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

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Abstract

Pollution impedes economic growth and development, but environmental regulation also can be costly. In this paper, we study the effects of an environmental regulation on firm productivity in China across the entire industrial sector. We find that productivity for firms in less pollution-intensive industries increases while there is no effect on those in the "dirtiest" industries. The net effect is therefore positive, highlighting the importance of considering firms across all industries when studying how regulation impacts economic activity. We explore the underlying mechanisms and find that there is no firm exit, on average, or input reallocation. Rather, firms use inputs more efficiently, although the the drivers vary based on firm ownership. Our findings suggest that environmental regulation can be a tool for fostering growth.

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1 Introduction

Air pollution is one of today's biggest barriers to economic growth. It creates substantial costs by dampening worker productivity (Graff Zivin and Neidell 2012; Chang, Graff Zivin, Gross and Neidell 2016 2019) and labor supply (Hanna and Oliva 2015), reducing life expectancy (Ebenstein, Fan, Greenstone, He and Zhou 2013), and increasing infant mortality (Chay and Greenstone 2003; Arceo, Hanna and Oliva 2015). At the same time, environmental regulation can be costly, as it has been shown to shift production and labor away from regulated industries (Kahn 1997; Greenstone 2002) and earnings losses can be high (Walker 2013). This tension has generated a long-standing "environment versus economy" discourse.

Yet regulation also has the potential to enhance growth, as it 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). However, despite the growing evidence that environmental policy and regulation spur R&D expenditures and patenting (Jaffe and Palmer 1997; Newell, Jaffe and Stavins 1999; Popp 2002; Aghion, Dechezlêpretre, Hemous, Martin and Van Reenen 2016; Calel and Dechezlêpretre 2016; Calel 2020), relatively little is known about the effects on productivity—a key indicator of economic growth and competitiveness. At the firm level, Greenstone (2002) and Greenstone, List and Syverson (2012) offer the most robust analyses to date, finding that the TFP of manufacturing plants declined in response to the U.S. Clean Air Act.[1] Developing a better understanding of how environmental regulation impacts productivity is thus of first-order importance and increasingly pressing as the consequences of climate change continue to intensify.

In this paper, we study the impact of an environmental regulation in China on firm productivity across the entire industrial sector. This entails examining firms not only in the "dirtiest" industries but also those in less pollution-intensive industries, which are often not considered in studies of environmental regulation and productivity. Doing so is important, though, since less pollution-intensive industries still pollute and are still regulated (unless specific industries are explicitly targeted), and they can be affected through other channels

[1] 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. Berman and Bui (2001) find that firm productivity increased in response to a regulation for refineries in Los Angeles. More recently, He, Wang and Zhang (2020) found that a water regulation in China reduced firm productivity.

such as the substitution of industrial activities and increased competitiveness.[2]

Specifically, we study the 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 the two sources of variation that determine treatment status—whether a prefecture is regulated and before/after variation based on the regulation's implementation timing—and allow the effects to vary based on whether the firm is in a heavy-polluting industry.

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 overall net 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 some of the first results providing evidence that environmental regulation enhances firm productivity, and given how we would have drawn different conclusions more in line with those in the literature if we did not estimate the effects on "cleaner" industries as well, they highlight the importance of considering all regulated firms to understand the implications of environmental regulation for industrial activity.

We conduct a series of additional tests to explore the underlying mechanisms. We first examine whether the regulation appears to have been enforced, expecting pollution to decrease if firms indeed complied, as changes in productivity otherwise may not be attributed to the regulation. We find that the regulation reduced SO_2 concentration levels by 4.6%, an amount that is comparable to similar regulations in other settings. We then examine whether firm sorting contributes to productivity gains and find that there is no change in the propensity to exit on average. There is also no input reallocation, as there are no effects on labor, capital, and intermediate input levels. Rather, added-value and sales increase as well as the productivity of each input (measured as added-value per input), suggesting that firms find ways to turn inputs into output more efficiently.

To explore this further, we also study private firms and state-owned enterprises (SOEs) separately, since the ways in which firms respond to the regulation may differ based on

[2] Hafstead and Williams (2018) also point this out and study how environmental policy affects employment in a general equilibrium model. They show that pollution taxes reduce employment in the regulated sector but it increases in other industries such that the net (negative) effect is small.

firm ownership. For example, it is common for firms to try to evade the compliance costs of environmental regulations, and this is especially the case for SOEs in China given the government's long history of favoritism. Local government officials—who are responsible for enforcing the regulation—typically appoint their executives 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 invest in pollution-abatement technology or activities.

For private firms, the regulation induced exit and the exit rate is higher for those that were less productive in the pre-regulation period, suggesting that firm sorting likely contributes to the productivity gains. That said, more productive private firms also exit, so it may not be the only factor at play. We then examine inputs, outputs, and single-factor productivity again and find that there is no input reallocation or output effect for these firms—input levels and sales do not change—but rather they become more efficient in their use of labor and intermediate inputs. The regulation also increased their average wages and management expenditures, which is consistent with investing in the higher-skilled labor required to pursue innovative activities in response to the regulation.

On the other hand, the channel for SOEs appears to be different. Productivity of labor, capital, and intermediate inputs all increase, and there is again no change in inputs. There is no exit of SOEs, though, whereas sales increase substantially. They only increase for older SOEs, pointing towards the possibility of favoritism, as older firms are more likely to have developed bargaining power given their longer relationships with government officials.

Lastly, we explore the implications of our results for aggregate productivity by estimating the effects on TFP for firms that were more and less productive before the regulation was implemented. The objective is to indirectly capture the effects on TFP dispersion, as dispersion often signals inefficiencies in production allocation that harm growth (Hsieh and Klenow 2009). The difference in productivity between the least- and most-productive firms increases for SOEs but not private firms.

This paper helps narrow the knowledge gap on how environmental regulation impacts firm productivity, which is important for understanding whether environmental regulation and growth are 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. On the other hand, our results suggest that intervention aiming to reduce pollution actually may be an effective tool for simultaneously fostering economic development and environmental quality improvements, a finding that relies on examining the net effect across all industries in our context. This is important and timely for policy in various settings and runs counter to the narrative that often dominates political debates. In the developed country context, productivity has been declining since the 2000s while the consequences of climate change are worsening, and developing countries disproportionately bear the burden of pollution while facing widespread poverty.

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.[3] Most relatedly, He et al. (2020) study a water regulation and find that it reduced productivity, and Fan, Graff Zivin, Kou, Liu and Wang (2019) study the effects of a regulation's stringency on firm performance, also finding that it declined despite the adoption of new practices.[4] Our paper complements this work.

This paper proceeds as follows. In Section 2, we plan to provide a conceptual framework and Section 3 provides institutional details for our empirical setting and describes research design. Section 4 discusses our data. We provide the main results in Section 5, and in Section 6, we explore the underlying mechanisms. We briefly discuss implications for aggregate growth in Section 7 and conclude in Section 8.

2 Conceptual Framework

TBD.

[3] 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).

[4] 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.

3 Background and Research Design

3.1 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.[5] 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 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.

3.2 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 met national environmental quality standards. Another goal was to significantly reduce precipitation pH levels relative to 2000 levels. The national

[5] 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 $\mu g/m^3$.

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.[6]

[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 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

[6] We detail our assignment of "regulated" in Section 4.

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.

3.3 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.[7] Although 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

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

zero otherwise, and $Post_t$ is an indicator equal to one in the post-policy years (from 1999 onwards) and zero otherwise.[8] 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 on the dirtiest firms. In Section 5, 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 5.2).

[8] 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.

4 Data

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).[9] 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

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

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.[10] 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.[11] 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 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.[12] 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)).

[10] 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.

[11] 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.

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

Lastly, for our examination of whether the regulation was enforced, we collect prefecture-level SO_2 data from two sources. Yearly SO_2 emissions data are 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 follow Chen, Oliva and Zhang (2017) to derive satellite-based SO_2 concentration levels using data from National Aeronautics and Space Administration (NASA).[13] 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.

4.1 Productivity Measures

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 industry-specific capital and labor coefficients including year fixed effects and whether the firm is located in a TCZ prefecture as a state variable 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.[14] In our robustness checks, we also measure

[13] 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).

[14] 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.

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. We examine single-factor productivity outcomes—labor, capital, and intermediate input productivity as measured by added-value divided by each input—when exploring the underlying mechanisms of our results.

4.2 Baseline Firm and Prefecture-Level Characteristics

Table 1 presents firm- and prefecture-level descriptive statistics in pre-policy years (1996-1998). 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 described in Section 3.3.

[TABLE 1 HERE]

5 Main Results

5.1 Effect of the TCZ Regulation on Firm Productivity

We begin by visualizing the effect of the TCZ regulation on firm TFP. Using (log) TFP as the dependent variable, we estimate 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.[15]

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).[16]

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

[15] 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.

[16] 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.

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 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.[17] 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.

[17] 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.

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 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 4 to Column 3 illustrates why using a heterogeneous difference-in-difference approach rather than a triple-difference approach is important. If we took the triple-difference approach, our conclusions would be reversed—a negative and fairly large effect—yet we know from Column 3 that the regulation's effect on these firms' productivity is actually zero. The results in Column 4 would also entirely miss the positive effects on less pollution-intensive firms.

5.2 Robustness Checks

A key threat to our identification strategy is that prefectures may evolve differently over time systematically for TCZ and non-TCZ regions, so it is important to control for this. We demonstrated that our baseline estimates for firms in the dirtiest industries are not sensitive to whether we control for time-varying prefecture characteristics with prefecture-specific trends as opposed to fixed effects in the previous subsection. Their interpretations are different, since the control group includes regulated firms in "cleaner" industries in the latter case, but the point estimates are statistically the same, suggesting that trends are sufficient. This supports the validity of using a heterogeneous difference-in-differences research design.

In this subsection, we summarize additional robustness checks that we carry out to ensure that the baseline results are not sensitive to our modeling choices and TFP variable construction. First, in Appendix Table C.3, we drop the years in which the database was in pilot mode (1996-1997) (Column 1). 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.[18]

[18]Once we limit our sample to firms only in industries for which there is an increase in TFP on average,

In Column 2, we control for China's accession into the the WTO in case TCZ and non-TCZ prefectures were affected differently by including a dummy variable for years after 2001 and also interacting this dummy with the TCZ dummy. The results become stronger, if anything.

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 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 estimate our baseline model and conduct the same set of robustness checks using an alternative measure of TFP as the dependent variable. Instead of following Ackerberg et al. (2015), we follow Levinsohn and Petrin (2003) and provide the findings in Appendix Table C.4. 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. Although using Ackerberg et al. (2015) as our baseline is preferred because it corrects the simultaneity concerns of both Levinsohn and Petrin (2003) and Olley and Pakes (1996), finding the same results provides us with confidence that our estimates are not biased by our productivity measure choice.

5.3 Addressing SUTVA Concerns

The second identification assumption of our 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). One 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

these results are larger and stronger, as we show in subsequent sections.

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.[19] If workers or firms in high-tech industries move to TCZ regions, we would expect these measures to increase.

Appendix Table C.5 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

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

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 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.

5.4 "Responsive" Industries

Throughout the remainder of the paper, we limit our sample to firms that are in "responsive" industries (i.e., industries for which TFP increased) so we can focus on what may be driving productivity gains. To identify which industries were "responsive," we estimate the effects separately for every industry at the 2-digit CIC level.

It turns out that, even though there were no effects on TFP for heavier-polluting firms on average, productivity increases for three key "dirty" industries: smelting and pressing of ferrous metals (i.e., the steel industry), chemical materials and products, and textiles (see Appendix Table C.6). It is intuitive that the steel and chemicals industries responded, as they are extremely coal-intensive—they each account for 5-6% of total SO_2 emissions in China. The textiles industry is also a heavy polluter (1-2% of emissions), so they also had an incentive to adjust. There are no effects on any other pollution-intensive industries.[20]

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,

[20] 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.

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.[21]

Before moving on to explore what drives productivity improvements for "responsive" industries, we re-estimate our baseline model and main set of robustness checks to find the average effect on responsive industries. 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.

[TABLE 3 HERE]

6 Why Does Productivity Increase?

We now turn to investigating the ways in which firms responded to the regulation by first examining whether the regulation was enforced and then whether productivity gains are driven by input changes as opposed to more efficient use of inputs. Since strategies also may differ based on firm ownership, we also examine private firms and SOEs separately. For the firm-level regressions, we include only the "responsive" industries.

6.1 Regulation Enforcement

One potential explanation of our findings is that the regulation was simply not enforced. If so, firms did not face costs associated with compliance, and the productivity changes may not actually be driven by the regulation but rather some other unobserved change that occurred at the same time. We explore this by examining whether the regulation reduced

[21] 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.

pollution, expecting larger declines in TCZ prefectures relative to non-TCZ prefectures if the regulation was enforced.

Starting with a descriptive analysis, we plot the raw prefecture-level pollution data over time for TCZ and non-TCZ regions in Appendix Figure B.2. We use two sources: self-reported industrial SO_2 emissions data from the China Environmental Yearbook in Panel A and SO_2 concentration data from NASA in Panel B. While emissions most directly capture the pollution measure of interest, governments have an incentive to misreport to show 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).

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.[22] 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.[23]

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:

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

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

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

[23] 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.

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 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).

6.2 Firm Sorting

One leading 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 reallocation and turnover 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.

We estimate the effect of the regulation on exit for firms in "responsive" industries and provide the results in Table 4. On average, there are no statistically significant effects and the magnitudes of the coefficients are small.[24] When we examine industries separately, we can see that the propensity to exit increases in two of the more pollution-intensive industries—steel and textile industries (Appendix Table C.7)—but there is no effect on average or for any of the cleaner industries.

[TABLE 4 HERE]

6.3 Inputs, Outputs, and Efficiency Improvements

Our results from the previous sub-section suggest that firm sorting is not a central driver of productivity improvements. Besides reallocation across firms, productivity could also increase if firms increase or reallocate inputs. For example, technology adoption is often an important channel 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, they 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.

To explore these channels, we estimate the regulation's effects on inputs, outputs, and single-factor productivity, which we define as value-added divided by labor, capital, or intermediate inputs. The results are presented in Table 5, and taken together, they suggest that firms start using all three inputs more efficiently and output increases. In Panel A, we find that value added increases by about 17% and sales increase by 10% despite there being no change or reallocation of inputs (Panel B). There is no statistically significant effect on labor, capital, or intermediate inputs. Rather, all three single-factor productivity measures increase, with estimates ranging from 10% to 16%. One plausible interpretation of these findings is that firms engage in innovative activities to comply with the regulation—such as improving practices and processes—that translate into more efficient use of inputs.

[24] We also find no exit when using our entire sample rather than limiting it to responsive industries only. These results can be provided upon request.

6.4 Heterogeneity by Firm Ownership

We now probe the underlying drivers of our findings further by examining 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 regulatory costs and have no incentive to change behavior—the results of the previous subsection might mask important heterogeneity in firms' strategies.

We start by estimating our baseline specification separately for state-owned enterprises (SOEs) and private firms using TFP, added-value, and sales as the outcome variables and provide the results in Table 6. We include all years in the sample in Columns 1-3 and drop pilot years' data in Columns 4-6 for robustness purposes, since the pilot years' data primarily consist of SOEs. We examine inputs and single-factor productivity by firm ownership for all years in Table 7 and while dropping pilot year data in Appendix Table C.8.

Focusing first on SOEs, Panel A of Table 6 shows that TFP, added-value, and sales all increase substantially. Single-factor productivity increases for all three inputs while there is no change in any input levels (Panel A of Table C.8). On the other hand, there is no change in sales for private firms while their added-value and TFP increase (Panel B, Columns 3 and 6, of Table 6). These results begin to suggest that the Chinese government might be helping SOEs (but not private firms) in the face of compliance costs through some type of sales guarantee to generate revenue while private firms achieve productivity gains through another channel.

[TABLE 6 HERE]

[TABLE 7 HERE]

If this is indeed the case, we would expect the oldest SOEs to benefit most, as they have had more time to nurture deeper relationships and develop more bargaining power. We would also expect to see private firms exit with no exit of SOEs, as the sales guarantees help SOEs survive. As such, we estimate the effects on exit and sales based on firm age. The results are consistent with the favoritism hypothesis (see Table 8). We provide the estimates for firms of any age (Columns 1 and 4), for those that are younger than the median pre-policy age of 11 years old (Columns 2 and 5), and for those that are older than 11 (Columns 3 and 6).[25] In Panel A, we can see that no SOEs exit at all, and the increases in SOE sales are driven by older firms. Sales increase by 12% for SOEs that are older than the median age.

[TABLE 8 HERE]

The propensity for private firms to exit does in fact increase by 4% on average, and the effect is larger for older firms (Panel B, Columns 1-3). This is expected given how younger firms are more likely to have installed more recent (and thus more efficient) technology. With there being some private firm exit and no change in their sales, sorting could be one of the underlying drivers of private firms' productivity gains. To make this attribution, though, we should find that it is the least productive firms that exit, thus increasing average TFP. We test this by estimating the effects separately for firms that were more or less productive in the last year of the pre-policy period, splitting the sample by the median TFP as well as single-productivity measures for robustness purposes.

Private firms that were previously less productive exit at a higher rate, but there is also a non-trivial amount of exit for those that were more productive (see Appendix Table C.9).[26] Exit increases by 7% for private firms below the pre-policy median TFP level, for example, whereas it increases by 4% for those over it. Therefore, although private firm exit may contribute towards increased market shares and thus TFP gains for SOEs, it's unlikely to be the only factor contributing to TFP enhancement.

One remaining explanation for private firms is that innovation is at play. We saw in Panel B of Table 7 that labor and intermediate input productivity both increase for private firms yet there are no changes in inputs. This is consistent with the process and practice innovations that industrial firms can pursue to reduce emissions, such as fuel-switching or managing operations more effectively.

[25] We use the median firm age in the last year of the pre-policy period (1998).

[26] One exception is when splitting the sample by capital productivity in Columns 5 and 6. The effects are then insignificant and the magnitudes are about equal.

To explore this further, we examine whether firms invest more in high-skilled labor (e.g., scientists and engineers) and management in response to the regulation. If they engage in innovative activities, we would expect both average wages and management expenditures to increase, as innovation requires more time from both. We also disentangle whether firms appear to innovate in their practices and processes or new technologies by examining new product output as opposed to just gross output.

The results are consistent with innovative activity of private firms. In Table 9, we provide the regulation's effects on average wages (Column 1), management expenditures per worker (Column 2), new product output (as opposed to gross output) (Column 3), and the share of gross output that is new product output (Column 4) (all in logs). There is no effect on any outcome for SOEs (Panel A). On the other hand, average wages increase by 13% for private firms in less pollution-intensive industries and by 7% for more pollution-intensive industries (Column 1 of Panel B). Private firms in less pollution-intensive industries also invest specifically in management, as management expenditures per worker increase by 8% (Column 2). These findings point towards investments in higher-skilled labor, which is a key input into innovation, and managers' time and/or quality could be a contributing factor for those in less pollution-intensive industries.[27] Lastly, we find that there is no change in *new product* output (Columns 3 and 4 of Panel B), suggesting that, if firms are innovating, they are not doing so through new technology development but rather other avenues such as implementing new processes.

[TABLE 9 HERE]

7 Implications for Aggregate Growth

Although our data do not allow us to study the costs of the regulation—such as reduced entry—we can provide some insight into the implications of our findings for aggregate growth by studying the distribution of TFP. In their seminal work, Hsieh and Klenow (2009) show that high productivity dispersion reflects a misallocation of resources, which lowers aggregate

[27] This is consistent with the literature on how higher management quality improves firm outcomes. See Scur, Sadun, Van Reenen, Lemos and Bloom (2021) for a recent review.

productivity and welfare. A reduction in dispersion after the regulation's implementation may therefore suggest that improvements in aggregate productivity as well.

To get a sense of dispersion before the regulation was implemented, we plot the distribution of the TFP ratio for SOEs and private firms (see Appendix Figure B.3).[28] Dispersion is much higher for SOEs—the distribution is flatter and has longer tails—suggesting that there is a greater degree of misallocation.

We conduct two tests to see if the regulation affected dispersion. First, we estimate the effects on (log) TFP separately for firms that were below and above the pre-policy productivity median to see whether previously productive firms became even more productive relative to those that were previously less productive.[29] The results are in Columns 1 and 2 of Table 10. For SOEs that were previously more productive, TFP increases substantially, whereas it does not change for those under the median. This signals that dispersion increased, as the gap in productivity widens. On the other hand, for private firms, TFP increases more for those that were *below* the median in less pollution-intensive industries (23%) compared to those over it (14%), thus narrowing the gap.

We then estimate the effects on the TFP Ratio and the results confirm that dispersion increases only for SOEs. We split the sample based on a ratio threshold of one in the pre-policy period and present the results in Columns 3 and 4 of Table 10). A ratio exceeding one indicates that the firm's productivity is above its industry-year's mean (log) TFP and vice versa. Dispersion increases substantially for SOEs in less pollution-intensive industries while there is no effect at all on the TFP ratio for private firms. Taken together, these results suggest that the regulation may have increased dispersion for SOEs but not private firms, potentially exacerbating existing inefficiencies.

Another way to explore the potential effects on China's broader macroeconomic performance is to examine how the regulation impacted young firms, as high-growth start-ups play an important role in driving aggregate productivity. Research from industrial organization and growth theory literature indicate that shifts towards older incumbents weaken aggregate growth, as entrants help maintain market competition.[30] We now split the sample based on

[28]The TFP ratio calculation is described in Appendix A.

[29]As before, we use the median in the final year of the pre-policy period (1998).

[30]For example, Alon, Berger, Dent and Pugsley (2018) show that the relationship between firm age and

whether the firm was younger than five in the last year of the pre-policy period (1998), as this is the age that's typically associated with being a start-up, and estimate the effects on TFP and sales again.

The results are presented in Columns 5-8 of Table 10. Private start-ups increase TFP by 19% whereas there is no effect on more established private firms (Columns 5-6, Panel B), consistent with start-ups being more likely to innovate.[31] On the other hand, the TFP of *older* SOEs increases by 21% whereas the TFP of start-up SOEs does not change (Columns 5-6 in Panel A). This effect is again driven by sales for SOEs (Column 8, Panel A) while no private firms benefit from increased sales. This shift in market share towards older SOEs highlights one way in which favoritism may undermine the potential aggregate benefits of environmental regulation, as it detracts from private start-ups that innovate in response to the regulation.

[TABLE 10 HERE]

8 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.

productivity growth is downward sloping, and when studying the U.S., they find that a "start-up deficit" resulted in a 3.1% cumulative drag on aggregate productivity since 1980.

[31] The coefficient for older private firms (0.076) is also substantially lower than it is for younger firms.

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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: Main Results for 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" Industries

Outcome Variable (log):	TFP (1)	TFP (2)	TFP (3)	TFP (4)	TFP (5)	TFP (6)	TFP (7)	TFP (8)
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
Baseline	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 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.3. 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: Effect of the TCZ Regulation on Firm Exit

Outcome Variable:	Exit (1)	Exit (2)	Exit (3)	Exit (4)	Exit (5)	Exit (6)	Exit (7)	Exit (8)
TCZ * Post-Policy	0.010	0.018	0.014	-0.016	0.013	0.011	0.007	0.010
	(0.016)	(0.018)	(0.017)	(0.015)	(0.017)	(0.017)	(0.016)	(0.012)
TCZ * Post-Policy * Polluter	0.012	0.012	0.007	0.014	0.010	0.014	0.009	0.012
	(0.015)	(0.017)	(0.016)	(0.015)	(0.015)	(0.016)	(0.015)	(0.016)
Observations	144,512	119,813	135,893	144,512	142,595	138,370	132,321	144,512
Baseline	x							
Continuous Firms Only		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
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 effects of the TCZ regulation on exit for firms in "responsive" industries (i.e., industries for which the regulation enhanced TFP). Column 1 is the baseline model. In Column 2, we drop firms that are not continuously observed from their first and last year in the data. In Column 3, we drop the pilot data (years 1996-97). In Column 4, we include a dummy variable equal to one after 2001 to control for when China entered the WTO. In Column 5, we drop prefectures that have fewer than 200 unique firms to ensure the results are not driven by differences in industrialization. In Column 6, we drop firms if their CIC changed over the sample period. In Column 7, we drop 2003 because of our labor variable being different. In Column 8, we cluster standard errors by industry. All two-way interactions and main effects are absorbed. Standard errors are clustered at the prefecture level in all cases except Column 8. Asterisks denote $^{*}p<0.10$, $^{**}p<0.05$, $^{***}p<0.01$.

Table 5: Effects on Inputs, Outputs, and Single-Factor Productivity

	(1)	(2)	(3)
Panel A: Outputs			
Outcome variable:	Profit	Added-Value (log)	Sales (log)
TCZ * Post-Policy	-436.586	0.170***	0.102***
	(844.460)	(0.044)	(0.039)
TCZ * Post-Policy * Polluter	106.076	-0.084*	-0.055
	(962.560)	(0.044)	(0.039)
Observations	144,512	144,512	144,393
Panel B: Inputs			
Outcome variable:	Labor (log)	Capital (log)	Intermed. Inputs (log)
TCZ * Post-Policy	0.011	0.030	0.065
	(0.023)	(0.036)	(0.041)
TCZ * Post-Policy * Polluter	-0.007	-0.016	-0.023
	(0.025)	(0.034)	(0.040)
Observations	144,512	144,512	144,512
Panel C: Single-Factor Productivity			
Outcome variable:	Labor Productivity (log)	Capital Productivity (log)	Intermed. Inputs Productivity (log)
TCZ * Post-Policy	0.159***	0.140***	0.106***
	(0.041)	(0.045)	(0.033)
TCZ * Post-Policy * Polluter	-0.077*	-0.068	-0.061*
	(0.040)	(0.044)	(0.032)
Observations	144,512	144,512	144,512
Firm FEs	x	x	x
Industry x Prefecture FEs	x	x	x
Industry x Year FEs	x	x	x
Prefecture x Year Trends	x	x	x

Notes: Table reports the effects of the TCZ regulation on outputs (Panel A), inputs (Panel B), and single-factor productivity (Panel C). The sample includes only firms in "responsive" industries (i.e., industries for which the regulation enhanced TFP). In terms of outputs, the outcome variables are profit (millions), added-value, and sales. The outcome variables for inputs are labor, capital, and intermediate inputs. In Panel C, single-factor productivity is measured as value-added over each input. All outcome variables are in logs except profit. Standard errors are clustered at the prefecture level. Asterisks denote *p <0.10, **p <0.05, ***p <0.01.

Table 6: Effects on Productivity, Added Value, and Sales by Firm Ownership

Outcome (log):	TFP (1)	Added Value (2)	Sales (3)	TFP (4)	Added Value (5)	Sales (6)
Panel A: State-Owned Firms						
TCZ * Post-Policy	0.195***	0.206***	0.131***	0.140**	0.152***	0.119**
	(0.056)	(0.057)	(0.050)	(0.054)	(0.056)	(0.048)
TCZ * Post-Policy * Polluter	-0.088	-0.103	-0.060	-0.056	-0.071	-0.064
	(0.061)	(0.063)	(0.057)	(0.061)	(0.063)	(0.054)
Observations	34,253	34,288	34,221	29,698	29,731	29,667
Panel B: Private Firms						
TCZ * Post-Policy	0.126**	0.141**	0.066	0.091*	0.087	0.026
	(0.053)	(0.057)	(0.050)	(0.050)	(0.053)	(0.048)
TCZ * Post-Policy * Polluter	-0.072	-0.084	-0.052	-0.029	-0.021	-0.009
	(0.053)	(0.058)	(0.051)	(0.052)	(0.054)	(0.048)
Observations	110,130	110,156	110,104	106,069	106,094	106,041
All Years	x	x	x			
Drop Pilot Years				x	x	x
Firm FEs	x	x	x	x	x	x
Industry x Prefecture FEs	x	x	x	x	x	x
Industry x Year FEs	x	x	x	x	x	x
Prefecture x Year Trends	x	x	x	x	x	x

Notes: Table reports effects of the TCZ regulation on TFP (Columns 1 and 4), added-value (Columns 2 and 5), and sales (Columns 3 and 6) for SOEs (Panel A) and private firms (Panel B). All outcomes variables are in logs. The sample includes only firms in "responsive" industries (i.e., industries for which the regulation enhanced TFP). We include all years of data in Columns 1-3 and we drop the years in which data collection was in its pilot stage (1996-97) in Columns 4-6. Asterisks denote *p <0.10, **p <0.05, ***p <0.01.

Table 7: Effects on Inputs and Single-Factor Productivity by Firm Ownership

Outcome (log):	Labor	Capital	Intermed. Input	Labor Productivity	Capital Productivity	Intermed. Input Productivity
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: State-Owned Firms						
TCZ * Post-Policy	0.011	0.052	0.062	0.195***	0.154**	0.144***
	(0.027)	(0.039)	(0.052)	(0.056)	(0.061)	(0.050)
TCZ * Post-Policy * Polluter	0.005	-0.049	-0.005	-0.108*	-0.054	-0.098*
	(0.033)	(0.042)	(0.064)	(0.060)	(0.067)	(0.056)
Observations	34,288	34,288	34,288	34,288	34,288	34,288
Panel B: Private Firms						
TCZ * Post-Policy	0.022	0.045	0.055	0.119**	0.096	0.086*
	(0.033)	(0.054)	(0.052)	(0.051)	(0.060)	(0.044)
TCZ * Post-Policy * Polluter	-0.034	-0.016	-0.045	-0.049	-0.068	-0.039
	(0.035)	(0.052)	(0.051)	(0.049)	(0.060)	(0.043)
Observations	110,156	110,156	110,156	110,156	110,156	110,156
Firm FEs	x	x	x	x	x	x
Industry x Prefecture FEs	x	x	x	x	x	x
Industry x Year FEs	x	x	x	x	x	x
Prefecture x Year Trends	x	x	x	x	x	x

Notes: Table reports effects of the TCZ regulation on labor (Column 1), capital (Column 2), intermediate inputs (Column 3), labor productivity (Column 4), capital productivity (Column 5), and intermediate input productivity (Column 6). Single-factor productivity is calculated as value added over each input. All outcomes variables are in logs. The sample includes only firms in "responsive" industries (i.e., industries for which the regulation enhanced TFP).
Asterisks denote $^{*}p<0.10$, $^{**}p<0.05$, $^{***}p<0.01$.

Table 8: Effects on Exit and Sales by Firm Ownership

	Exit All (1)	Exit <11 Years (2)	Exit >11 Years (3)	Sales (log) All (4)	Sales (log) <11 Years (5)	Sales (log) >11 Years (6)
Panel A: State-Owned Firms						
TCZ * Post-Policy	0.008	0.039	0.020	0.131***	0.154	0.123**
	(0.024)	(0.054)	(0.024)	(0.050)	(0.130)	(0.055)
TCZ * Post-Policy * Polluter	0.024	0.073	0.012	-0.060	-0.178	-0.038
	(0.024)	(0.059)	(0.025)	(0.057)	(0.136)	(0.064)
Observations	34,288	6,406	27,882	34,221	6,381	27,840
Panel B: Private Firms						
TCZ * Post-Policy	0.042**	0.031	0.051*	0.066	0.075	-0.006
	(0.020)	(0.028)	(0.026)	(0.050)	(0.073)	(0.060)
TCZ * Post-Policy * Polluter	-0.009	-0.005	-0.006	-0.053	-0.056	0.003
	(0.018)	(0.027)	(0.023)	(0.051)	(0.073)	(0.067)
Observations	110,156	57,489	52,660	110,104	57,457	52,640
Firm FEs	x	x	x	x	x	x
Industry x Prefecture FEs	x	x	x	x	x	x
Industry x Year FEs	x	x	x	x	x	x
Prefecture x Year Trends	x	x	x	x	x	x

Notes: Table reports the effects of the TCZ regulation on exit (Columns 1-3) and sales (Columns 4-6) for SOEs (Panel A) and private firms (Panel B). The sample includes only firms in "responsive" industries (i.e., industries for which the regulation enhanced TFP) in all cases. In Columns 1 and 4, all firms are included. In Columns 2 and 5, only firms that are younger than 11 years are included, and those that are older than 11 years are included in Columns 3 and 6. Standard errors are clustered at the prefecture level. Asterisks denote *p <0.10, **p <0.05, ***p <0.01.

Table 9: Effects on Wages, Management Expenditures, and New Product Output

Outcome Variable:	Wage per Worker (log) (1)	Manage. Expend. per Worker (log) (2)	New Prod. Output (log) (3)	New Prod. Output / Total Output (log) (4)
Panel A: State-Owned Firms				
TCZ * Post-Policy	0.028	0.071	0.129	-0.001
	(0.036)	(0.047)	(0.127)	(0.005)
TCZ * Post-Policy * Polluter	-0.004	-0.052	-0.220	-0.005
	(0.040)	(0.055)	(0.142)	(0.006)
Observations	34,017	33,865	28,054	28,054
Panel B: Private Firms				
TCZ * Post-Policy	0.134***	0.083*	0.043	-0.000
	(0.035)	(0.049)	(0.121)	(0.006)
TCZ * Post-Policy * Polluter	-0.064*	-0.097*	0.064	0.001
	(0.033)	(0.058)	(0.116)	(0.006)
Observations	109,780	109,054	98,252	98,252
Firm FEs	x	x	x	x
Industry x Prefecture FEs	x	x	x	x
Industry x Year FEs	x	x	x	x
Prefecture x Year Trends	x	x	x	x

Notes: Table reports results of tests exploring whether firms hire higher-skilled labor in response to the regulation and/or whether they produce new products. Panel A provides results for SOEs and Panel B provides results for private firms. In Column 1, the outcome variable is the (log) average wage, which we calculate as total expenditures on wages over the number of employees. In Column 2, the outcome variable is (log) expenditures on management divided by number of employees. The outcome variables in Columns 3 and 4 examine output that is specifically associated with new products measured as (log) total output from new products in Column 3 and (log) new product output over total output in Column 4. The sample includes only firms in "responsive" industries (i.e., industries for which the regulation enhanced TFP). Standard errors are clustered at the prefecture level. Asterisks denote *p <0.10, **p <0.05, ***p <0.01.

Table 10: Implications for Aggregate Productivity and Growth

Outcome variable (log):	TFP	TFP	Ratio	Ratio	TFP	TFP	Sales	Sales
Sample split on pre-policy:	**TFP**		**TFP Ratio**		**Age**		**Age**	
	Low	High	Low	High	Low	High	Low	High
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: State-Owned Firms								
TCZ * Post-Policy	-0.052	0.370***	-0.002	0.033***	0.037	0.208***	0.227	0.126**
	(0.087)	(0.067)	(0.012)	(0.008)	(0.238)	(0.057)	(0.215)	(0.051)
TCZ * Post-Policy * Polluter	0.156	-0.235***	0.017	-0.022**	0.108	-0.096	-0.176	-0.044
	(0.100)	(0.077)	(0.015)	(0.010)	(0.259)	(0.064)	(0.229)	(0.059)
Observations	16,690	17,556	18,523	14,549	2,096	32,155	2,092	32,127
Panel B: Private Firms								
TCZ * Post-Policy	0.226***	0.144**	0.011	0.001	0.188*	0.076	0.079	0.037
	(0.080)	(0.062)	(0.010)	(0.008)	(0.110)	(0.056)	(0.100)	(0.051)
TCZ * Post-Policy * Polluter	-0.144*	-0.108	0.003	0.003	-0.171	-0.017	-0.099	-0.018
	(0.082)	(0.067)	(0.011)	(0.008)	(0.114)	(0.059)	(0.099)	(0.055)
Observations	47,857	62,270	48,817	56,268	19,196	90,928	19,187	90,911
Threshold for Low vs. High:								
Median TFP	x	x						
TFP Ratio of One			x	x				
Age (5 years)					x	x	x	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 effects of the TCZ regulation on (log) TFP (Columns 1-2 and Columns 5-6), the TFP ratio described in Section 4 (Columns 3-4), and (log) sales (Columns 7-8) for different sub-samples that help shed light on the regulation's aggregate productivity implications. Using pre-policy values, we split the sample based on the median TFP level in Columns 1-2, a TFP ratio of one in Columns 3-4, and being younger or older than 5 years old in Columns 5-8. Only firms in "responsive" industries (i.e., industries for which the regulation enhanced TFP) are included. 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

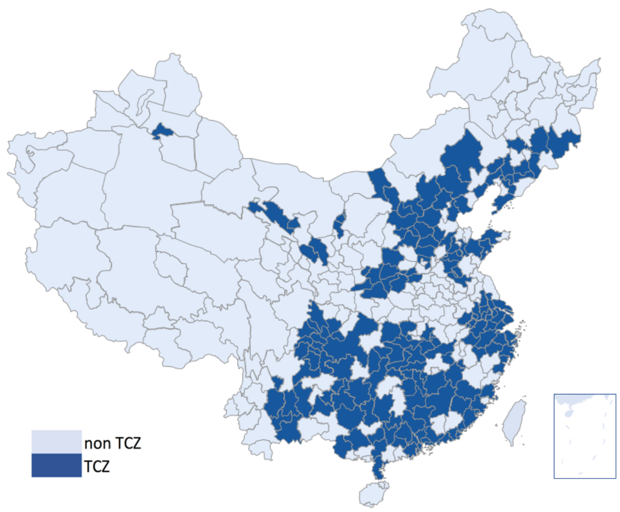
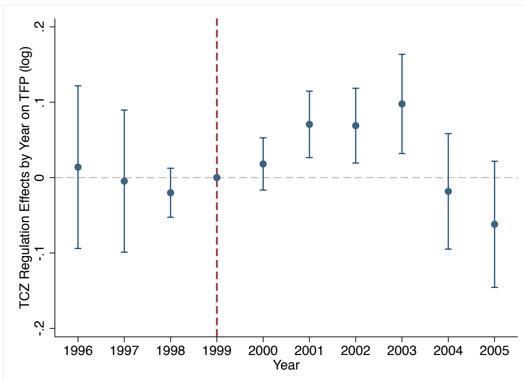
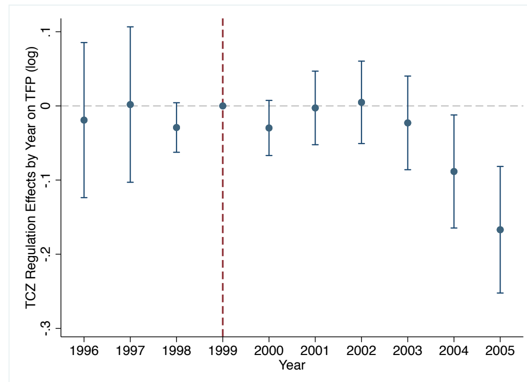


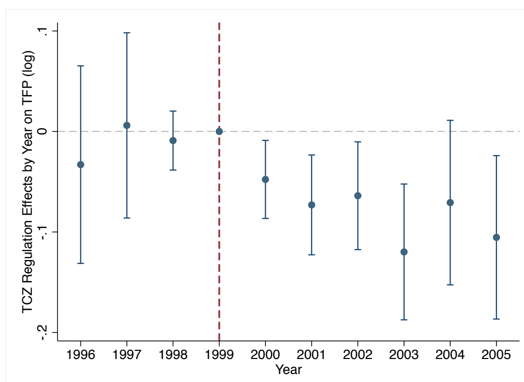
Figure 2: Event Study for Effects of TCZ Regulation on Firm Productivity



(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.[32]

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 explore distributional implications of the TCZ policy and show the pre-policy period prefecture-level characteristics, such as "GDP per capita", "GDP growth rate" 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.

[32] 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

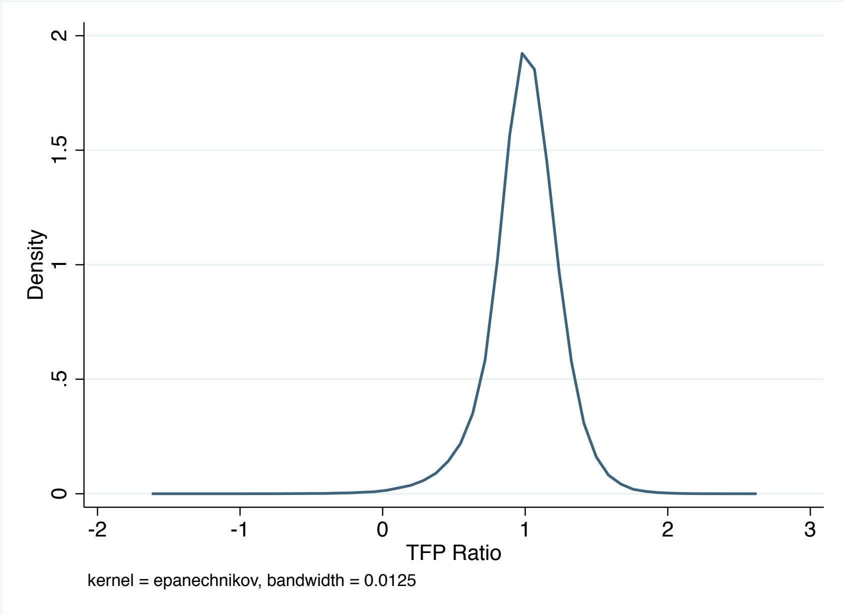
All nominal values (except industrial intermediate input) in CIED dataset and China City Statistical Yearbook are converted to real values in 1998 by using output deflators. The output deflators are constructed following Yang (2015) by using price indexes extracted from the "Urban Price Yearbook 2011" 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 4 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.1. The ratio appears to follow a normal distribution and exhibits about the amount of dispersion expected for a developing country.

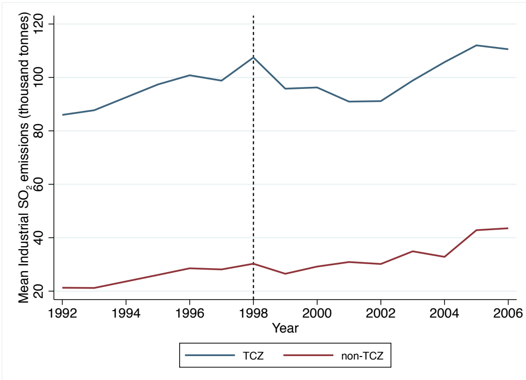
B Appendix: Additional Figures - Online Only

Figure B.1: Dispersion of Log(TFP)

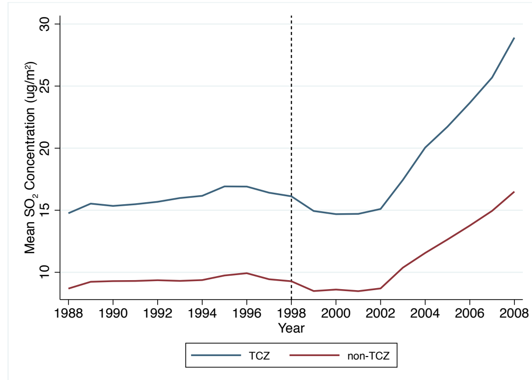


Note: This figure plots the "TFP Ratio" for the full sample period as measured by the ratio between the firm's (log) TFP and the industry-year average (log) TFP.

Figure B.2: SO_2 Emissions and Concentration Over Time



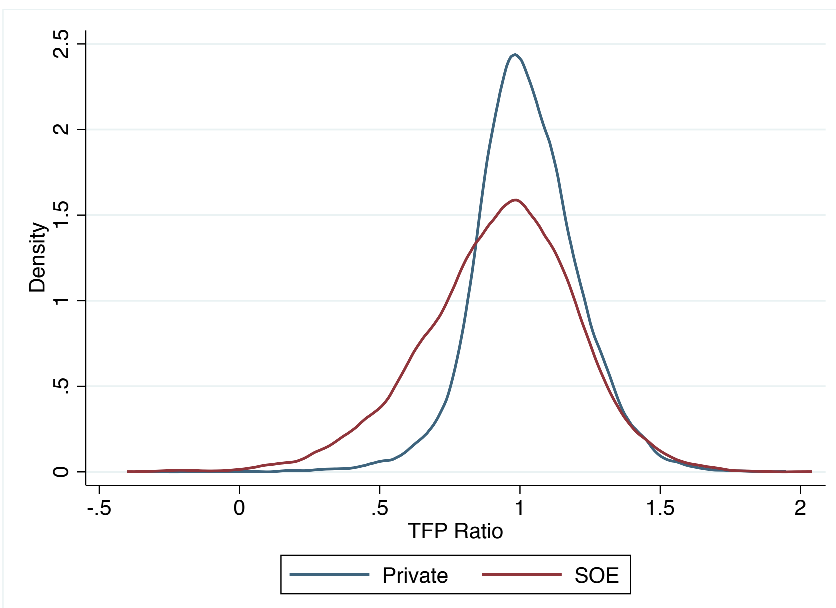
(a) SO_2 Emissions (Reported Data)



(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.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: 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.4: Effect on Alternative Measure of TFP

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 4.1. 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.5: 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 5.3). 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.6: 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.7: Effect on Exit for Firms in "Responsive" Industries (Separately)

Outcome Variable:	Exit	Exit	Exit	Exit
	(1)	(2)	(3)	(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.

Table C.8: Effects on Inputs and Single-Factor Productivity by Firm Ownership (Pilot Years Omitted)

Outcome (log):	Labor (1)	Capital (2)	Intermed. Input (3)	Labor Productivity (4)	Capital Productivity (5)	Intermed. Input Productivity (6)
Panel A: State-Owned Firms						
TCZ * Post-Policy	0.008	0.076*	0.030	0.144**	0.076	0.122**
	(0.027)	(0.039)	(0.047)	(0.059)	(0.060)	(0.051)
TCZ * Post-Policy * Polluter	0.016	-0.083**	0.025	-0.087	0.012	-0.096
	(0.029)	(0.042)	(0.055)	(0.065)	(0.069)	(0.060)
Observations	29,731	29,731	29,731	29,731	29,731	29,731
Panel B: Private Firms						
TCZ * Post-Policy	-0.012	-0.028	0.020	0.099**	0.115*	0.068
	(0.032)	(0.049)	(0.049)	(0.050)	(0.060)	(0.048)
TCZ * Post-Policy * Polluter	0.005	0.060	-0.007	-0.026	-0.080	-0.014
	(0.035)	(0.050)	(0.048)	(0.051)	(0.062)	(0.046)
Observations	106,094	106,094	106,094	106,094	106,094	106,094
Firm FEs	x	x	x	x	x	x
Industry x Prefecture FEs	x	x	x	x	x	x
Industry x Year FEs	x	x	x	x	x	x
Prefecture x Year Trends	x	x	x	x	x	x

Notes: Table reports effects of the TCZ regulation on labor (Column 1), capital (Column 2), intermediate inputs (Column 3), labor productivity (Column 4), capital productivity (Column 5), and intermediate input productivity (Column 6). The years in which data collection was in its pilot stage are omitted for all regressions of Table 7. Single-factor productivity is calculated as value added over each input. All outcomes variables are in logs. The sample includes only firms in "responsive" industries (i.e., industries for which the regulation enhanced TFP). Asterisks denote *p <0.10, **p <0.05, ***p <0.01.

Table C.9: Effects on Exit Based on Pre-Policy Productivity

Outcome variable: *Under/Over Productivity Median:*	Exit Under (1)	Exit Over (2)	Exit Under (3)	Exit Over (4)	Exit Under (5)	Exit Over (6)
Panel A: State-Owned Firms						
TCZ * Post-Policy	0.037	0.005	0.003	0.044	0.009	0.041
	(0.037)	(0.026)	(0.030)	(0.030)	(0.028)	(0.036)
TCZ * Post-Policy * Polluter	0.002	0.034	0.026	0.024	0.011	0.088**
	(0.038)	(0.028)	(0.031)	(0.038)	(0.028)	(0.044)
Observations	16,690	17,591	22,567	11,718	25,710	8,576
Panel B: Private Firms						
TCZ * Post-Policy	0.067*	0.041*	0.070**	0.044*	0.041	0.045
	(0.035)	(0.023)	(0.030)	(0.023)	(0.025)	(0.028)
TCZ * Post-Policy * Polluter	-0.015	-0.009	-0.014	-0.020	-0.013	0.001
	(0.035)	(0.021)	(0.030)	(0.020)	(0.027)	(0.023)
Observations	47,857	62,296	45,498	64,655	46,727	63,423
Productivity measure for split:						
TFP	x	x				
Labor Productivity			x	x		
Capital Productivity					x	x
Firm FEs	x	x	x	x	x	x
Industry x Prefecture FEs	x	x	x	x	x	x
Industry x Year FEs	x	x	x	x	x	x
Prefecture x Year Trends	x	x	x	x	x	x

Notes: Table reports effects of the TCZ regulation on exit for SOEs (Panel A) and private firms (Panel B) for firms that were more or less productive in the year before the regulation was implemented. The sample includes only firms in "responsive" industries (i.e., industries for which the regulation enhanced TFP) and it is split based on the median pre-policy TFP. Results for those that were under the median are in odd-numbered columns and results for those that were over it are in even-numbered columns. Standard errors are clustered at the prefecture level. Asterisks denote $^{*}p<0.10$, $^{**}p<0.05$, $^{***}p<0.01$.