4), the outcome is the inverse distance-weighted average COD readings from monitoring stations upstream and within 100 km of a city, which is used as a placebo outcome as water pollutants cannot flow from low to high. In column (5), the outcome is the simple average COD readings from monitoring stations downstream and within 100 km of a city. In column (6), the outcome is the inverse distance-weighted average COD readings from monitoring stations downstream and within 100 km of a city. The standard errors reported in parentheses are clustered at the city level. * denotes significance at the 10% level. ** denotes significance at the 5% level. *** denotes significance at the 1% level.