Environmental Regulation and Productivity Are Not Always at Odds: Evidence from Firms in China*

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Abstract

Environmental regulations can be costly, but they can also induce innovation that improves productivity if the benefits outweigh the costs, and not all firms face the same costs. We provide the first empirical evidence of a regulation enhancing firm productivity across the industrial sector, showing how differences in compliance costs matter for understanding the impact of regulation on economic activity. We use a heterogeneous difference-in-difference research design to study not only the "dirtiest" firms facing the highest compliance costs but also those in "cleaner" (but still regulated) industries. Productivity increases by 4% for firms in cleaner industries while there is no effect on the dirtiest. This is driven partially by firm sorting. Private firms exit and sales increase for state-owned enterprises. Private firms also invest in higher-skilled labor and management yet there is no effect on new product output, suggesting that they may innovate in their processes and practices.

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1 Introduction

Air pollution is one of today's biggest barriers to economic growth. Not only does it play a pivotal role in driving climate change, but it also creates substantial costs in and of itself. It dampens worker productivity (Graff Zivin and Neidell 2012; Chang, Graff Zivin, Gross and Neidell 2016 2019) and labor supply (Hanna and Oliva 2015), reduces life expectancy (Ebenstein, Fan, Greenstone, He and Zhou 2013), and increases infant mortality (Chay and Greenstone 2003; Arceo, Hanna and Oliva 2015). At the same time, environmental regulations are also costly—particularly for industrial firms that often have to make large capital investments in abatement technology or adjust their production processes to comply—which has generated a long-standing "environment versus economy" discourse amongst policymakers. Indeed, empirical analyses so far have primarily found that environmental regulation hurts firm productivity (Kahn 1997; Greenstone 2002; Greenstone, List and Syverson 2012) and reallocates labor away from regulated industries (Walker 2013).

However, not all firms face the same costs, and this heterogeneity has implications for developing an understanding of how regulation impacts economic activity and competitiveness. While firms typically must invest to some degree, the level of investment required and the strategies firms take to come into compliance, should they choose to do so, depend on their current pollution intensity. Firms that pollute more typically must invest more—such as by replacing old machinery—whereas those that already pollute less may only need to make minor adjustments in their production processes and practices. And in both cases, firms also have incentives innovate to reduce the cost of using pollution-intensive inputs. Such investments may even improve performance and enhance productivity if the benefits outweigh the costs.

In this paper, we study the impact of an environmental regulation on industrial firms' productivity, and importantly, we estimate the effects on firms not only in the most pollution-intensive industries (that face the highest compliance costs) but also those that are "cleaner" (and still regulated). This allows us to capture the *net* effect, and in doing so, we provide the first empirical evidence of environmental regulation enhancing firm productivity on average across the industrial sector.[1] The findings are consistent with what is often considered the "strong" version of the Porter Hypothesis. That is, regulation creates incentives for firms to develop or adopt new technologies, processes, or practices, which can, in turn, enhance productivity (Porter and Van der Linde 1995).

The theory behind the Porter Hypothesis is not new. However, while there is growing evidence that environmental policy and regulation spur innovative activity like R&D and patenting (i.e., the "weak" version of the Porter Hypothesis), relatively little is known about whether this translates into productivity improvements.[2] Greenstone (2002) and Greenstone et al. (2012) offer the most robust analyses of air quality regulation and industrial firm productivity to date, finding that the total factor productivity (TFP) of the most pollution-intensive manufacturing plants declined in response to the U.S. Clean Air Act.[3] As productivity is ultimately the key input into economic growth and competitiveness, continuing to build this evidence base is of first-order importance, and it is increasingly urgent amidst productivity growth declines in developed countries and as developing countries face widespread poverty yet disproportionately bear the burden of pollution.

To help narrow this knowledge gap, we specifically study China's Two Control Zone (TCZ) regulation, which was implemented in 1998 and set objectives for reducing sulfur dioxide (SO_2) emissions in about half of China's prefectures. We use a heterogeneous difference-in-difference research design, exploiting two main sources of variation that determine treatment status—whether a firm is located in a regulated prefecture and before/after variation based on the regulation's implementation timing—and we allow the effect to vary based on whether the firm is in one of the dirtiest or "cleaner" industries.

[1] Hafstead and Williams (2018) also point out the importance of examining the net effect and study the impact of pollution taxes on employment in a general equilibrium model, finding that employment decreased in some industries but increased in others such that the net (negative) effect is small. Our work differs by studying firm productivity and a regulation (rather than labor and taxes), by taking a reduced form approach, and in our findings that there are positive effects even on some directly regulated firms.

[2] See Jaffe and Palmer (1997), Newell, Jaffe and Stavins (1999), Popp (2002), Aghion, Dechezlêpretre, Hemous, Martin and Van Reenen (2016), Calel and Dechezlêpretre (2016), and Calel (2020). In addition to not examining productivity, most of this work also studies market-based interventions like carbon taxes as opposed to command-and-control regulation.

[3] He, Wang and Zhang (2020) more recently also found that regulation reduced firm productivity in China but in the water context, and Bailey (2019) finds that carbon pricing reduces productivity of coal power plants. See Dechezleprêtre and Sato (2017) and Cohen and Tubb (2018) for the latest reviews of the literature. Most work so far on productivity studies it at the industry or country level. For example, Berman and Bui (2001) find that firm productivity increased in response to a regulation for refineries in Los Angeles.

We find that total factor productivity (TFP) increases by 4.3% for firms in less pollution-intensive industries while there is no effect on those in more pollution-intensive industries. The net average effect is thus positive. When we limit the sample to only industries that are "responsive" (i.e., those whose productivity increases), the estimates increase to 16% and 8%, respectively. These are the first results providing evidence that environmental regulation enhances firm productivity across an entire sector to the best of our knowledge. We would have drawn different conclusions that are more in line with the existing literature if we did not also consider the effects on "cleaner" industries, highlighting the importance of considering all regulated firms to understand the implications of environmental regulation for industrial activity and growth.

A natural question that emerges from these results is whether the regulation was enforced and that firms actually took action to reduce their pollution levels. If not, such productivity improvements would not be associated with the regulation itself. We examine this by estimating the effect of the regulation on SO_2 concentration—the pollutant targeted by the TCZ regulation—using satellite-based data at the prefecture-month-year level and find that pollution did indeed decrease by about 5%. This suggests that firms did make adjustments in response to the regulation.

We conduct a series of additional tests to explore the underlying mechanisms driving our results, and taken together, they suggest that firm sorting and innovative activity are likely at play. There are no increases or reallocation of inputs, but sales and value-added increase, leading to higher productivity of labor, capital, and intermediate inputs. Productivity gains are thus associated with an output effect, and firm exit plays a role. The propensity to exit increases by about 5% in response to the regulation but only for firms that were least productive in the pre-regulation period, resulting in a higher average productivity. This type of reallocation across firms is reasonable and brings efficiency improvements, as resource misallocation is common in the developing country context.

Lastly, we explore whether firms innovate in response to the regulation as the Porter hypothesis would suggest. We do not examine the effects on traditional innovation measures like patenting, since industrial firms are unlikely to develop new technologies unless an environmental technology is already their core business. They are more likely to innovate in their processes and practices, and these innovations are less likely to be patented (Hall, Helmers, Rogers and Sena 2013). Rather, we investigate whether firms start investing in high-skilled workers—such as scientists and engineers—and whether they allocate more resources towards management, which is critical for successfully integrating new knowledge into processes and practices for it to affect productivity. We find that both average wages and expenditures on management do indeed increase. Furthermore, there is no change in output specifically associated with new products, suggesting that firms may be innovating in their processes and practices if average wages and management expenditures are indeed good proxies for investment in higher-skilled labor.

One caveat to this conclusion is that we cannot fully rule technology adoption as a mechanism. Although we find no effect on capital, firms could displace old machinery with more efficient and less pollution-intensive machinery, leading to a net-zero effect. That said, since technology adoption typically does not require ongoing high-skilled labor (just workers to operate the equipment) or additional ongoing management expenditures, we find process and practice innovation to be a likely driver of productivity improvements.

To probe the underlying mechanisms further, we also study private firms and state-owned enterprises (SOEs) separately given how incentive structures vary by firm ownership. Environmental regulation evasion is common, and China has a long history of government favoritism of the state sector. Local public officials—who are responsible for enforcing the regulation—typically appoint the executives of SOEs and benefit from the relationships (Barwick, Cao and Li Forthcoming; Lei forthcoming). This creates strong incentives for the government to help SOEs survive, reducing the pressure on public sector firms to innovate.

Our results are consistent with favoritism behavior like this. It turns out that the effects on exit and innovation are almost entirely driven by private firms, despite productivity increasing for both SOEs and private firms. There is no exit of SOEs on average whereas the propensity to exit increases by 5% for private firms, an effect that's driven by older firms only. This aligns with our previous finding that less productive firms are those that exit, as older firms are more likely to rely on older and less efficient machinery. On the other hand, the propensity to exit increases by 15% for both SOEs and private firms that are small, although the number of firms that this equates to is much smaller for SOEs. This is in line with China's "grasping the large, letting go of the small" approach to industrial reformation, but there is still a small effect on larger firm exit, suggesting that China particularly favored larger SOEs when "grasping." Finally, we find positive effects on average wages and management expenditures only for private firms. Considering these heterogeneous effects across firm ownership together points to the regulation being enforced unequally such that SOEs benefit from protectionism, and this comes with potential threats to economic growth and competitiveness given how small private firms are often key drivers of innovation.

Our paper makes several contributions. First and foremost, by studying how environmental regulation impacts not only the most pollution-intensive industries but also those that are "cleaner," we are able to shed new light on how environmental regulation and growth are not always at odds. The two papers closest to ours are Greenstone (2002) and Greenstone et al. (2012), who found that the U.S. Clean Air Act dampened the TFP of manufacturing plants. Our alternative approach to studying how regulation impacts productivity and the conclusions that it leads to complement this work.

The second main contribution of this paper is that we look under the hood of the firm to explore the strategies firms take to comply with environmental regulation and whether these translate into improved productivity. Our exploration of the channels through which firms achieve such outcomes suggests that process and practice innovation at least part of what drives productivity improvements. Even in non-environmental contexts, it is rare to empirically connect innovation to productivity.

Our empirical setting is also important in its own right, as China is the world's heaviest polluter and a significant contributor to global economic activity. The literature focusing on how environmental regulation and quality impacts other firm outcomes in China is growing but less attention has been paid to productivity.[4] One exception is He et al. (2020) who study a water regulation and find that it reduced productivity. Another closely related paper is Fan, Graff Zivin, Kou, Liu and Wang (2019)'s examination of how regulation stringency impacts firm performance, also finding that it declined despite the adoption of new practices.[5]

[4]For example, some have studied how pollution impacts labor demand (Liu, Shadbegian and Zhang 2017; Gray, Shadbegian, Wang and Meral 2014; Liu, Zhang and Geng 2018) and supply (Liu, Tan and Zhang 2021; Fan and Grainger 2021).

[5]Tanaka, Yin and Jefferson (2014), an unpublished working paper, also study how the TCZ regulation impacted firm productivity but they do not have before/after variation.

Lastly, this paper is important and timely for policy in various settings. Our findings challenge the narrative that often dominates political debates, suggesting that intervention aiming to reduce pollution actually may be an effective tool for simultaneously fostering economic development and environmental quality improvements.

This paper proceeds as follows. In Section 2, we discuss the institutional details of our empirical setting and our research design. In Section 3, we describe our data and examine the effect of the regulation on pollution to demonstrate that it "worked" before moving on to our main analysis of productivity. We provide the main results in Section 4, and in Section 5, we explore the underlying mechanisms. We explore differences across firm ownership in Section 6 and conclude in Section 7.

2 Background and Research Design

2.1 Air Pollution and Environmental Regulation in China

China's rapid economic growth has come with significant increases in air pollution. In particular, sulfur dioxide (SO_2) emissions from the industrial sector were a major contributor to China's ambient air pollution through the 1980s and 1990s, which reached 23.7 million by 1995 and created severe acid precipitation in more than 30% of the country's territory (Hao, Wang, Liu and He 2001). According to the 8th Five-Year Plan (1991 to 1995) statistics, SO_2 pollution level exceeded the Class II of Chinese National Ambient Air Quality Standards (CNAAQS) for SO_2 in 149 out of 280 surveyed prefectures.[6] High levels of SO_2 and soot are severely detrimental to human health, with economic losses estimated to be about 95 billion yuan (real value) in the year 1995 (Johnson, Liu and Newfarmer 1997).

This reality and increasing public concern led the Chinese government to introduce a number of environmental regulations, eventually resulting in some of the most comprehensive environmental regulation in the developing world to date. The first was the Air Pollution Prevention and Control Law (APPCL) in 1987 (He, Huo and Zhang 2002). However, it

[6]According to Chinese National Ambient Air Quality Standards, annual average SO_2 concentration level below 20 $\mu g/m^3$ is classified as Class I standard; Class II standard ranges from 20 $\mu g/m^3$ to 60 $\mu g/m^3$; Class III standard is between to 60 $\mu g/m^3$ and 100 $\mu g/m^3$.

provided only a general provision related SO_2 emissions and excluded the power sector, and consequently, had very little impact on reducing SO_2 emissions or acid rain. The government amended the law in 1995 with a new article imposing more stringent regulations on specific regions assigned as acid rain control zones and SO_2 pollution control zones, which became known as the Two Control Zones (TCZ) regulation.

2.2 The TCZ Regulation

The Two Control Zone (TCZ) regulation was enacted in 1998 and aimed to limit China's total SO_2 emissions to be within 2000 levels by the year 2010, achieving urban ambient air sulfur dioxide concentrations that would meet national environmental quality standards. Another goal was to significantly reduce precipitation pH levels relative to 2000 levels. The national government designated prefectures as being SO_2 pollution control zones (i.e., regulated) based on whether the prefecture's average annual ambient SO_2 concentrations exceeded the national Class II standard, whether the prefecture's daily average concentrations exceeded the National Class III standard, and whether "high" SO_2 emissions were recorded. Prefectures were designated as acid rain control zones based on whether their average annual pH values for precipitation were less than or equal to 4.5, sulfate deposition was greater than the critical load, and high emissions were recorded.

These rules resulted in 175 regulated prefectures, spanning regions that accounted for 11.4% of the nation's territory, 40.6% of the population, 62.4% of GDP, and 58.9% of SO_2 emissions according to 1995 figures (Hao et al. 2001). Figure 1 illustrates their geographic distribution.[7]

[FIGURE 1 HERE]

Although some aspects of the regulation were vague, it did lay out specific requirements for some industries, imposing relatively stringent pollution control measures according to nationally-mandated thresholds compared to previous efforts. The regulation particularly targeted industries related to the life cycle of coal, namely coal mining, processing, and combustion, given their particularly high contributions to SO_2 pollution in China. China

[7] We detail our assignment of "regulated" in Section 3.

consumed 963 metric tons carbon equivalent of coal in 1998, accounting for about 30% of the world's coal consumption that year (IEA 2020). The main coal users, like coal power plants, industrial boilers, and kilns, contributed to approximately 35%, 34% and 11% of the total SO_2 emissions in TCZ regions (Hao et al. 2001).

The most explicit control measures were imposed on coal mining and thermal power plants. No new coal mines with sulfur content higher than 3% or no new coal-burning power plants in large and medium-sized prefectures (that were also in TCZ regions) could be built. All new and existing coal mines with sulfur content higher than 1.5% had to be equipped with coal washing facilities. Existing mines producing coal with sulfur content higher than 3% were to be gradually shut down or have output restricted. All new and existing power plants using coal with sulfur content higher than 1% had to be equipped with desulphurization facilities; existing plants were required to take action to reduce SO_2 emissions before 2000 and establish desulfurization facilities by 2010 (Hao et al. 2001).

Other polluting industries were also regulated but they were provided more flexibility in how emissions reductions could be achieved. Firms in chemical, metallurgical, nonferrous metal (including concrete), and building materials industries in TCZs, for example, had to either construct waste gas treatment facilities (e.g., scrubbers) or "take other emissions reduction measures," such as retrofitting industrial boilers and kilns or switching to low-sulfur or washed coal. The TCZ policy also generally promoted clean production and technical renovation in all manufacturing processes to effectively reduce SO_2 emissions.

2.3 Research Design

Our research design leverages two sources of variation created by the TCZ regulation: geographic variation based on whether a prefecture is designated as being regulated and timing variation based on before/after based on the regulation's implementation year. We examine whether productivity changes after implementation are different for firms in TCZ prefectures relative to those that are not regulated. In addition, we allow the effects to vary based on whether the firm is in a more ("dirtier") or less pollution-intensive ("cleaner") industry as measured by the proportion of total SO_2 emissions generated by that industry.[8] Although

[8]Our method for making these assignments are detailed in Appendix A.3.

firms in "cleaner" industries face lower compliance costs, as they usually have to take less extreme measures to meet the requirements, these are polluting industrial firms, so all firms in TCZ prefectures are defined as "regulated."

This heterogeneous difference-in-differences approach allows us to identify the effect of the regulation on firms in both more and less pollution-intensive industries. We estimate the following model throughout our main analyses:

$$log(Y_{it}) = \beta_1(TCZ_p * Post_t) + \beta_2(TCZ_p * Post_t * Polluter_s) + \mu_p * t + \alpha_i + \gamma_{st} + \delta_{sp} + \epsilon_{it} \quad (1)$$

where Y_{it} is firm i's (log) total factor productivity (TFP) (or other outcomes) in year t, TCZ_p is a "regulated" indicator equal to one for firms located in TCZ prefectures (p) and zero otherwise, and $Post_t$ is an indicator equal to one in the post-policy years (from 1999 onwards) and zero otherwise.[9] The variable $Polluter_s$ is an indicator equal to one for firms in more pollution-intensive industries (s) and zero otherwise.

The main coefficients of interest are β_1 and β_2. β_1 captures the regulation's effect on firms in less pollution-intensive industries and β_2 reflects the "extra" impact on the dirtiest firms relative to those that are cleaner. The total effect on the dirtiest firms is the sum of the two coefficients.

The two identifying assumptions of our research design are that: 1) trends in productivity are parallel for regulated and unregulated firms absent the regulation, and 2) there are no spillover effects on firms in non-TCZ prefectures (i.e., the stable unit treatment values assumption (SUTVA) holds). One potential threat to identification is that treated and untreated prefectures may be affected by macroeconomic shocks differently over time. For example, since treatment was not randomly assigned—it was determined by historical pollution levels—the pace of industrialization and development may systematically differ.

To account for this, we include prefecture-specific linear time trends, $\mu_p * t$. The most flexible approach would be to use prefecture-year fixed effects instead, but doing so does not allow us to estimate the effects on less pollution-intensive (but still regulated) firms. It would be equivalent to estimating a triple-difference model and identifying the effects only

[9] We treat the year 1998 as "pre-policy" since there is a delay between the policy's announcement at the central government level and implementation at the local government level.

on the dirtiest firms. In Section 4, we provide evidence that time trends sufficiently control for these concerns, allowing us to address this potential bias while providing a more complete picture of how the regulation affects industrial activity.

We include a rich set of additional fixed effects. Firm-level fixed effects (α_i) control for time-invariant mean differences in outcomes across firms, so our estimates can be interpreted as within-firm effects. Industry-year fixed effects control for how industries may be affected differently by shocks to economic activity (γ_{st}) and industry-prefecture fixed effects control for how industries may be affected differently across prefectures (δ_{sp}). Standard errors are clustered at the prefecture level in our baseline specification, which is the conservative approach relative to clustering at the industry or firm level (see Section 4.2).

3 Data, Productivity Measurement, and Regulation Enforcement

In this section, we provide an overview of our data sources, how we address challenges with the data, and our production function estimation strategy. We also provide evidence that the regulation was enforced before moving on to our main results in the following section.

3.1 Data Overview

To study the TCZ regulation, we match several data sets that provide firm-level, prefecture-level, and industry-level information for China's industrial sector. We start by gathering data on firms for the period 1996 to 2006 from the China Industrial Enterprise Database (CIED), which is maintained through annual surveys conducted by the National Bureau of Statistics. This database includes detailed accounting information that provides us with the variables needed for calculating TFP (value-added, labor, capital, and intermediate inputs), along with other key measures such as gross industrial output, sales, and more.

The CIED data include all state-owned enterprises (SOEs) as well as private firms with annual sales exceeding 5 million Chinese yuan. It covers 40 two-digit Chinese Industrial Classification (CIC) industries, including mining, manufacturing, and public utilities. Although it does not contain the smallest firms in the economy, the aggregate industrial output and employment included represents about 90% and 70%, respectively, of the whole industrial sector according to 2004 figures (Brandt, Van Biesebroeck and Zhang 2012). We keep only firms that appear at least once before and once after 1999, which we mark as the first year that the TCZ policy is in effect. This allows us to include firm-level fixed effects and study the policy impacts using within-firm variation.

This database has been used in a number of economics studies so far (e.g., Hsieh and Klenow 2009; Song, Storesletten and Zilibotti 2011; Brandt et al. 2012; He et al. 2020).[10] We follow the preparation procedures developed by Brandt et al. (2012) that have been widely adopted, such as their approach to matching firms over time and dropping observations that violate standard accounting principles. All nominal financial values are converted to real values (1998) using input and output deflators following Yang (2015) and He et al. (2020). This entails using annual output price indexes for every 2-digit industry to construct output deflators, and for input deflators, using industry-level intermediate input in National Input-Output tables, which allows us to account for the dynamics of input prices in different industries. See Appendix A for more detail.

One difference with the panel we construct relative to others in the literature is that we extend the time covered to 1996-97 when the surveys were being piloted. During these years, mostly only SOEs were included and the sample size is much smaller.[11] Many other studies using this data start from 1998, but given the timing of the TCZ regulation, it's important to have data dating back further to probe the validity of our research design. We consider 1998 a "pre-policy" year to allow for implementation and adjustment time, so we have one year of fully comprehensive data and two years of pilot year data in the pre-policy period.[12] We end up with about 24,000 firms in 1996 and 1997, increasing to around 165,000 firms in 1998 and 301,000 firms by 2006.

To determine whether firms are regulated, we obtain the list of cities designated with TCZ

[10]It is often referred to as the Annual Survey of Industrial Firms.

[11]Although the survey was only intended to include SOEs during these years, some of these firms are actually considered private by the definition we follow described in Appendix A, as they may have gone from being state-owned to private through the period we study.

[12]Many concerns with using the pilot data are alleviated by our analysis being within-firm. We also provide robustness checks dropping the pilot data throughout the paper.

regulatory status from Chinese government documentation (China State Council 1998). We assume the production site is located at the recorded address, as we do not observe whether firms have multiple sites.[13] The government's designations are made at the prefecture level for acid rain control zones and at the district/county level for SO_2 pollution control zones. We assign TCZ status at the prefecture level, defining a prefecture as regulated if it contains TCZ districts or counties, as there were several changes of administrative divisions during the sample period. Moreover, districts and counties within the same prefecture are likely to be governed under the same criteria set by the local administration.

We enhance these data with three additional sources that report prefecture-level characteristics as well as SO_2 emissions and concentration levels. From the China Statistical Yearbook, we gather industry-specific SO_2 emissions intensity information, which allows us to designate firms as being in more or less pollution-intensive industries. We define firms as more or less pollution-intensive if they belong to an industry accounting for at least 1% of total SO_2 emissions, which correlates very closely with coal consumption intensity. Appendix Table C.1 provides a list of all industries in our data set and identifies those that we classify as pollution-intensive, which align closely with the classifications of others in the literature studying the United States (e.g., Greenstone (2002)).

Lastly, for our examination of whether the regulation was enforced, we collect SO_2 pollution data from two sources. First, we gather prefecture-year level SO_2 emissions data from the China Environmental Yearbook. However, these figures are reported by local government officials and may be subject to manipulation (Ghanem and Zhang 2014; Karplus, Zhang and Almond 2018). Therefore, we also follow Chen, Oliva and Zhang (2017) to derive satellite-based SO_2 concentration levels using data from National Aeronautics and Space Administration (NASA).[14] The data are reported monthly at the 60 by 50 kilometer grid level. We match this to Chinese prefectures by re-gridding the satellite data to their geolocations using nearest-neighbor remapping. This results in a balanced prefecture-year-month panel from January 1988 through December 2008. See Appendix A for more detail.

[13] Brandt et al. (2012) examine this and conclude that more than 95% of all observations are single-plant firms.

[14] More specifically, we extract the variable "SO_2 Surface Mass Concentration" from M2TMNXAER version 5.12.4, derived from the project of Modern-Era Retrospective analysis for Research and Applications version 2 (MERRA-2).

3.2 Production Function Estimation

The CIED production and financial information allows us to measure firm-level total factor productivity (TFP) following the method developed in Ackerberg, Caves and Frazer (2015). We estimate production functions separately for each industry using the added value as the dependent variable, capital as a state variable, and intermediate inputs as the proxy variable (all in logs of the value plus one). We include year fixed effects to control for macroeconomic shocks and firm fixed effects to account for unobserved firm-level characteristics as well as age and an indicator for whether the firm is located in a TCZ prefecture as state variables to account for how these firms might install more equipment to reduce emissions relative to those in non-TCZ prefectures. We then use these estimates to construct firm-level TFP . More detail can be found in Appendix A.

We primarily rely on the Ackerberg et al. (2015) approach throughout the paper because it improves upon previously-developed control function methods in two important ways. First, it uses intermediate inputs as the proxy variable as opposed to investments, which is used in Olley and Pakes (1996), to mitigate the "lumpy investments" challenge associated with firm-level data. Second, it corrects the functional dependence problem that both Olley and Pakes (1996) and Levinsohn and Petrin (2003) face.[15] In our robustness checks, we also measure TFP following Levinsohn and Petrin (2003), which still uses intermediate inputs as the proxy variable but assumes that firms adjust immediately after experiencing a productivity shock at no cost, and we examine single-factor productivity outcomes—labor, capital, and intermediate input productivity as measured by added-value divided by each input—as more transparent measures of productivity as well.

One potential concern with our TFP measure is that it is revenue-based rather than quantity-based, so changes in productivity could be associated with firm-specific mark-ups even though we deflate revenues using industry-specific price indices. Unfortunately, data on quantities sold as opposed to revenue are not available, however we explore whether mark-ups drive our results later by limiting the sample to the most competitive and for homogenous

[15]In practice, we implement single-step approach following Rovigatti and Mollisi (2018) with an "ACF correction" rather than the two-step approach of Ackerberg et al. (2015), since the ACF method can be extremely sensitive to the starting points passed to the optimization function. See Rovigatti and Mollisi (2018) for more detail.

goods markets. We discuss this more in Section 4.2 and conduct some robustness checks.

3.3 Pre-Regulation Firm and Prefecture Characteristics

Table 1 presents firm- and prefecture-level descriptive statistics in pre-policy years (1996-1998). As expected since regulated regions were targeted based upon previous pollution levels and this is strongly correlated with the degree of industrialization, firms in TCZ prefectures are more productive, more capital-intensive, and have higher revenues, sales, and profits (Panel A). There are also statistical differences in prefecture-level characteristics: TCZ prefectures have higher GDP per capita, populations, and SO_2 emissions intensity (Panel B). Given these differences, an important component of our identification strategy is controlling for time-invariant prefecture characteristics as well as how they may evolve differently over time, as discussed in Section 2.3.

[TABLE 1 HERE]

3.4 Did the regulation "work?"

Before moving forward with our primary analysis of the regulation's effect on firm productivity, it is important to ensure that the policy was actually enforced and that firms thus faced compliance costs. If not, then any effects we find of the regulation on TFP are likely not actually driven by the regulation. We explore this by examining whether the regulation reduced SO_2—the targeted pollutant—expecting larger declines in TCZ prefectures relative to non-TCZ prefectures if the regulation was actually binding and changed firm behavior.

Starting with a descriptive analysis, we plot the raw SO_2 emissions data from the China Environmental Yearbook (Panel A) and SO_2 concentration data from NASA (Panel B) over time for TCZ and non-TCZ regions in Appendix Figure B.1. While emissions as opposed to concentration most directly capture the pollution created by industrial firms—as concentration is also a function of geographic-specific environmental factors—governments have an incentive to misreport to suggest that they are in compliance. Such manipulation has been documented for Chinese self-reported air pollution data (Ghanem and Zhang 2014; Karplus et al. 2018), making the data from the China Environmental Yearbook data less reliable than data from NASA that is gathered using satellites.

By both measures, pollution decreases by more in TCZ regions. The decline is particularly steep in Panel A, as average emissions drop substantially immediately following the regulation's implementation.[16] This may embed some data manipulation, but the NASA SO_2 concentration data also show a slightly more significant decline for TCZ prefectures, although pollution in non-TCZ prefectures also declines.[17]

To provide further confidence that pollution decreased by more in regulated regions, we estimate the effect of the regulation on SO_2 concentration levels conditional on controls (using the NASA data). In addition to not being subject to manipulation, the NASA concentration data are also more comprehensive, covering more prefectures and with a higher time-resolution (the prefecture-year-month level). We estimate the following model:

$$log(S_{ptm}) = \beta_1(TCZ_p * Post_t) + \alpha_p + \gamma_m + \delta_t + \mu_p * t + \epsilon_{pt} \quad (2)$$

where $\log(S_{ptm})$ is the log of SO_2 concentration levels (micrograms per square meter) in prefecture p in year t and month m. The coefficient of interest is β_1, capturing the effect of the regulation on (log) SO_2 concentration. TCZ_p is an indicator equal to one if the prefecture is regulated by the TCZ regulation and zero otherwise, and $Post_t$ is equal to one in the post-implementation period. We include month fixed effects (γ_m) to control for seasonal differences in weather and economic activity, year fixed effects (δ_t) to control for idiosyncratic shocks to economic or industrial activity in all prefectures, and prefecture-year trends ($\mu_p * t$) to control for how industrial activity may change differently over time for prefectures due to local factors.

The results are presented in Appendix Table C.2. When using the full data set covering 1988 through 2008, we find that the TCZ regulation reduced SO_2 concentration levels by 3.7% (Column 1). Once limiting the sample to the time period that we study in our firm analysis (1996-2006), the effect is enhanced slightly to a 4% reduction (Column 2). We then

[16]There is also a slight decrease in non-TCZ prefecture emissions for one year and they then increase again thereafter.

[17]In both sets of data, pollution increases again after 2002 for all prefectures, which is consistent with industrial activity increasing with China's accession into the WTO.

aggregate the data to the prefecture-year level, using annual average concentration levels as the dependent variable, and find similar results. There is a 4.1% reduction when using data for 1988 through 2008 (Column 3) and a 4.6% reduction when using data for 1996 through 2006 (Column 4). These results suggest that the regulation reduced SO_2 concentration levels in TCZ prefectures relative to reductions that occurred in non-TCZ prefectures.

There is also evidence from previous studies examining the TCZ regulation's impact on other outcomes demonstrating that it was "effective" (Tanaka 2015; Cai, Lu, Wu and Yu 2016), and documentation regarding firm closures and pollution treatment projects suggests that the regulation was enforced as well. About 4,492 high-sulfur coal mines, 784 product lines in small cement and glass plants, and 404 lines in iron and steel plants were closed in TCZ zones by May 2001. Nearly 2,100 treatment projects—including boiler and kiln retrofit, waste gas treatment, flue gas desalinization installation, and fuel-switching to low-sulfur coal—were completed in regulated areas in the first half of 2000 (He et al. 2002).

4 Main Results

4.1 Effect of Regulation on Firm TFP

We begin by visually examining the effect of the TCZ regulation on firm (log) TFP by estimating an event study version of Equation 1 and plot the coefficients in Figure 2 (absorbing industry-prefecture fixed effects) along with their 95% confidence intervals. We allow the effects to differ for firms in less and more pollution-intensive industries following our heterogeneous difference-in-differences research design. Panel A illustrates the effects for firms in less pollution-intensive industries, which correspond to β_1 of Equation 1. In Panel C, we provide the effects on firms in more pollution-intensive industries *relative to the effect on firms in less pollution-intensive industries*, which corresponds to the coefficient of the interaction term (β_2) of Equation 1. We also estimate a separate event study for "dirty" firms only, showing the effects on firms in the most pollution-intensive industries relative to pollution-intensive firms in unregulated regions (Panel B).

[FIGURE 2 HERE]

Figure 2 provides three key insights. First, there are no statistical differences in TFP in pre-policy years (conditional on industry-prefecture fixed effects). The magnitudes of the effects are very close to zero and the trends appear to evolve similarly in pre-policy years. This provides confidence that our first identifying assumption—that there are no other factors generating a systematic difference in TFP trends for firms in TCZ versus non-TCZ prefectures besides the regulation—holds.[18]

Second, TFP begins to increase immediately after the TCZ regulation is implemented for firms in less pollution-intensive industries (Panel A). In Panel C, we also see that there is an immediate and sharp decrease in TFP for firms in more pollution-intensive industries *relative to those in less pollution-intensive industries.* However, there is no effect at all on firms in more pollution-intensive industries relative to dirty firms in unregulated prefectures for the first several years after the regulation is implemented (Panel B). Their productivity relative to non-TCZ firms does not decrease until 2004, which is likely due to another shift in the economy around this time, as we can see that there is a similar decline for firms in less pollution-intensive industries as well (Panel A).[19]

Third, these findings illustrate the importance of identifying the effect on both sets of firms rather than only those in more pollution-intensive industries to fully capture the regulation's impact on industrial activity. When considering them both, we can see that there is a net positive effect, as the regulation enhanced productivity for firms in less pollution-intensive industries and had zero effect on firms in more pollution-intensive industries. If we had only considered the effect on the latter and included the less pollution-intensive firms in the control group, this would produce triple-difference estimates that essentially correspond to those illustrated in Panel C. Our conclusion in that case would have been not only that the regulation reduced the productivity of firms in more pollution-intensive industries, but also that the overall net effect was negative. This also highlights how firms in less pollution-intensive industries in regulated areas embody a poor control group, as they would be in a triple-difference framework. Given how they are indeed affected by the regulation, including

[18] Although the standard errors are large in the years in which data collection was in its pilot stage (1996-97), the point estimates are very similar to the 1998 estimates.

[19] For instance, this is about when all of the tariff changes related to China's accession into the WTO fully were in place, even though they entered the WTO in 2001.

them as part of the control group would bias the results.

Turning to the regression analysis, our results from estimating Equation 1 on firm-level TFP are presented in Table 2. The estimates are consistent with the event study—there is a positive effect on firms in less pollution-intensive industries and zero effect on heavier polluters. We include only firm and year fixed effects in Column 1, add industry by prefecture fixed effects in Column 2, and add industry by year fixed effects and prefecture-specific linear time trends in Column 3.[20] If we take Column 3 with our richest set of controls as the baseline, we find that the TCZ regulation increases the productivity of firms in less pollution-intensive industries by 4.3%. While TFP for firms in more pollution-intensive industries decreases by 4.8% *relative to those in more pollution-intensive industries*, there is no effect on them relative to firms in unregulated regions (i.e., the addition of the two coefficients). The net effect on industrial firms in TCZ prefectures is therefore positive.

[TABLE 2 HERE]

Column 4 of Table 2 presents the results from estimating the triple-difference model of Equation **??**, whereby the effect on less pollution-intensive firms is absorbed by fixed effects. These results help demonstrate the importance of taking a heterogeneous difference-in-difference approach rather than a triple-difference approach. Column 4 suggests that there is a 5.2% decrease in TFP for the heavier polluters. However, these effects capture not just the treatment in comparison to unregulated firms but also regulated ones in less pollution-intensive industries. The negative effect in Column 4 is driven by the fact that there is a significant positive effect on less pollution-intensive firms, and these firms embody a poor control group since they are indeed affected.

Importantly, the -5.2% result of Column 4 is not statistically different from the -4.8% effect in Column 3. This provides evidence that controlling for how prefectures might respond to macroeconomic shocks differently over time with prefecture-specific trends as opposed to fixed effects is sufficient. That is, our results when using the heterogeneous difference-in-difference approach do not appear to be biased by the exclusion of prefecture-year fixed

[20]While the main effects for the post-policy and regulated indicators are absorbed, the main effect for "polluter" and its two-way interactions are not (we omit their estimates from this table). They are not absorbed because industry fixed effects are at the 2-digit CIC level but we break the power industry into thermal power vs. other using their 4-digit CICs.

effects—they add little or no value in this case—while using prefecture-specific trends provides the benefit of allowing us to identify the effects on all firms.

Comparing Column 3 to Column 4 also illustrates why using a heterogeneous difference-in-difference approach rather than a triple-difference approach is important for understanding the implications of regulation for industrial activity. If we took the triple-difference approach, our conclusions would be reversed—the takeaway would be that there is a negative and fairly large effect—yet we see in Column 3 that the regulation's effect on these firms' productivity is actually zero. One key observation here is that, because cleaner firms in regulated regions are included in the control group in Column 4, the estimates reflect the effect on the dirtiest firms not just relative to unregulated firms but also cleaner regulated firms. This helps explain the key difference in the conclusions. Second, though, the results in Column 4 also miss the positive effects on cleaner firms, which are important for fully understanding how the regulation impacts economic activity.

4.2 Potential Threats to Baseline Findings and Robustness Checks

In this section, we conduct a series of additional empirical analyses to explore the validity of our research design and to test whether our results are sensitive to our modeling choices or variable creation.

4.2.1 Addressing SUTVA

A key identification assumption of our heterogeneous difference-in-difference research design is that there are no spillovers or indirect effects of the regulation on firms in non-TCZ prefectures (i.e., the stable unit treatment value assumption (SUTVA) holds). A potential threat to this assumption is that firms in unregulated regions could become more productive once the policy is implemented, since they do not face the regulatory costs that their competitors in regulated regions face. However, this would attenuate our estimates, if anything. Spatial sorting could also introduce bias. Firms that exit could have moved and re-opened in non-TCZ regions. That said, it is extremely costly to move large manufacturing and power plants—most likely much costlier than complying with the regulation for most firms—and coal mining can only occur where coal already exists.

A more likely scenario that could bias our results in either direction is related to migration of high-skilled workers. Recent work has shown how pollution has increased migration away from polluted cities in China, primarily for well-educated workers (Chen et al. 2017; Khanna, Liang, Mobarak and Song 2021). Since non-TCZ regions are less polluted on average, high-skilled workers may have been moving out of TCZ prefectures already, which would increase productivity of firms in non-TCZ regions. Indeed, Khanna et al. (2021) find that pollution-induced migration substantially reduces aggregate productivity when high-skilled workers leave. On the other hand, the TCZ regulation could have induced migration of high-skilled workers towards TCZ regions given the pollution reductions, which could dampen aggregate productivity in non-TCZ regions.

We conduct two sets of analyses to provide evidence that the regulation did not induce migration. First, we classify firms by technology intensiveness according to the OECD (2011)'s criteria and examine whether the regulation has a differential impact on TFP for high- and low-tech firms, assuming that high-tech firms employ more high-skilled workers. We would expect high-tech firm productivity to increase by more than low-tech firms if the regulation induced migration, as more educated workers would raise average productivity. Second, we estimate the regulation's effect on the fraction of firms and workers in each prefecture that are in high-tech industries.[21] If workers or firms in high-tech industries move to TCZ regions, we would expect these measures to increase.

Appendix Table C.3 provides the findings, which are consistent with there being no movement of high-tech workers or firms. In Columns 1 and 2, we use our firm-level data to estimate a variation of the baseline model that interacts a dummy variable equal to one if the firm is in a high-tech industry with our two policy treatment variables. There is no differential impact of the regulation on the productivity of high- and low-tech firms when using our baseline measure of TFP as the dependent variable (Column 1) as well as our alternative TFP measure (Column 2).

In Columns 3 and 4, we aggregate the data to the prefecture-year level and estimate the

[21]This is similar to the approach taken by Fu, Viard and Zhang (2021) in their study of how pollution impacts worker productivity.

TCZ regulation's effects on the share of firms (Column 3) and workers (Column 4) that are in high-tech industries. We include year and prefecture fixed effects as well as prefecture-specific trends. We also control for the total number of firms (for each prefecture) in Column 3 and the total number of workers in Column 4 to account for differences in how high-tech firms and high-skilled workers are more likely to move to prefectures with these characteristics in the first place. In both regressions, the effects are not only statistically insignificant but also extremely close to zero in their magnitudes. These four tests provide confidence that there is no systematic movement of high-tech firms or high-skilled workers between TCZ and non-TCZ regions in response to the regulation.

4.2.2 Measuring Productivity

Many methods for estimating production functions and TFP have been developed in the literature. We use the Ackerberg et al. (2015) approach in the baseline because it corrects the simultaneity concerns of the commonly used Levinsohn and Petrin (2003) and Olley and Pakes (1996) methods, and it uses intermediate materials as the proxy variable rather than investments, which can be lumpy at the firm level. That said, since there tend to be differences in estimated TFP across methods, we estimate the effects of an alternative TFP measure that we construct following Levinsohn and Petrin (2003) to see whether our results are sensitive to our choice. The results are presented in Column 1 of Appendix Table C.4 and are the same as in our baseline.

A second and perhaps more challenging concern related to our TFP measure is that it is revenue-based rather than quantities-based. This introduces the risk of enhanced productivity being associated with firms increasing mark-ups rather than actually improving productivity. Unfortunately, we do not have access to data on quantities, which is frequently the case in the literature. However, we indirectly explore whether mark-ups might be driving the results by seeing whether they hold for homogenous goods markets specifically and for markets that are most competitive. We would expect firms to not increase mark-ups (or at least not by as much) in these contexts, as their market power is limited.

We first estimate our baseline model including only industries producing homogenous goods, such as of electricity and water, smelting and pressing of metals, petroleum processing, and mining.[22] The results become stronger (see Column 2 of Appendix Table C.4). Productivity increases for both clean and dirty firms by 8%, although the coefficient for dirty firms relative to clean is negative and just not statistically significant.

Next, we estimate the effects for firms in industries that appear to be the most competitive in the year just prior to policy implementation. Our first approach is to calculate the average market share and HHI for each industry—using sales to determine market shares—and include only industries that fall in the bottom quartiles of these distributions. The findings are presented in Columns 3 and 4 of Appendix Table C.4 and are very similar to the baseline. Lastly, we find the number of firms in each industry in the last year just before policy implementation and estimate the effects only for firms in industries in the top quartile of the distribution (Column 5). The results are again about the same as in the baseline.

4.2.3 Additional Robustness Checks

We conduct a few final tests related to some potential issues in the database and other economic activities throughout the time period that we are studying and present the findings in Appendix Table C.5. In Column 1, we drop the years in which the database was in pilot mode (1996-1997), since this is not as comprehensive of a sample and primarily includes SOEs. The magnitude and statistical significance decreases for firms in less pollution-intensive industries, but it remains positive and statistically significant, and the overall effect for firms in more pollution-intensive industries remains statistically zero. In Column 2, we control for China's accession into the the WTO by including a dummy variable for years after 2001 and also interacting this dummy with the TCZ dummy in case firms in TCZ and non-TCZ prefectures were affected differently. For example, firms that were already more advanced and in industrialized regions may have been more likely to be exporters. The results become stronger, if anything.[23]

In Column 3, we drop prefectures for which the number of unique firms is below 200 to ensure that we are comparing similarly-industrialized prefectures. In Column 4, we drop firms for which their industry CIC changed over our sample period, and in Column 5, we drop

[22] Examples of industries that we do not define as those with homogenous goods are manufacturing of medical and pharmaceutical products, food and beverage manufacturing, and special machinery manufacturing.

[23] We explore the importance of entry into the WTO more later.

the year 2003 since we needed to use an alternative labor variable for this year (see Appendix A for detail). The results remain statistically significant and have similar magnitudes to the baseline findings. Finally, we ensure that our results hold when clustering standard errors at different levels. We cluster by industry in Column 6 to account for how treatment intensity varies by industry, and in Column 7, we cluster at the firm level to account for possible serial correlation in the dependent variable. Our results become stronger in both cases.

Lastly, we conduct the same series of robustness checks but using the alternative TFP measure following Levinsohn and Petrin (2003) and provide the findings in Appendix Table C.6. The estimates for firms in less pollution-intensive industries range from 3.5% to 5.3% and remain statistically significant, and the effects on the heavier polluters are still zero.

5 Mechanisms: Why Does Productivity Increase?

We now turn to investigating the underlying mechanisms of our findings and the strategies firms take to comply with the regulation. In doing so, we examine industries with productivity gains versus no effect or losses separately, and we also explore heterogeneity by firm ownership given the political economy of regulation in China. We summarize what conclusions we believe can be drawn about the mechanisms at play in Subsection 5.6.

5.1 "Responsive" vs. "Unresponsive" Industries

So we can explore the mechanisms that drive productivity gains, we start by estimating the effects on TFP for each industry separately at the 2-digit CIC level to identify industries in which the regulation enhanced productivity as opposed to reducing it or having no effect. The results are presented in Appendix Table C.7. It turns out that, even though there were no effects on TFP for heavier-polluting firms on average, productivity does actually increase for three major "dirty" industries: smelting and pressing of ferrous metals (i.e., the steel industry), chemical materials and products, and textiles. This is intuitive as these are industries that are coal-intensive in their production processes, thus facing high compliance costs and having the incentive to make significant adjustments.[24]

Of the less pollution-intensive industries, TFP increases in petroleum and natural gas extraction, ferrous metals mining and dressing, beverage manufacturing, and tobacco processing. The first two industries are not particularly coal-intensive polluters on their own, but they are upstream to pollution-intensive industries. The second two fall within the broader category of industrial polluting-firms being "encouraged" to reduce SO_2 emissions under the regulation.[25]

Before moving on to explore what drives productivity improvements for "responsive" industries as well as whether non-responsive industries may have responded to the regulation but in ways that did not enhance productivity, we re-estimate our baseline model and main set of robustness checks to find the average effect on responsive industries and non-responsive industries separately. The results are provided in Table 3. Unsurprisingly, the positive effects become much stronger for firms in less pollution-intensive firms and become positive and large for firms in more pollution-intensive industries. The effects on TFP are 16% and 8%, respectively, when using our baseline model in Column 1. On the other hand, when including the "non-responsive" firms together as a sub-sample, we can see that productivity decreases for firms in the dirtiest industries by about 6%. These findings therefore suggest that they do indeed in respond as well but in ways that do not enhance their productivity, which we further explore in the next sub-sections.

[TABLE 3 HERE]

5.2 Inputs, Output, and Single-Factor Productivity

One potential explanation of changes in productivity is input reallocation or changes in input levels. In the industrial sector context, technology adoption is often an important channel

[24]Steel and chemicals, for instance, each account for 5-6% of total SO_2 emissions in China and the textiles industry accounts for 1-2% of emissions. There are no effects on any other dirty industries. Interestingly, there is no effect on the productivity of thermal power and coal mining firms, the two industries that faced the most specific regulatory requirements with thresholds to meet.

[25]The only two industries with declines in TFP were natural gas suppliers (CIC 45) and transportation equipment manufacturing (CIC 37). Both of these are low SO_2 emitters. The latter has a small sample size, and when grouped with other equipment manufacturing, the effect goes away. We cannot point to an explicit mechanism for natural gas suppliers' decreased productivity, but one potential explanation is substitution between gas and cleaner coal.

for reducing emissions (Popp 2011; Dechezleprêtre, Glachant and Ménière 2013), and end-of-pipe and scrubber solutions are large capital investments. Labor could also increase since continuous operation of pollution abatement equipment tends to require more workers. On the other hand, firms could find innovative ways to use their capital, labor, and intermediate inputs more efficiently, such as by using lower sulfur-content fuel in their production processes (which tends to be more efficient) or adopting new management practices that improve operational efficiency without changing inputs, resulting in increased single-factor productivity via an output channel.

We test whether productivity increases through the input or output channels by estimating the regulation's effects on inputs (labor, capital, and intermediate inputs), output (value-added and sales), and single-factor productivity, which we define as value-added divided by each input. The results are presented in Panels A and B of Table 4 for responsive and non-responsive industries, respectively.

The overall takeaway from this set of results is that productivity gains for firms in responsive industries appear to be driven by an output effect whereas firms in non-responsive industries start scaling down production. For responsive firms, there are no changes in input (Columns 1-3), but both value-added and sales increase by 17% and 10%, respectively (Columns 7 and 8), leading to increases in all three measures of single-factor productivity (Columns 4-6). On the other hand, labor and intermediate inputs both decrease for dirty firms in non-responsive industries, but capital increases for relatively clean. As value-added and sales decrease in these industries for dirty firms while there is no change in output for their relatively clean counterparts, these findings suggest that the cleaner firms invest in new machinery—and they can bear the costs without it hurting performance—whereas the dirtiest firms (facing the highest compliance costs) start to scale down production but not yet retiring old equipment.

[TABLE 4 HERE]

5.3 Firm Sorting

Another potential explanation for improved productivity is that the regulation drove out inefficient firms. Resource misallocation is common in developing countries, which has been shown to be the case for China (Hsieh and Klenow 2009; He et al. 2020), and turnover and reallocation across firms could enhance average productivity. While He et al. (2002) document exit of coal mining firms, this is not one of the industries for which productivity increases, so further investigation of whether firm sorting can explain the results is needed.

To explore whether this occurs, we estimate the effects of the regulation on exit for responsive and non-responsive firms including the full sample and then by splitting the samples by their median pre-policy productivity level (see Table 5). On average, there is no effect on exit for responsive and non-responsive industries, but when splitting the sample, we can see that the least productive firms do indeed exit. The regulation increases the propensity to exit by 5% for firms in responsive industries and 2.5% for those in non-responsive industries. These findings suggest that efficient firm exit may be at play and we explore this further later when examining private and state-owned enterprises separately.

[TABLE 5 HERE]

5.4 Human Health and Worker Performance

There is increasing evidence in the literature that pollution impacts worker productivity and performance as well as cognition, and thus another potential channel through which the regulation impacts productivity could be through the pollution reduction itself. However, two pieces of evidence from preceding sections suggest that this is not at play. First, when considering single-factor productivity, we would expect only (or primarily) labor productivity to be significant if health can explain the findings, but we find that capital- and intermediate input productivity also increase substantially, and the magnitudes of the effects across the three measures are similar. Furthermore, we would expect the effects to be largest for workers in high-tech industries that are more likely to require high-skilled labor. We find no statistical differences for firms in high- and low-tech industries, however, when we explore this in Section 4.2.1.

5.5 Induced Innovation

Lastly, productivity improvements could be driven by innovative activity. Firms can innovate in various ways, such as by developing and adopting new technologies, processes, and practices. We first explore this potential channel by examining whether firms start investing in more high-skilled labor (e.g., scientists and engineers), estimating the effects on average wages (Column 1 of Table 6. Average wages should increase if firms engage in innovative activities, as this is the type of human capital required. We find that this is indeed the case for both responsive and non-responsive cleaner firms but the effect is about double the size for responsive firms—responsive firms increase average wages by 8.8% and non-responsive firms' average wages increase by 4.5%.[26] Dirty firms also increase average wages by 8.8% in responsive industries and 2.3% in non-responsive industries.

If high-skilled labor contributes to productivity improvements, these findings raise the question as to why there is no change in productivity for non-responsive industries as well. One explanation could be management quality. A key input required for turning the knowledge produced by such workers into productivity gains is high-quality management of their activities and adoption of the newly created innovations. We estimate the effects on expenditures specifically on management and find that it is only cleaner responsive firms that invest more in management in response to the regulation, suggesting that more time and resources allocated to management are necessary to translate the high-skilled human capital into improved productivity.

Finally, if these results do reflect innovative activity, understanding whether firms are producing new technologies as opposed to improving their processes and practices can provide deeper insight into the strategies they take to comply with the regulation. We estimate the effects of the regulation on new product output and its percentage of total output (Columns 3 and 4 of Table 6) to test this, and the results are consistent with innovating in processes and practices, as there are no effects on new product output for any set of firms. This aligns with the type of innovative activity we would expect for these firms as well—unless a firm's main product is already an environmental technology, they are unlikely to start producing one due to the regulation as opposed to either adopt new technologies or adjust

[26] Since labor does not change, these effects are driven by higher wages per worker.

their operations in ways that help reduce pollution.

[TABLE 6 HERE]

5.6 Summary of Mechanisms

Taken together, these sets of results suggest that firms achieve productivity improvements through an output effect rather than by reallocating or increasing inputs, and there are two likely mechanisms underlying the main findings. First, firms that were previously less productive exit, increasing average productivity. This is intuitive given how the additional costs imposed by the regulation could make it unmanageable to compete with firms that are already more productive. Second, although we remain cautious in our interpretation, our findings suggest that the regulation may have induced process and practice innovation. Moreover, when firms do invest in higher-skilled human capital, investing more time and resources into management appears to be important for innovation to improve productivity.

6 Heterogeneity by Firm Ownership

We now probe the underlying drivers of our findings further by examining exit and innovative activities for private firms and state-owned firms (SOEs) separately. Given China's long history of protecting SOEs (Eaton and Kostka 2017)—such as by favoring them in product and services purchasing (e.g., public procurement) to help them survive or subsidizing them such that they face lower or zero compliance costs and have no incentive to change behavior—the results of the previous subsection might mask important heterogeneity in firms' strategies that can shed light on the political economy of environmental regulation. Furthermore, such heterogeneity may have implications for growth and competitiveness given how private firms are on average significantly more productive than state-owned enterprises in China.

First, we estimate the effects of the regulation on exit for firms in responsive industries for SOEs and private firms. We start by using the full baseline samples (Column 1) and then split them based on firm age (Columns 2-3) and size (Columns 4-5), as evidence in the innovation economics literature has shown that start-up firms are more likely to contribute significant innovations relative to older and larger firms.

We find that it is almost entirely private firms that exit (see Table 7). On average, the propensity for private firms to exit increases by 5% whereas there is no effect on SOEs, and it is only older private firms that exit. This is in line with our previous findings of less productive firms exiting, as older firms are more likely to be using older and less efficient machinery, and it also begins to suggest that local government officials responsible for the regulation's enforcement may be protecting SOEs. Such behavior is not uncommon in China—local officials typically appoint the executives of SOEs and benefit from the relationships (Barwick et al. Forthcoming; Lei forthcoming). This creates strong incentives for the government to help SOEs survive, reducing the pressure on state sector firms to invest in pollution-abatement technology or activities. On the other hand, the propensity to exit increases by about 15% for both state-owned and private small firms, although the sample size is much larger for private firms, so the level effect is much smaller for SOEs. This is consistent with China's known practice of protecting larger firms when new regulations are imposed, and although the findings overall are still mostly driven by private firm exit, this does provide evidence of there being some regulation enforcement for SOEs as well.

[TABLE 7 HERE]

Finally, we examine whether both SOEs and private firms engage in innovative activity, as this was the second mechanism underlying our baseline results. The results from estimating the effects on average wages (Column 1), management expenditures per worker (Column 2), new product output per employee (Column 3), and the percentage of output associated with new products (Column 4) are presented in Table 8 for SOEs and private firms. If we interpret these outcomes as we did before, the findings indicate that it is only private firms that innovate. There are no effects on any of these outcomes for SOEs. Instead, the increases in average wages are entirely accounted for by private firms, as they increase by 14% for cleaner firms and 7% for dirtier firms. Furthermore, private firms' expenditures in management increase by 10% for cleaner firms (and do not change for dirty private firms). There are again no effects on new product output for either sample, suggesting that private firms' investments in higher-skilled human capital go towards improving operational efficiency rather than developing new technologies.

[TABLE 8 HERE]

This heterogeneity suggests that the regulation is likely enforced primarily for private firms—and private firms that do not exit respond in ways that lead to productivity improvements—shedding light on the political economy of environmental regulation. It also provides new insight into how such favoritism and protection of the state sector impacts China's competitiveness. Given how younger and smaller firms typically are important drivers of innovation relative to their older and larger counterparts, and how private firms are far more productive than SOEs in China on average, the exit of these exact sets of firms may have negative consequences for economic growth. If their exit is indeed due to unequal enforcement for SOEs and private firms, political protectionism may be a detriment to the potential productivity gains that could be achieved through environmental regulation.

7 Conclusion

Policy goals related to protecting the environment and driving economic growth are often pitted against each other, implying that their objectives cannot be achieved simultaneously. Our paper shows that they are not always at odds. By estimating the effects of an environmental regulation on firms across the entire industrial sector, we find that the net effect on productivity is positive, suggesting that environmental regulation can actually be an effective tool for fostering development.

Our results importantly rely on considering how regulation impacts not just the "dirtiest" industries that face the highest compliance costs. Ignoring the effects on less pollution-intensive industries leaves out a critical component of industrial activity, and if we had done so, we would have drawn the opposite conclusions. Such effects should be included when assessing the costs and benefits of environmental regulation.

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MAIN TEXT TABLES

Table 1: Descriptive Statistics in Pre-Regulation Period (1996-1998)

	Means			St. Deviations		Observations	
	TCZ	Non-TCZ	Difference	TCZ	Non-TCZ	TCZ	Non-TCZ
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Firm-Level Characteristics							
TFP (log)	5.483	5.427	0.056***	1.569	1.652	122,243	38,609
Labor (log)	5.466	5.493	-0.027***	1.331	1.401	122,245	38,675
Capital (log)	9.192	9.076	0.117***	1.881	1.877	122,245	38,675
Capital-Labor Ratio	87.51	69.38	18.13***	308.33	225.89	122,245	38,675
Revenue (millions)	75.54	61.64	13.90***	422.95	626.97	122,245	38,675
Sales (millions)	76.24	64.24	12.00***	418.48	630.07	122,245	38,675
Profit (millions)	3.22	2.07	1.15**	52.44	126.85	122,245	38,675
Panel B: Prefecture-Level Characteristics							
GDP per capita (10,000s)	9,665	6,579	3,086**	10,252	5,130	140	81
GDP growth rate (%)	12.03	15.51	-3.48	11.47	41.17	140	81
Population (10,000s)	426.46	346.95	79.51*	320.81	244.20	140	81
SO_2 emissions (t/km^2)	60.87	31.36	29.51***	84.25	45.91	138	74
SO_2 concentration (ug/m^3)	15.94	9.36	6.58***	9.71	9.43	22,836	22,968

Notes: Table provides descriptive statistics of firm-level (Panel A) and prefecture-level (Panel B) characteristics in the pre-policy period. All monetary values are in real 1998 Yuan and variables are constructed as explained in Appendix A. All prefecture-level statistics are drawn from yearly data except for the NASA SO_2 data, which is monthly. Asterisks denote *p <0.10, **p <0.05, ***p <0.01.

Table 2: Effect of the TCZ Regulation on Firm TFP

Outcome Variable (log):	TFP (1)	TFP (2)	TFP (3)	TFP (4)
TCZ * Post-Policy	0.035	0.035	0.043**	
	(0.022)	(0.022)	(0.022)	
TCZ * Post-Policy * Polluter	-0.037*	-0.037*	-0.048***	-0.052***
	(0.020)	(0.020)	(0.017)	(0.017)
Observations	762,957	762,957	762,957	762,922
Firm FEs	x	x	x	x
Year FEs	x	x		
Industry x Prefecture FEs		x	x	x
Industry x Year FEs			x	x
Prefecture x Year Trends			x	
Prefecture x Year FEs				x

Notes: Table reports the effects of the TCZ regulation on (log) TFP with various sets of fixed effects. In Column 1, we include only firm and year fixed effects. We add industry-by-prefecture fixed effects in Column 2. In Column 3, we include prefecture-by-year linear trends, and in Column 4, we include prefecture-by-year fixed effects. The effect on firms in less pollution-intensive industries is in the first row and the effect on firms in more pollution-intensive industries is in the second row. The two-way interactions are included as well as the "polluter" indicator. The other two main effects are absorbed by the fixed effects. Standard errors are clustered at the prefecture level. Asterisks denote *p <0.10, **p <0.05, ***p <0.01.

Table 3: Effects on TFP for Firms in "Responsive" vs. "Non-Responsive" Industries

Outcome Variable (log):	TFP (1)	TFP (2)	TFP (3)	TFP (4)	TFP (5)	TFP (6)	TFP (7)	TFP (8)
Panel A: "Responsive"								
TCZ * Post-Policy	0.160***	0.117***	0.161***	0.174***	0.141***	0.155***	0.160***	0.160***
	(0.041)	(0.039)	(0.042)	(0.042)	(0.041)	(0.041)	(0.040)	(0.035)
TCZ * Post-Policy * Polluter	-0.077*	-0.032	-0.077*	-0.092**	-0.056	-0.069*	-0.077*	-0.077**
	(0.041)	(0.041)	(0.041)	(0.042)	(0.042)	(0.041)	(0.032)	(0.038)
Observations	144,451	135,835	144,451	142,595	138,311	132,264	144,451	144,451
Panel B: "Non-Responsive"								
TCZ * Post-Policy	0.025	0.021	0.037	0.026	0.023	0.026	0.025*	0.025***
	(0.022)	(0.021)	(0.023)	(0.022)	(0.021)	(0.022)	(0.014)	(0.009)
TCZ * Post-Policy * Polluter	-0.063***	-0.068***	-0.063***	-0.063***	-0.061***	-0.060***	-0.063**	-0.063***
	(0.020)	(0.021)	(0.020)	(0.021)	(0.021)	(0.020)	(0.028)	(0.014)
Observations	618,501	592,466	618,501	607,662	583,479	564,245	618,501	618,501
Baseline	x							
Drop Pilot Data		x						
Control for WTO Entry			x					
Drop if Low Firm Count				x				
Drop Changed Industry					x			
Drop 2003						x		
Cluster SEs by Industry							x	
Cluster SEs by Firm								x

Notes: Table reports the regulation's effects on TFP (log) when including only firms in "responsive" industries (i.e., industries for which the regulation enhanced TFP). Column 1 is comparable to the baseline main results in Column 3 of Table 2. In Columns 2-7, we conduct the same set of robustness checks as we do for for the full sample in Appendix Table C.5. All baseline FEs are included. Standard errors are clustered at the prefecture level in all cases except Columns 7-8. Asterisks denote *p <0.10, **p <0.05, ***p <0.01.

Table 4: Effects on Inputs, Output, and Single-Factor Productivity

Outcome variable (log):	Labor (1)	Capital (2)	Intermed. (3)	VA/L (4)	VA/K (5)	VA/M (6)	VA (7)	Sales (8)
Panel A: "Responsive"								
TCZ * Post-Policy	0.011	0.030	0.065	0.159***	0.140***	0.106***	0.170***	0.102***
	(0.023)	(0.036)	(0.041)	(0.041)	(0.045)	(0.033)	(0.044)	(0.039)
TCZ * Post-Policy * Polluter	-0.007	-0.016	-0.023	-0.077*	-0.068	-0.061*	-0.084*	-0.055
	(0.025)	(0.034)	(0.040)	(0.040)	(0.044)	(0.032)	(0.044)	(0.039)
Observations	144,512	144,512	144,512	144,512	144,512	144,512	144,512	144,393
Panel B: "Non-Responsive"								
TCZ * Post-Policy	0.018	0.031**	0.018	0.018	0.005	0.019	0.037	0.026
	(0.012)	(0.014)	(0.019)	(0.022)	(0.023)	(0.019)	(0.023)	(0.019)
TCZ * Post-Policy * Polluter	-0.044***	-0.045***	-0.070***	-0.038**	-0.037	-0.012	-0.082***	-0.070***
	(0.013)	(0.015)	(0.019)	(0.019)	(0.022)	(0.018)	(0.022)	(0.018)
Observations	618,723	618,723	618,723	618,723	618,723	618,723	618,723	618,175

Notes: Table reports the effects of the TCZ regulation on inputs (labor, capital, and intermediate inputs in Columns 1-3) and single-factor productivity (measured as value-added over each input in Columns 4-6). Firms in responsive industries are included in Panel A and those in non-responsive industries are in Panel B. All baseline FEs are included. Standard errors are clustered at the prefecture level. Asterisks denote *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

Table 5: Effect of the TCZ Regulation on Firm Exit

Outcome Variable:	Exit	Exit	Exit
Sample:	Full	Least Productive	Most Productive
	(1)	(2)	(3)
Panel A: "Responsive"			
TCZ * Post-Policy	0.010	0.050*	0.014
	(0.016)	(0.026)	(0.017)
TCZ * Post-Policy * Polluter	0.012	-0.015	0.011
	(0.015)	(0.025)	(0.017)
Observations	144,512	64,592	79,914
Panel B: "Non-Responsive"			
TCZ * Post-Policy	0.010	0.025**	0.007
	(0.009)	(0.011)	(0.010)
TCZ * Post-Policy * Polluter	0.012	0.008	0.008
	(0.008)	(0.010)	(0.009)
Observations	618,723	283,309	335,411

Notes: Table reports effects of the TCZ regulation on exit for responsive (Panel A) and non-responsive (Panel B) industries. The full sample is included in Column 1 and the samples are split by the median pre-policy productivity levels in Columns 2 and 3. All baseline FEs are included. Standard errors are clustered at the prefecture level. Asterisks denote *p <0.10, **p <0.05, ***p <0.01.

Table 6: Evidence of Innovative Activity: Effects on Wages, Management Expenditures, and New Products

Outcome Variable (log):	Avg. Wages (1)	Manage. Exp per Worker (2)	New Product Output (3)	New Product % of Output (4)
Panel A: "Responsive"				
TCZ * Post-Policy	0.088***	0.074**	0.104	-0.001
	(0.028)	(0.035)	(0.089)	(0.004)
TCZ * Post-Policy * Polluter	-0.026	-0.079*	-0.043	-0.001
	(0.027)	(0.043)	(0.085)	(0.004)
Observations	143,863	142,983	126,374	126,374
Panel B: "Non-Responsive"				
TCZ * Post-Policy	0.045***	0.007	0.036	-0.001
	(0.014)	(0.016)	(0.062)	(0.002)
TCZ * Post-Policy * Polluter	-0.022*	-0.022	-0.022	-0.001
	(0.013)	(0.018)	(0.045)	(0.002)
Observations	615,953	608,502	548,806	548,806

Notes: Columns 1 and 2 provide the effects on average wages and expenditures on management per employee, respectively. Columns 3 and 4 provide the effects on new product output level and its percentage of total output, respectively. All baseline FEs are included. Standard errors are clustered at the prefecture level. Asterisks denote *p <0.10, **p <0.05, ***p <0.01.

Table 7: Effect on Exit in Responsive Industries by Firm Ownership, Age, and Size

Outcome Variable: *Sample:*	Exit Full (1)	Exit Young (2)	Exit Older (3)	Exit Small (4)	Exit Larger (5)
Panel A: State-Owned Firms					
TCZ * Post-Policy	0.008	0.144	0.009	0.156*	-0.003
	(0.024)	(0.151)	(0.024)	(0.082)	(0.023)
TCZ * Post-Policy * Polluter	0.025	-0.088	0.026	0.088	0.033
	(0.024)	(0.161)	(0.024)	(0.087)	(0.024)
Observations	34,134	880	33,004	3,095	30,459
Panel B: Private Firms					
TCZ * Post-Policy	0.052**	0.043	0.054**	0.153**	0.033*
	(0.020)	(0.056)	(0.023)	(0.071)	(0.018)
TCZ * Post-Policy * Polluter	-0.018	-0.016	-0.016	-0.053	-0.003
	(0.018)	(0.060)	(0.020)	(0.075)	(0.017)
Observations	108,670	5,680	101,247	11,859	94,697

Notes: Table reports effects on exit by firm ownership, age, and size for firms in responsive industries. The full sample is included in Column 1. Firms that are younger and older than 5 years old are included in Columns 2 and 3, respectively. Firms with fewer or more than 50 employees are included in Columns 4 and 5. All baseline FEs are included and standard errors are clustered at the prefecture level. Asterisks denote *p <0.10, **p <0.05, ***p <0.01.

Table 8: Effect on Wages, Management Expenditures, and New Product Output by Firm Ownership

Outcome Variable:	Avg. Wages (1)	Manage. Exp per Worker (2)	New Product Output (3)	New Product % of Output (4)
Panel A: State-Owned Firms				
TCZ * Post-Policy	0.027	0.070	0.129	-0.001
	(0.036)	(0.047)	(0.127)	(0.005)
TCZ * Post-Policy * Polluter	-0.003	-0.052	-0.216	-0.005
	(0.040)	(0.055)	(0.142)	(0.006)
Observations	33,863	33,712	27,933	27,933
Panel B: Private Firms				
TCZ * Post-Policy	0.143***	0.096*	0.048	-0.002
	(0.035)	(0.049)	(0.125)	(0.007)
TCZ * Post-Policy * Polluter	-0.070**	-0.107*	0.053	0.003
	(0.034)	(0.060)	(0.119)	(0.006)
Observations	108,310	107,593	96,980	96,980

Notes: Table reports effects on average wages (Column 1), management expenditures (Column 2), new product output per employee (Column 3), and the percentage of output that is from new products (Column 4) by firm ownership. All baseline FEs are included and standard errors are clustered at the prefecture level. Asterisks denote *$p<0.10$, **$p<0.05$, ***$p<0.01$.

MAIN TEXT FIGURES

Figure 1: Geographic Location of TCZ vs. Non-TCZ Prefectures

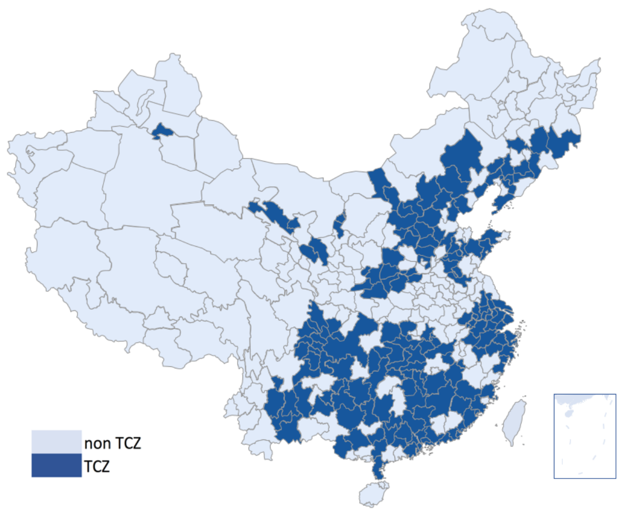
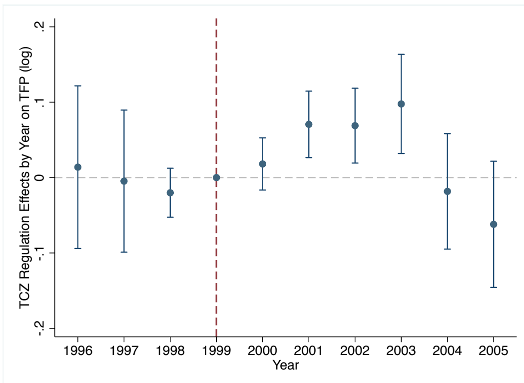
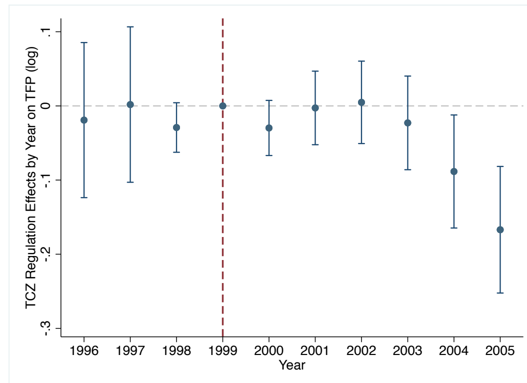


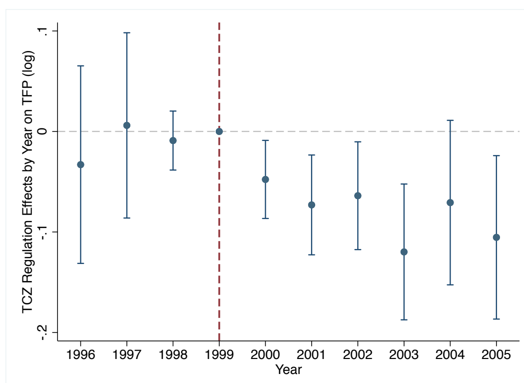
Figure 2: Event Study for Effects of TCZ Regulation on Firm Productivity



(a) TCZ Regulation Effect on Firms in Less Pollution-Intensive Industries



(b) TCZ Regulation Effect on Firms in More Pollution-Intensive Industries



(c) TCZ Regulation Effect on Firms in More Pollution-Intensive Firms *Relative to Firms in Less Pollution-Intensive Industries*

Note: Figure plots the coefficients from an event study regression version of Equation 1 (absorbing industry-prefecture fixed effects) estimating the effect of the TCZ regulation on (log) TFP along with their 95% confidence intervals. Panel A presents the findings for less pollution-intensive firms and Panel B presents the findings for more pollution-intensive firms relative to firms in the unregulated areas. Panel C presents the effects on more pollution-intensive firms *relative to less pollution-intensive firms in regulated prefectures.* The findings in Panel A demonstrate that productivity increases for less pollution-intensive firms immediately following policy implementation. For more pollution-intensive firms, there is no effect on productivity relative to unregulated firms (Panel B), however their productivity decreases relative to less pollution-intensive firms (Panel C).

A Appendix: Data Preparation – For Online Publication Only

A.1 Firm-level data

We obtain annual firm-level data for the period 1996-2006 from the China Industrial Enterprise Database (CIED). The database provides three types of variables: basic information (firm ID, location, total employment etc.), production information (main product, industrial output etc.) and financial information (capital stock, revenue, profit, wage etc.). In our treatment of the data, we follow others in the literature and draw heavily from Brandt et al. (2012). We link firms over time using firms' numerical ID, and where possible, other information including firms' names, legal person, phone number, city code, founding year, industry code etc. We match the sample of two consecutive years first and then expand it to three consecutive years. For more details, please refer to Brandt et al. (2012)'s appendix. One extension that we implement beyond their approach is that we also include the pilot year data from 1996-1997. Although there were some changes in the format of firm ID codes, we are able to match a large portion of the data (approximately 70%) based on the other information, and including these data are helpful for the methods we use in this paper since they expand our pre-policy period.

The CIED contains two variables concerning employment—the number of employees at the end of the year and the average number throughout the year. We use the former to represent employment for all years except 2003, where we use the latter as the former is missing for the year 2003. We conduct a robustness check that drops 2003 in the paper to ensure that this does not affect our results.

We drop observations that appear to contain errors in the key variables that we use. That is, we drop observations for which employment, wages, capital, added value, or gross industrial output are negative (2.5% of the observations). We also drop observations for which we are missing labor or fixed assets data and cases that violate standard accounting principles: observations for which the sum of liquid assets and fixed assets are higher than total assets, current assets are higher than total assets, or fixed assets are greater than total assets (0.08%). At this point, we have an unbalanced panel of 1,874,627 observations for 506,414 unique firms from 1996-2006.

Firm Ownership

We categorize firms as being either "state-owned" or "private" according to their paid-up capital sources. If a firm receives more than 50% paid-up capital from state sources in that year, we consider it to be a state-owned enterprise (SOE). We define all other firms as being private, including foreign firms, which are those that receive less than 50% paid-up capital from the state and more than 25% from foreign sources and Hong Kong, Taiwan and Macao sources.

Firm Industry

The database covers 40 two-digit Chinese Industrial Classification (CIC) industries, including those in mining, manufacturing, and public utilities. Given that the industry-level SO_2 emission data is only available at the 2-digit industry level (see below), we categorize firms at two-digit industry level. In 2003, the industrial code classification system was revised and several changes were made. To make industry codes comparable, we adjust 1996-2002 observations' industry codes according to the post-2003 version. The industry code used changes for firms sometimes, so we use the mode of industry codes for each firm as the assigned industry for that firm for all years so that we can assign it as being more or less pollution-intensive in our heterogeneous difference-in-differences framework.[27]

Firm Age

We assume all firms are founded after the year 1800 and consider founding year missing if firms reported an earlier founding year. If firms have indicated different founding years in each survey, we use the mode of founding years within-firm for calculating the firm's age.

A.2 Prefecture-level data

We collect sociodemographic prefecture data from China City Statistical Yearbook. This yearbook contains variables both at prefecture and district level. As we assign TCZ status at prefecture level, we use all variables at prefecture level to be consistent. The data is primarily used to examine the pre-policy period prefecture-level characteristics, such as GDP per capita and population. We also collect prefecture-level SO_2 data from China Environmental Yearbook and NASA MERRA-2 to test the effectiveness of the policy in reducing SO_2 pollution.

[27]These inconsistencies are largely due to ambiguous definitions. For example, manufacturing of fire van can be assigned to Industry 36 (manufacturing of transport equipment) or Industry 37 (manufacturing of dedicated devices).

A.3 Industry-level data

We use industry-level SO_2 emission and coal consumption data to assign industries as being more or less pollution-intensive, which is required for our heterogeneous difference-in-differences research design. In China, SO_2 emissions are highly correlated with coal consumption, and some of the TCZ regulation's more explicit measures for reducing SO_2 emissions specifically targeted the life cycle of coal. Therefore, we consider SO_2 emission and coal consumption as relevant indicators for deciding whether an industry is more or less pollution-intensive.

Our approach entails computing industry-level pollution intensity and then following Greenstone (2002) to define pollution-intensive. We gather data on SO_2 emissions from the China Statistical Yearbook 2002, which contains data for the year 2001. Unlike city-level emission data, which are subject to potential misreporting by local government officials (Karplus et al. 2018), industry-level emission data are less likely to be manipulated as emission levels of different industries are inherently heterogeneous, depending on the industry's characteristics i.e. raw materials used, manufacturing process, residues produced. The reason we use 2001 data is that it contains 40 industries (compared to 20 industries in 1997) and also includes the number of firms in each industry, which allows us to compute average SO_2 emissions per firm in each industry. To relieve the concern that the 2001 data may be affected by the implementation TCZ policy, we find high correlations between data in 1997 and 2001, i.e., 0.98 for SO_2 emission variable and 0.99 for coal consumption variable. This confirms that the 2001 data is a fairly good proxy of industry polluting levels, as suggested by Cai et al. (2016).

We calculate each industry's share of total SO_2 emission and coal consumption. If the share is more than 1 percent for both indicators, we define this industry as being "more pollution-intensive" (see Appendix Table C.1). One special case is that we break up the electricity, steam and hot water supply industry (CIC "44") based on major differences in pollution intensity. The electricity, steam and hot water supply industry accounts for more than 50% of total coal consumption and SO_2 emission, and this is mostly contributed by the thermal power generation (CIC code "4411"). Therefore, include thermal power in the more pollution-intensive group whereas we define other power generation industries contained in CIC "44" (hydro, wind, solar, nuclear, etc.) as less pollution-intensive.

This results in a set of ten industries as being in the more pollution-intensive category, and the industries align closely with those that are defined as being SO_2 regulated in Greenstone (2002). Greenstone (2002) define the following industries as being SO_2 regulated: Pulp and paper(corresponding to CIC code "22"), Inorganic chemicals(CIC code "26"), Petroleum refining(CIC code "25"), Stone, clay, glass, and concrete (CIC code "31"), Iron and steel

(CIC code “32”) and Nonferrous metals (CIC code “33”). These six industries are all covered in our defined pollution-intensive industries, and we include four more that qualify given the definition that we apply: coal mining and dressing (CIC code “06”), agricultural food processing (CIC code “13”), textiles (CIC code “17”), and electricity, steam and hot water supply (CIC code “4411”).

Finally, we compare the industries that we define as being more pollution-intensive with those highlighted in the TCZ policy document and targeted by the regulation. We cover three out of four industries mentioned in the policy document: chemical industry (26), metallurgical industry (32), and nonferrous metal industry (33). The only one that we do not cover is the building material industry, which is not included in our data.

A.4 Final Preparation and the TFP Ratio

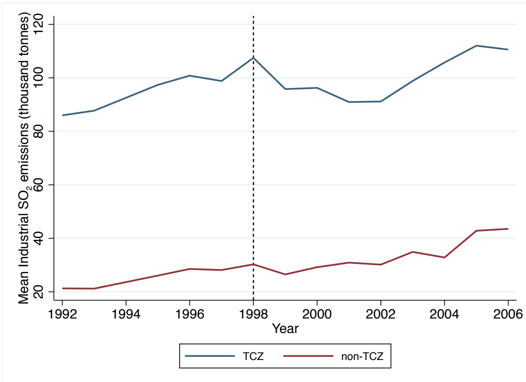
All nominal values (except industrial intermediate input) in CIED dataset and China City Statistical Yearbook are converted to real values in 1998 by using output deflators. The output deflators are constructed following Yang (2015) by using price indexes extracted from the “Urban Price Yearbook 2011” published by the National Bureau of Statistics (NBS). We convert the 2-digit industry level “total output price index (chain)” in the years 1985-2010 to fixed-base index using 1998 as the base year. The input deflators, which are used to deflate nominal industrial intermediate input, are constructed by using National Input-Output (IO) tables in 1997, 2002, and 2007. Precisely, We use the industry-level intermediate input indicated in IO tables as weights to convert output deflators the input deflators. The years before 2000 were using the 1997 IO tables, the years 2001-2005 and post-2006 were using IO tables in 2002 and 2007 respectively. As noted by Yang (2015), this approach takes into account the dynamics of input price in different sectors. We use these deflators to deflate nominal values to real values in 1998.

Once we match all of the aforementioned data and keep observations for which we have the information needed to determine treatment status, we keep only firms for which we observe at least once before and once after the policy was implemented, since we conduct a within-firm analysis. This is the step that leads to the greatest number of observations dropped, leaving us with a little over 800,000 observations. When we go through the matching process, we also check for additional cases for which there appears to be data entry or reporting errors or when we are missing values for the key variables we need to estimate TFP and other key variables we use throughout the analysis. We also drop cases for which employment is less than eight employees, since firms that are below this threshold lack reliable audit systems. The final unbalanced panel that we use as the baseline data set includes 763,240 observations from 1996 to 2006 for 127,757 unique firms.

Descriptive statistics for our baseline data set are provided in Appendix Table 1, which are discussed in Section 3 of the main text along with our approaches to measuring TFP and the other productivity measures. The mean estimated (log) TFP is about 5.47 with a standard deviation of 1.59 (for firms in TCZ and non-TCZ prefectures together). To illustrate its dispersion, we construct the "TFP Ratio" following Hsieh and Klenow (2009) and He et al. (2020)—measured as the ratio between the firm's (log) TFP and the firm's industry's average (log) TFP—and plot its distribution in Appendix Figure B.2. The ratio appears to follow a normal distribution and exhibits about the amount of dispersion expected for a developing country.

B Appendix: Additional Figures - Online Only

Figure B.1: SO_2 Emissions and Concentration Over Time

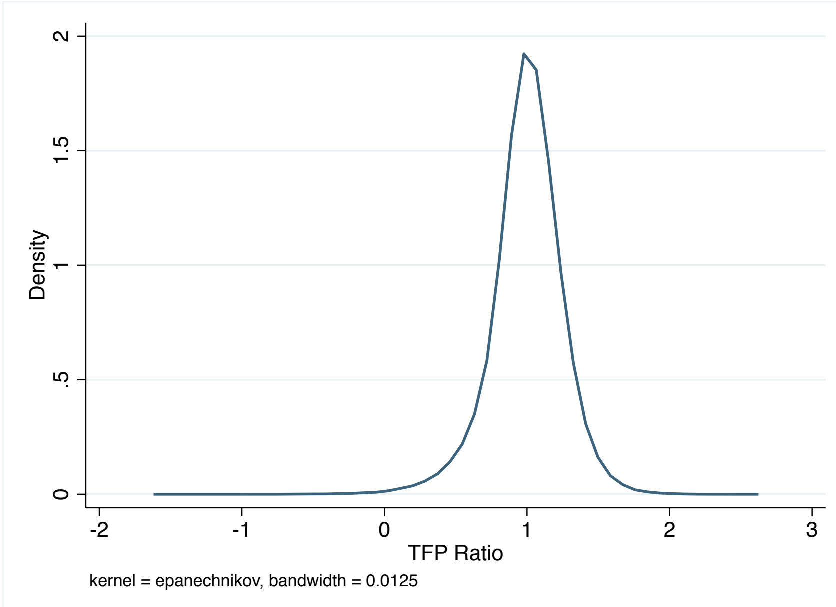


(a) SO_2 Emissions (Reported Data)

(b) SO_2 Concentration (NASA Data)

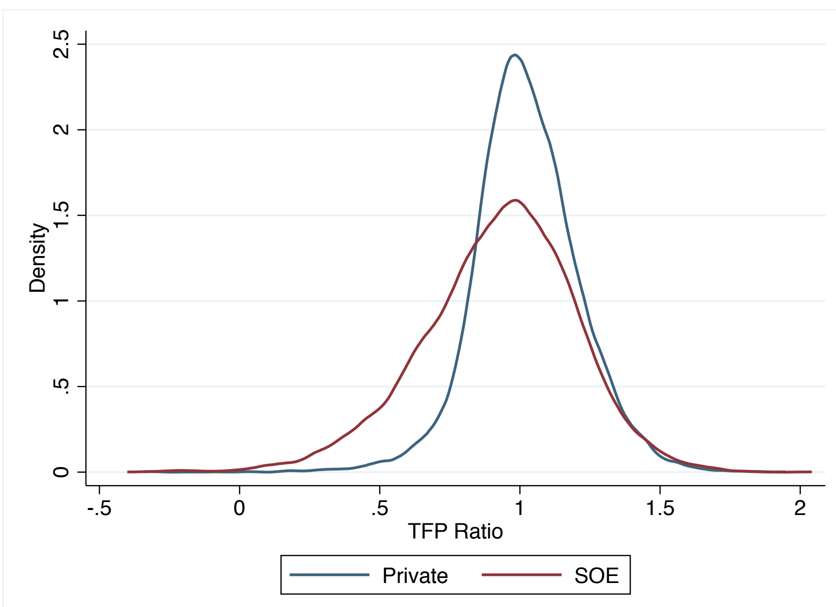
Note: SO_2 emissions reported in the China Environmental Yearbook at the prefecture-year level (Panel A) and SO_2 concentration data from NASA reported at the prefecture-year-month level (Panel B). Both plots use prefecture-year averages and show declines in TCZ prefectures after the regulation was implemented. In Panel A, there is a steeper drop in TCZ emissions relative to the decline in concentration levels in Panel B, which may be indicative of false reporting. In Panel B, concentration levels in both TCZ and non-TCZ prefectures appear to drop, but by more so in TCZ prefectures. Both measures increase for TCZ and non-TCZ prefectures upon accession into the WTO. We formally test the regulation's effectiveness using the NASA data as described in the paper and provide the results in Appendix Table C.2.

Figure B.2: Dispersion of Log(TFP)



Note: This figure plots the "TFP Ratio" for the full sample period as measured by the ratio between the firm's (log) TFP and the industry-year average (log) TFP.

Figure B.3: Pre-Regulation Dispersion of Log(TFP) by Ownership



Note: This figure plots the "TFP Ratio" in 1998 for firms in responsive industries as measured by the ratio between the firm's (log) TFP and the industry-year average (log) TFP.

C Appendix: Additional Tables - Online Only

Table C.1: Industry List, Emissions Data, and Pollution-intensive Industry Assignments

2-Digit CIC code	Industry name	SO_2 per emissions (2001 data) (t SO_2/firm/year)	SO_2 share (2001 data)	Coal consumption (2001 data)	Defined as pollution intensive?
06	Coal Mining and Dressing	79.78	1.37%	6.05%	Yes
07	Petroleum and Natural Gas Extraction	184.08	0.24%	0.70%	
08	Ferrous Metals Mining and Dressing	51.59	0.21%	0.06%	
09	Nonferrous Metals Mining	56.98	0.43%	0.08%	
10	Nonmetal Minerals Mining	122.99	0.49%	0.42%	
11	Other Mining	18.75	0.01%	0.00%	
13	Agricultural Food Processing	53.36	1.28%	1.24%	Yes
14	Food Manufacturing	38.01	0.65%	0.54%	
15	Beverage Manufacturing	57.06	0.94%	0.54%	
16	Tobacco Manufacturing	61.32	0.10%	0.11%	
17	Textile	38.29	1.70%	1.17%	Yes
18	Garments and Fiber Products	15.02	0.07%	0.11%	
19	Leather, Fur, and Feather Products	18.32	0.12%	0.06%	
20	Timber Processing and Related Products	58.60	0.24%	0.18%	
21	Furniture Manufacturing	13.45	0.02%	0.04%	
22	Papermaking and Paper Products	79.76	2.58%	1.49%	Yes
23	Printing and Related Products	4.78	0.02%	0.04%	
24	Cultural, Educational and Sports Products	43.95	0.07%	0.02%	
25	Petroleum Processing and Coking	394.67	2.75%	7.43%	Yes
26	Raw Chemical Materials and Products	112.95	5.43%	6.30%	Yes
27	Medical and Pharmaceutical Products	32.81	0.46%	0.44%	
28	Chemical Fiber Manufacturing	434.76	0.84%	0.70%	
29	Rubber Products	67.44	0.30%	0.23%	
30	Plastic Products	14.18	0.09%	0.12%	
31	Nonmetal Mineral Products	118.50	11.44%	8.01%	Yes
32	Smelting and Pressing of Ferrous Metals	414.21	6.01%	9.47%	Yes
33	Smelting and Pressing of Nonferrous Metals	490.11	4.96%	1.12%	Yes
34	Metal Product Manufacturing	6.29	0.21%	0.20%	
35	General Machinery Manufacturing	13.73	0.26%	0.31%	
36	Special Machinery Manufacturing	28.59	0.29%	0.27%	
37	Traffic Equipment Manufacturing	29.23	0.38%	0.59%	
39	Electric Apparatus Manufacturing	23.65	0.22%	0.15%	
40	Electronic Apparatus Manufacturing	13.88	0.10%	0.05%	
41	Instrument, Meter and Office Equipment	6.16	0.02%	0.02%	
42	Handicrafts and other Manufacturing	13.86	0.06%	0.19%	
44	Electricity, Steam and Hot Water Supply	4521.33	55.36%	50.59%	Yes*
45	Production and Supply of Gas	227.71	0.18%	0.83%	
46	Production and Supply of Tap Water	22.77	0.02%	0.04%	

*Only coal-fired electric power supply and production firms are classified as pollution-intensive (CIC code 4411).

Table C.2: Effect of the TCZ Regulation on SO_2 Concentration Levels

Sample Period:	1988-2008	1996-2006	1988-2008	1996-2006
Outcome Variable:	SO_2	SO_2	SO_2	SO_2
	(1)	(2)	(3)	(4)
TCZ * Post-Policy	-0.037***	-0.040***	-0.041***	-0.046***
	(0.011)	(0.013)	(0.012)	(0.015)
Observations	87,444	45,804	7,287	3,817
Month-year-prefecture data	x	x		
Year-prefecture data			x	x
Month FEs	x	x		
Prefecture FEs	x	x	x	x
Year FEs	x	x	x	x
Prefecture x Year Trends	x	x	x	x

Notes: Table provides the effects of the TCZ regulation on SO_2 concentration levels (ug/m^3) at the prefecture-year-month level (Columns 1-2) and the prefecture-year level (Columns 3-4) using data from NASA. All data that are available are used in Columns 1 and 3, and we limit the sample to the period we study (1996-2006) in Columns 2 and 4. Standard errors are clustered at the prefecture level. Asterisks denote *p <0.10, **p <0.05, ***p <0.01.

Table C.3: Addressing SUTVA Concerns

Outcome Variable:	TFP (log)	TFPv2 (log)	% Firms High-Tech	% Labor High-Tech
	(1)	(2)	(3)	(4)
TCZ * Post-Policy	0.043*	0.045**	-0.005	-0.010
	(0.023)	(0.023)	(0.007)	(0.007)
TCZ * Post-Policy * Polluter	-0.054***	-0.057***		
	(0.019)	(0.019)		
TCZ * Post-Policy * High-Tech	0.001	-0.004		
	(0.023)	(0.023)		
TCZ * Post-Policy * Polluter * High-Tech	0.037	0.040		
	(0.040)	(0.040)		
Observations	762,957	763,237	3,566	3,566
Firm-Year Data	x	x		
Prefecture-Year Data			x	x
Firm FEs	x	x		
Industry x Prefecture FEs	x	x		
Industry x Year FEs	x	x		
Prefecture x Year Trends	x	x	x	x
Prefecture FEs			x	x
Year FEs			x	x

Notes: Table reports from two sets of results addressing SUTVA concerns related to movement of firms and people in high-tech industries (see Section 4.2.1). In Columns 1 and 2, we use the baseline firm-year data with the two versions of (log) TFP as outcome variables. In Columns 3 and 4, we use prefecture-year data, and the outcome variable is the percent of firms that are in high-tech industries in Column 3 and the percent of workers that are in high-tech industries in Column 4. In Columns 1-2, the two-way interactions are included as well as the "polluter" indicator. The other two main effects are absorbed by the fixed effects. Standard errors are clustered at the prefecture level. Asterisks denote $^{*}p<0.10$, $^{**}p<0.05$, $^{***}p<0.01$.

Table C.4: TESTING SENSITIVITY OF RESULTS TO TFP MEASURE CHOICE

Outcome Variable (log):	LP TFP (1)	TFP (2)	TFP (3)	TFP (4)	TFP (5)
TCZpost1998	0.043**	0.077***	0.051*	0.057*	0.052*
	(0.022)	(0.027)	(0.028)	(0.029)	(0.027)
TCZpost1998dirty	-0.049***	-0.041	-0.060**	-0.072***	-0.061***
	(0.017)	(0.032)	(0.024)	(0.023)	(0.023)
Observations	763,237	158,696	407,318	362,027	436,624
Sample used for regressions:					
Full	x				
Homogenous Goods Markets		x			
Q1 of Industry Market Share			x		
Q1 of Industry HHI				x	
Q4 Industry No. of Firms					x

Notes: Table reports results from tests of whether the results are sensitive to our TFP variable choice. In Column 1, We use the approach of Levinsohn and Petrin (2003) to construct TFP. In Columns 2-5, we limit the sample to explore whether the findings are driven by firm-level mark-ups as opposed to actual productivity increases. Only homogenous goods markets are included in Column 2. In Columns 3 and 4, firms in industries in the bottom quartiles of the pre-regulation industry-level distribution of market shares and HHI are included, respectively. In Column 5, firms in industries in the top quartile of the industry-level pre-regulation total number of firms distribution are included. Asterisks denote *p <0.10, **p <0.05, ***p <0.01.

Table C.5: Additional Robustness Checks for Main TFP Results

Outcome Variable (log):	TFP (1)	TFP (2)	TFP (3)	TFP (4)	TFP (5)	TFP (6)	TFP (7)
TCZ * Post-Policy	0.035*	0.053**	0.045**	0.040*	0.044**	0.043***	0.043***
	(0.021)	(0.023)	(0.022)	(0.021)	(0.022)	(0.015)	(0.009)
TCZ * Post-Policy * Polluter	-0.044***	-0.048***	-0.050***	-0.044**	-0.045***	-0.048*	-0.048***
	(0.017)	(0.017)	(0.017)	(0.018)	(0.017)	(0.025)	(0.012)
Observations	728,305	762,957	750,262	721,790	696,514	762,957	762,957
Drop Pilot Data	x						
Control for WTO Entry		x					
Drop if Low Firm Count			x				
Drop if Industry Changed				x			
Drop 2003					x		
Cluster SEs by Industry						x	
Cluster SEs by Firm							x
Firm FEs	x	x	x	x	x	x	x
Industry x Prefecture FEs	x	x	x	x	x	x	x
Industry x Year FEs	x	x	x	x	x	x	x
Prefecture x Year Trends	x	x	x	x	x	x	x

Notes: Table reports results from several robustness checks of the baseline results in Column 3 of Table 2. In Column 1, we drop observations in the years for which the data gathering was in its pilot stage (1996-97). In Column 2, we include a dummy variable equal to one after 2001 to control for when China entered the WTO. In Column 3, we drop prefectures that have fewer than 200 unique firms to ensure the results are not driven by differences in industrialization. In Column 4, we drop firms if their CIC changed over the sample period. In Column 5, we drop 2003 because of our labor variable being from a different source this year. In Columns 6 and 7, we cluster standard errors by industry and firm, respectively. The two-way interactions are included as well as the "polluter" indicator. The other two main effects are absorbed by the fixed effects. Standard errors are clustered at the prefecture level in all cases except Column 6-7. Asterisks denote $*p<0.10$, $**p<0.05$, $***p<0.01$.

Table C.6: Robustness Checks for Alternative TFP Measure Results

Outcome Variable (log):	TFP (1)	TFP (2)	TFP (3)	TFP (4)	TFP (5)	TFP (6)	TFP (7)	TFP (8)
TCZ * Post-Policy	0.043**	0.035*	0.053**	0.045**	0.041*	0.044**	0.043***	0.043***
	(0.022)	(0.021)	(0.023)	(0.022)	(0.021)	(0.022)	(0.016)	(0.009)
TCZ * Post-Policy * Polluter	-0.049***	-0.045***	-0.049***	-0.051***	-0.046**	-0.046***	-0.049*	-0.049***
	(0.017)	(0.017)	(0.017)	(0.017)	(0.018)	(0.017)	(0.025)	(0.012)
Observations	763,237	728,578	763,237	750,262	722,059	696,774	763,237	763,237
Baseline Model	x							
Drop Pilot Data		x						
Control for WTO Entry			x					
Drop if Low Firm Count				x				
Drop if Industry Changed					x			
Drop 2003						x		
Cluster SEs by Industry							x	
Cluster SEs by Firm								x
Firm FEs	x	x	x	x	x	x	x	x
Industry x Prefecture FEs	x	x	x	x	x	x	x	x
Industry x Year FEs	x	x	x	x	x	x	x	x
Prefecture x Year Trends	x	x	x	x	x	x	x	x

Notes: Table reports the regulation's effects on TFP (log) when using the alternative measure of TFP described in Section 3.2. Column 1 is comparable to the baseline main results in Column 3 of Table 2. In Columns 2-8, we conduct the same set of robustness checks as we do for for the full sample. In Column 2, we drop observations in the years for which the data gathering was in its pilot stage (1996-97). In Column 3, we include a dummy variable equal to one after 2001 to control for when China entered the WTO. In Column 4, we drop prefectures that have fewer than 200 unique firms to ensure the results are not driven by differences in industrialization. In Column 5, we drop firms if their CIC changed over the sample period. In Column 6, we drop 2003 because of our labor variable being from a different source this year. In Columns 7 and 8, we cluster standard errors by industry and firm, respectively. The two-way interactions are included as well as the "polluter" indicator. The other two main effects are absorbed by the fixed effects. Standard errors are clustered at the prefecture level in all cases except Columns 7-8. Asterisks denote *p <0.10, **p <0.05, ***p <0.01.

Table C.7: Effects on TFP for Firms in "Responsive" Industries (Separately)

Outcome Variable (log):	TFP (1)	TFP (2)	TFP (3)	TFP (4)
Panel A: Most Pollution-Intensive Industries				
	Textiles	Chemicals	Steel	
TCZ * Post-Policy	0.084*	0.085**	0.113*	
	(0.046)	(0.037)	(0.065)	
Observations	56,704	53,116	14,208	
Panel B: Less Pollution-Intensive Industries				
	Petro & Gas Processing	Ferrous Metals Mining & Dress	Beverage Manufacturing	Tobacco Manufacturing
TCZ * Post-Policy	0.435*	0.306*	0.083*	0.231*
	(0.216)	(0.166)	(0.045)	(0.119)
Observations	349	2,644	15,360	2,070
Firm FEs	x	x	x	x
Year FEs	x	x	x	x
Prefecture FEs	x	x	x	x
Prefecture x Year Trends	x	x	x	x

Notes: Table reports the regulation's effects on TFP (log) for "responsive" industries (i.e., industries for which the regulation enhanced TFP). Panel A includes the most pollution-intensive industries with positive effects (textiles, chemicals, and steel). Panel B includes the less pollution-intensive industries with positive effects (petroleum and gasoline processing, ferrous metals mining and dressing, beverage manufacturing, and tobacco manufacturing). Standard errors are clustered at the prefecture level. Asterisks denote *p <0.10, **p <0.05, ***p <0.01.

Table C.8: EFFECT ON EXIT FOR FIRMS IN "RESPONSIVE" INDUSTRIES (SEPARATELY)

Outcome Variable:	Exit (1)	Exit (2)	Exit (3)	Exit (4)
Panel A: Most Pollution-Intensive Industries				
	Textiles	Chemicals	Steel	
TCZ * Post-Policy	0.065***	0.011	0.072***	
	(0.016)	(0.015)	(0.022)	
Observations	56,709	53,142	14,229	
Panel B: Less Pollution-Intensive Industries				
	Petro & Gas Processing	Ferrous Metals Mining & Dress	Beverage Manufacturing	Tobacco Manufacturing
TCZ * Post-Policy	0.111	0.081	0.009	0.002
	(0.159)	(0.059)	(0.018)	(0.045)
Observations	349	2,644	15,360	2,079
Firm FEs	x	x	x	x
Year FEs	x	x	x	x
Prefecture FEs	x	x	x	x
Prefecture x Year Trends	x	x	x	x

Notes: Table reports the effect of the TCZ regulation on exit for firms in "responsive" industries (i.e., industries for which the regulation enhanced TFP). Panel A provides the results for the more pollution-intensive industries, which includes textiles, chemicals, and steel. Panel B provides the results for the less pollution-intensive industries, which includes petroleum and gasoline processing, ferrous metals mining and dressing, beverage manufacturing, and tobacco manufacturing. Standard errors are clustered at the prefecture level. Asterisks denote *p <0.10, **p <0.05, ***p <0.01.