Greening to Grow: Evidence from Environmental Regulation and Industrial Firm Productivity in China*

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Abstract

We study the impact of an environmental regulation on industrial firm productivity in China. In doing so, we show how understanding the economic implications of regulation hinges upon investigating industry and firm heterogeneity. Exploiting variation in regulatory exposure intensity, we find that productivity increased by 5% for regulated firms in less pollution-intensive industries while remaining steady for firms in "dirtier" industries relative to unregulated firms. Further analyses of mechanisms indicate that market selection, reallocation, and within-firm upgrading drive the results. Creative destruction dynamics are concentrated amongst private firms rather than state-owned enterprises, highlighting how political institutions can impact growth.

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1 Introduction

Whether regulation is good or bad for growth and competitiveness is a long-standing debate in many economic and policy settings, and the tension is notoriously contentious in the context of how environmental regulation impacts industrial activity. Setting limits on pollution imposes an implicit tax on regulated firms, which may divert resources away from profitable opportunities and dampen productivity. But regulatory pressure can also catalyze firms to pursue innovative activities—like developing new production methods or adopting more efficient technologies—that can reduce costs and enhance productivity (Porter and Van der Linde 1995). Moreover, insofar as some firms exit, reallocation to more efficient firms may boost aggregate productivity (Syverson 2011).

This "economy versus environment" juxtaposition has dominated the climate policy discourse for decades, garnering substantial attention from economists. However, empirical evidence as to how environmental policies impact productivity remains inconclusive (Dechezleprêtre and Sato 2017; Cohen and Tubb 2018). There is especially little convergence when considering the effects across industries and pollutants. For example, in their study of the U.S. Clean Air Act, Greenstone, List and Syverson (2012) found that productivity declined for regulated firms on average, but the results are mixed across regulated pollutants. Studies of related policies (e.g., carbon taxes) also find no adverse effects on economic outcomes like GDP and employment in aggregate, as the impacts are heterogeneous and concentrated in certain sectors (Metcalf and Stock 2020). Understanding how environmental regulation shapes economic activity may therefore hinge upon developing a better understanding of the underlying heterogeneity. Doing so is increasingly urgent given the intensifying consequences of climate change and ongoing policy debates around "green" industrial policy.[1]

In this paper, we study the impact of an air pollution regulation on firm productivity across China's industrial sector and provide evidence on mechanisms. In doing so, we show how accounting for across- and within-industry heterogeneity is pivotal when evaluating the effects of environmental regulation on the economy. A key motivation behind our focus is the observation that firms may respond to regulations in various ways that come with different

[1] We adopt Juhász, Lane and Rodrik (2023)'s broad definition of industrial policies as "government policies that explicitly target the transformation of the structure of economic activity in pursuit of some public goal."

implications for productivity. For instance, whether firms choose to invest in pollution abatement—and which abatement strategies they employ—may depend on initial pollution levels, production techniques, skills, and the ease of substituting inputs.[2] Compliance costs can therefore vary significantly, and in turn, so can the impact of regulation on productivity.

More specifically, we examine the effects of China's Two Control Zone (TCZ) regulation, which was implemented in 1998 and set objectives for reducing sulfur dioxide (SO_2) emissions in about half of China's prefectures.[3] Although this took place more than two decades ago, it was a time when China was undergoing tremendous economic transformation while also facing severe pollution and transitioning institutions. It thus offers a unique opportunity to provide contemporary lessons for how environmental regulation might impact the economy of other developing and emerging economies at similar stages today, like India and Vietnam.

Using firm-, industry-, and prefecture-level data from several sources, we start by estimating industry-level production functions and construct firm-level total factor productivity (TFP). We then employ a heterogeneous difference-in-differences research design that exploits three sources of variation. The first two are generated by the regulation itself and determine treatment status—whether a firm is located in a regulated prefecture and before/after variation based on the regulation's implementation timing. We then allow the effect of the policy shock to vary based on the intensity of "regulatory exposure," which we define based on whether the firm is in a more or less pollution-intensive industry. Our approach is akin to how others in the literature leverage variation in the degree of exposure to a broader policy or macroeconomic shock determined by pre-shock factors.[4]

The way in which we use industry pollution intensity variation is an important point of departure in our paper relative to previous studies. Although others examining environmental regulations have exploited similar variation, it is usually with the purpose of including less pollution-intensive firms in a control group. This implicitly assumes that they are not affected by the regulation. However, when a regulation sets out to reduce pollution within a geographic space, these firms may still need to make abatement investments to be in

[2]For example, in their study of pulp and paper mills, Gray and Shadbegian (2003) find that the negative effects of pollution abatement on productivity are almost entirely driven by a specific type of mill that faces substantially higher abatement costs due to the technology employed.

[3]Others have studied the effects of this regulation but on other outcomes. We discuss these papers below.

[4]For example, see Autor, Dorn and Hanson (2013) and Autor, Dorn, Hanson, Pisano and Shu (2020).

compliance. They also often compete with firms in dirtier industries for capital and labor. Therefore, we interpret initial pollution intensity at the industry level to be a proxy for regulatory exposure and estimate the effects on firms in both less and more pollution-intensive industries, allowing for closer scrutiny of the *net* effect across industries.

We present three main sets of results. First, we find that average firm productivity increases by 5% for regulated firms in less pollution-intensive industries relative to unregulated firms. For firms in more pollution-intensive industries, productivity declines relative to regulated firms in "less dirty" industries, but it remains steady relative to firms in *unregulated* prefectures. This highlights the importance of considering across-industry heterogeneity, especially if the goal is to understand how a regulation impacts economic activity overall (as opposed to the distributional consequences). That is, while there may be some redistribution across industries within regulated regions, the benefits may offset the costs in aggregate. It also points to how including firms operating in less pollution-industries but located in regulated regions in the control group would have led to a severe bias in our estimates.

One question that emerges from these results is whether the regulation was actually enforced.[5] We examine the effect of the regulation on prefecture-level SO_2 concentrations using granular satellite-based data from NASA and find that concentration levels did indeed decrease by about 5%, suggesting that firms made adjustments in response to the regulation.

We carry out a number of robustness checks and tests probing our underlying identification assumptions as well. We particularly pay close attention to whether our results appear to be driven by other market forces and trends throughout our sample period, as China's economy was experiencing significant structural shifts at the time. For example, although we include prefecture-year trends and industry-year fixed effects throughout our analyses, we also include various controls for China's entry into the World Trade Organization (WTO) and the results remain stable. We also find that our results are not sensitive to how we construct TFP and other checks for whether mark-ups may be driving our results.

Second, we investigate mechanisms on both the extensive and intensive margins by examining several other outcomes—such as exit, sales, inputs, capital age, and wages—and find

[5] When local governments are responsible for enforcing national-level policies, as is the case here, it is not uncommon for principal-agent problems to lead to weak enforcement (Axbard and Deng 2024). This may especially be the case if local authorities have an incentive to protect firms within their jurisdictions.

that a combination of market selection, reallocation, and within-firm "industrial upgrading" by surviving incumbents drives the results. Following the regulation's implementation, the exit rate increases by 1.9 percentage points on average, and firms that were initially less productive (pre-regulation) are much more likely to exit than those that were initially more productive. Firms in less pollution-intensive industries benefit from an increases in sales and inputs (workers and capital), but they also start using these factors more efficiently, as reflected by enhanced single-factor productivity.[6] Firms in more pollution-intensive industries see a slight decline in sales but almost no change in labor and capital along with a decrease in intermediate inputs. This suggests that they scale back production, but they also begin to use intermediate inputs much more efficiently.

For both sets of firms, these productivity gains through intermediate input use efficiency improvements are particularly striking and consistent with the type of upgrading industrial firms can make to comply with environmental regulations. Inputs like energy are both pollution-intensive and costly. Reinforcing our interpretation around industrial upgrading, we also find that firms' capital *age* and intermediate input use per unit of capital decline while average wages increase.

Our third main set of results comes from exploring heterogeneity by firm ownership and pre-regulation productivity levels, which can shed more light on mechanisms as well as implications for allocative efficiency. National laws are frequently enforced by local government officials who, in turn, often benefit from the economic performance of SOEs located within their jurisdictions. China has a long history of protecting SOEs through preferential treatment in the allocation of resources, like subsidies and access to credit (Harrison, Meyer, Wang, Zhao and Zhao 2019; Song, Storesletten and Zilibotti 2011; Barwick, Cao and Li 2021), which can be important for financing large abatement investments.

It appears as though the creative destruction dynamics are concentrated amongst private sector firms, whereas SOEs benefit from an increase in subsidies. For private sector firms, exit increases by much more for previously less productive firms relative to more productive firms. Exit of SOEs is much lower overall, and while there is some exit of SOEs in "dirtier" industries, it is only for small SOEs. This aligns with China's tradition of grasping the large and letting go of the small (Hsieh and Song 2015).

[6] We measure single-factor productivity as value-added over each input.

Although both private firms and SOEs in less pollution-intensive industries experience similar productivity gains, productivity for "dirtier" SOEs declines by 3% relative to unregulated firms whereas it remains steady for private sector firms in these industries. Sales increase for private firms in less pollution-intensive industries and wages increase. On the other hand, the likelihood of receiving government subsidies increases for SOEs (in more pollution-intensive industries). These findings highlight how the underlying political institutions can be an important aspect of whether firms comply with environmental regulation and the subsequent consequences for productivity.

Lastly, while it is not central to our analyses, we explore the implications of our findings for allocative efficiency by examining whether dispersion in TFP declines, as large differences in productivity are often associated with misallocation and productivity losses (Hsieh and Klenow 2009; Song et al. 2011).[7] For firms in more pollution-intensive industries, we find that TFP increases substantially for initially less productive firms whereas there is no change for initially more productive firms. This implies some degree of technological "catch up." At the same time, for firms in less pollution-intensive industries, TFP increases more for those that were already more productive.

Taken together, the findings in our paper challenge the narrative that there is an inherent trade-off between environmental quality improvements and economic growth. In fact, under certain conditions, regulation may even be a tool for catalyzing technological change and growth. The effectiveness, however, may hinge upon the underlying political institutions and the incentives they create for firms to innovate and compete.

Related Literature and Contributions

Our paper is most directly related to the literature on how environmental policies and regulations impact firm performance. Dechezleprêtre and Sato (2017) and Cohen and Tubb (2018) provide reviews, finding that empirical evidence on how regulation impacts productivity remains particularly inconclusive. For example, Greenstone (2002) and Greenstone et al. (2012) find that the U.S. Clean Air Act dampened firm output and productivity, on average, but the effects vary depending on the regulated pollutant. When examining specific

[7]This analysis must be interpreted with caution, however, as dispersion alone does not necessarily signal misallocation (Syverson 2011; De Loecker and Syverson 2021).

industries, others find positive effects.[8] Recent studies of similar policies (like carbon taxes) also find either no adverse effects or even positive effects on economic outcomes in aggregate (Martin, de Preux Laure and Wagner 2014; Metcalf and Stock 2020), underscoring how the effects may be particularly concentrated in specific industries.

These mixed results suggest that understanding how environmental policies shape economic activity may hinge upon developing a better understanding of the underlying heterogeneity. We tackle this in various ways, especially with our focus on exploiting variation in "regulatory exposure" and estimating the effects for firms in both more and less pollution-intensive industries, which provides insight into the *net* effect across industries. Hafstead and Williams (2018) also point out similar issues and study the impact of pollution taxes on employment in a general equilibrium model. Our work differs by studying firm productivity and a regulation (rather than labor and taxes) and by taking a reduced form approach.

We also provide evidence on the mechanisms through which regulation can improve productivity, which are frequently discussed theoretically but empirical evidence remains thin. A closely related body of work finds that environmental policies lead to increases in some innovation outcomes, like R&D expenditures and patenting, especially with a focus on market-based policies like carbon pricing and emissions trading schemes (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). However, the connection between innovative activity and productivity has remained loose so far.[9]

Understanding the impact of environmental regulations is especially important for developing countries as they simultaneously face widespread poverty and inequality along with poor environmental quality. Results from studies of developed countries cannot be readily extended because they operate within settings characterized by different incentives, bureaucratic norms, and institutional capacity. A growing literature examining developing is now emerging with recent advances in data quality and availability (e.g., Duflo, Greenstone, Pande and Ryan (2018); Grenstone, Pande, Ryan and Sudarshan (2022), and others). Two

[8]For example, Berman and Bui (2001a) find that productivity of oil refineries in Los Angeles increased following more stringent regulation.

[9]Liu, Tan and Zhang (2021) make progress on this in their study finding a decline in labor demand following the implementation of an air pollution regulation in China, linking this decline to an increase in labor productivity from technological progress. However, the implications for firm productivity are not studied, as the focus is on labor and the distributional consequences for workers.

papers perhaps closest to ours are Fan, Graff Zivin, Kou, Liu and Wang (2019) and He, Wang and Zhang (2020), who both study water regulations in China and find that firm performance declined.[10] Like much of the related literature on developed countries, though, these papers focus on directly regulated industries or those that are most pollution-intensive.

Some have specifically studied the TCZ regulation but with respect to other outcomes, like infant mortality (Tanaka 2015) and foreign direct investment (Cai, Lu, Wu and Yu 2016).[11] More generally, there is much more evidence on the effects of environmental interventions on labor markets relative to productivity in both the developing and developed country contexts (Berman and Bui 2001b; Morgenstern, Pizer and Shih 2002; Walker 2013; Hafstead and Williams 2018; Colmer 2021). A parallel literature examines how *pollution* impacts worker productivity (Graff Zivin and Neidell 2012; Chang, Graff Zivin, Gross and Neidell 2016 2019; He, Liu and Salvo 2019) but less has been done on regulation itself.

On a related note, a flourishing literature examines the distributional effects of environmental policies, finding significant transitional costs for workers in regulated industries and negative consequences for long-term earnings (Walker 2013). These costs certainly should not be ignored. Our objective is to highlight how less attention has been paid so far to firms that potentially benefit from the reallocation, though, given the potential implications for aggregate productivity and growth. More generally, economic evaluation of regulations in many settings historically focused more on the costs and not the benefits (Sunstein 2020).

Lastly, we contribute to the broader literature on the economics of industrial policy (see Juhász et al. (2023) for a comprehensive review). Industrial policy has long-been a staple in China and it is now also taking center stage in ongoing political debates in the U.S. and Europe, especially with a focus on aiming to foster a "green" transition. There is thus a revived interest in economics to develop a better understanding of how such policies might impact the economy. Historically, the related literature mostly paints a negative picture, but it has taken great strides in recent years and studies applying state of the art empirical methods are providing a more nuanced story (Juhász et al. 2023).

[10] Fu, Viard and Zhang (2021a) studies the effects of air pollution on manufacturing firm productivity, finding that decreasing pollution increases productivity. But this is an estimate of pollution itself rather than a regulation that imposes compliance costs on firms.

[11] Tanaka, Yin and Jefferson (2014), an unpublished working paper, also study how the TCZ regulation impacted firm productivity but they do not have sufficient pre-regulation data, weakening their identification.

2 Institutional Background

2.1 Air Pollution in China

Along with China's rapid economic growth in the 1980s and 1990s came significant increases in air pollution. In particular, sulfur dioxide (SO_2) emissions from the industrial sector were a major contributor to ambient air pollution, reaching 23.7 million by 1995 and creating 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 (1991-1995) statistics, SO_2 pollution levels exceeded the Class II of Chinese National Ambient Air Quality Standards (CNAAQS) in 149 out of 280 surveyed prefectures at the time.[12] 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 regulations in the developing world for decades to come. The first was the Air Pollution Prevention and Control Law (APPCL) in 1987 (He, Huo and Zhang 2002), but it provided only a general provision related SO_2 emissions and excluded the power sector. Consequently, it had very little impact on reducing SO_2 emissions or acid rain. The government amended the law in 1995 with a new article imposing more stringent regulations on specific regions assigned as acid rain control zones and SO_2 pollution control zones, which became known as the Two Control Zones (TCZ) regulation.

2.2 The Two Control Zones (TCZ) Regulation

In 1998, China enacted the Two Control Zones (TCZ) regulation, a command-and-control policy aiming to limit the country's total SO_2 emissions to 2000 levels by the year 2010 and to reduce precipitation pH levels. The national government designated specific prefectures as SO_2 pollution control zones if their average annual ambient SO_2 concentrations exceeded the national Class II standard, if daily average concentrations exceeded the National Class

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

III standard, and if "high" SO_2 emissions were recorded. Prefectures were designated as acid rain control zones if their average annual precipitation pH values 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 "TCZ-regulated" prefectures, spanning about half of China's prefectures and accounting 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.

The TCZ regulation imposed stringent pollution control measures relative to previous efforts, targeting industries involved in the life cycle of coal given their significant contributions to SO_2 pollution.[13] Firms in TCZ-regulated prefectures were required to either shift away from high-sulfur coal in their production processes or install pollution abatement equipment. Most explicitly, all new and existing coal mines with sulfur content higher than 1.5% had to be equipped with coal washing facilities. Existing mines producing coal with sulfur content higher than 3% were to be gradually shut down or have output restricted. All new and existing power plants using coal with sulfur content higher than 1% had to be equipped with desulphurization facilities; existing plants were required to take action to reduce SO_2 emissions before 2000 and establish desulfurization facilities by 2010 (Hao et al. 2001). Construction of new coal mines with sulfur content higher than 3% was also prohibited as was the construction of thermal power plants in large and medium-sized TCZ-regulated prefectures.

Firms in other industries were also required to make substantial investments or adjustments but without output reduction requirements or being forced to shut down. For example, firms in the chemical, metallurgical, nonferrous metal (including concrete), and building materials industries 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 regulation also generally aimed to promote a shift towards cleaner production and technical renovation in all manufacturing processes.

[13] 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).

2.3 Potential Effects on Industrial Firm Productivity

The potential effect of environmental regulations on industrial firm productivity is theoretically ambiguous. On the one hand, regulations can be costly for regulated firms and dampen productivity. Complying often necessitates significant investments in physical and human capital. For example, one common strategy is to install end-of-pipe pollution abatement equipment, like scrubbers that remove pollution at the end of the production process. Such abatement technologies do not come with direct benefits for output or productivity on their own absent other adjustments, so needing to make such investments can divert resources away from other more profitable or productive uses, at least in the short run.

At the same time, regulation may spur technology adoption and innovative activities that ultimately enhance productivity, which we refer to broadly as "industrial upgrading" throughout this paper. For example, firms may develop or adopt new production techniques, processes, and management practices.[14] They also may replace old machinery with more modern equipment, and as technology tends to become more efficient as it advances, they may then require fewer inputs (like raw materials and electricity) per unit of output. Retrofitting boilers can generate significant reductions in heat losses and fuel use. Since heat loss is often a key source of inefficiency and energy bills make up a significant portion of overall operating costs, this can translate into substantial operating cost savings. Even installing end-of-pipe pollution abatement equipment may lead to productivity improvements despite not having direct benefits, as the large investment and installation process may catalyze firms to reevaluate current technologies, techniques, processes, and practices at other stages of production or expose pre-existing inefficiencies (Berman and Bui 2001a).

The potential labor market dynamics further underscore the ambiguity in how environmental regulation may impact firm productivity. Demand for labor may contract if firms scale down, yet continuous operation of abatement equipment requires workers. Firms also may need to acquire new skills, such as by hiring engineers and specialists with expertise in specific production technology and process upgrades that might help reduce pollution. Retrofitting boilers and other abatement activities often involve chemical processes that require re-optimizing inputs accordingly. Furthermore, if marginal production costs decline

[14] For example, in their study of the manufacturing sector in the United Kingdom, Bloom, Genakos, Martin and Sadun (2010) find that better-managed firms use energy more efficiently.

from productivity-enhancing upgrades, labor demand may increase.

One may wonder why profit-maximizing firms would not adopt more efficient techniques and technologies absent regulatory pressure if they are indeed productivity-enhancing. There are several potential explanations. Technological opportunities are constantly evolving, so a combination of uncertainty, incomplete information, and organizational inertia may influence firm behavior. Firm-level constraints and market-level distortions, such as a lack of competition or capital constraints, also may dampen the incentive or ability to invest.

3 Empirical Strategy and Data

3.1 Estimation Approach

To study the effects of the TCZ regulation on industrial firms, we rely on three main sources of variation. The first two were generated by the regulation: whether a firm is located in a TCZ-regulated prefecture or not and before/after variation based on the regulation's implementation timing. This naturally lends itself to a difference-in-differences research design, whereby we can estimate the changes in productivity for firms in regulated prefectures relative to those in unregulated prefectures. Importantly, though, regulatory burdens and compliance costs can differ across and within industries for various reasons, such as differences in initial pollution or productivity levels. In turn, firm response strategies and effects on productivity also may vary across industries in ways that are important for understanding how regulation impacts economic activity across the entire industrial sector.

With this in mind, we also exploit variation in "regulatory exposure" stemming from initial differences in pollution levels across industries and estimate the effects of the regulation on firms not only in the most pollution-intensive industries but also those less pollution-intensive (yet still regulated) industries.[15] We designate industries as being more or less pollution-intensive based on their initial contributions to aggregate SO_2 concentration levels

[15] This approach is in the spirit of others in the literature who exploit variation in the intensity of exposure to various shocks (e.g., Autor et al. (2013) and Autor et al. (2020)).

(see Section 3.2 and Appendix A for more detail) and estimate the following model:[16]

$$log(Y_{it}) = \beta_1(TCZ_p \times Post_t) + \beta_2(TCZ_p \times Post_t \times Dirtier_s) + \alpha_i + \gamma_{st} + \mu_p \times t + \epsilon_{it} \quad (1)$$

where Y_{it} is firm i's (log) total factor productivity (TFP) (or other outcomes) in year t, TCZ_p is a "regulated" indicator equal to one for firms located in TCZ prefectures (p) and zero otherwise, and $Post_t$ is an indicator equal to one in the post-regulation years (from 1999 onwards) and zero otherwise.[17] The variable $Dirtier_s$ is an indicator equal to one for firms in more pollution-intensive industries (s) facing higher compliance costs and zero otherwise.

The main coefficients of interest are β_1 and β_2, with β_1 capturing the change in the outcome for regulated firms in less pollution-intensive industries relative to firms in unregulated prefectures, and β_2 reflects the "extra" change for firms in more pollution-intensive industries *relative to regulated firms in less pollution-intensive industries.* The total change for regulated firms in more pollution-intensive industries relative to *unregulated* firms (i.e., those in non-TCZ prefectures) is the sum of the two coefficients.

The first main assumption of our identification strategy is that trends in productivity would be parallel for firms in TCZ and non-TCZ prefectures absent the regulation. 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, but doing so would absorb effects on less pollution-intensive firms, which is a crucial part of our objective in this paper.[18] In Section 4, we provide evidence that time trends sufficiently control for these concerns, allowing us to address this potential bias while providing a more complete picture of how

[16] As we describe later, data limitations lead us to using 2002 data as the "initial" year, but we discuss how our designations of more and less pollution-intensive industries are correlated with coal consumption in pre-regulation years and how the industries in each category end up being very similar to others in the literature that split industries by pollution intensity.

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

[18] That being said, we do estimate the model with prefecture-year fixed effects in Section 4 to examine how it changes the interpretation and to compare the findings to others in the literature.

the regulation affects industrial activity.

We include firm-level fixed effects (α_i) control for time-invariant mean differences in outcomes across firms and industry-year fixed effects to control for how industries may be affected differently by macroeconomic shocks (γ_{st}). Importantly, we also directly control for how regulated prefectures may have been affected differently by China's entry into the World Trade Organization (WTO). Through various channels, market size shocks like these can increase innovative activity (Aghion, Bergeaud and Van Reenen 2023), and TCZ prefectures may have disproportionately benefited given its higher degree of industrialization. We cluster standard errors at the firm level.

A second key identification assumption of our design is that there are no spillover effects on firms in non-TCZ prefectures (i.e., the stable unit treatment values assumption (SUTVA) holds). Violations could bias the results in either direction. For example, unregulated firms may benefit from competitors facing higher costs, which would put downward pressure on our estimates. On the other hand, firms in unregulated prefectures may experience negative indirect effects if the regulation increased demand for labor in TCZ prefectures. We probe these possibilities in Section 4.2.

3.2 Data Overview

This paper combines data from several sources at different levels of granularity. We use firm-level production and financial data that covers the majority of China's industrial sector from 1996 through 2006, and prefecture- and industry-level data for various purposes throughout the paper. We also use satellite-based pollution data in preliminary analyses of enforcement. This section provides an overview of our sources and preparation procedures. More details can be found in Appendix A.

Firm-Level Production and Financial Data. We start by gathering firm production and financial data 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.[19] This dataset includes detailed production information that we use to construct TFP (e.g., labor, capital, and intermediate inputs), along with other key financial measures

[19]This dataset is also sometimes referred to as the Annual Survey of Industrial Firms (ASIF).

such as sales and wages.

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 after the regulation was implemented so we can study the within-firm effects.

A number of papers use this data (e.g., Hsieh and Klenow 2009; Song et al. 2011; Brandt et al. 2012; He et al. 2020). We implement the widely-adopted preparation procedures developed by Brandt et al. (2012) to match firms over time and drop observations that violate standard accounting principles. We convert all nominal financial values 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 using these data is that we extend the time covered back to 1996-97 when the surveys were first piloted, whereas the first year included for most papers using this data is 1998 (when the survey was fully deployed). Doing so is important for including more pre-regulation years in our analyses but the sample size is much smaller during the pilot years and is weighted towards SOEs.[20] That said, since we limit the sample to firms that appear at least once before and after the TCZ regulation was implemented, this is not as less of a problem in our context as it is for studies examining aggregate productivity and growth trends. We also show that our results are robust to dropping the data from pilot years.

Production Function Estimation. We use numerous measures of productivity throughout this paper, particularly when probing the mechanisms, but we primarily use a firm-level

[20]Our prepared sample includes about 24,000 firms in 1996 and 1997, and this increases to around 165,000 firms in 1998 (and 301,000 firms by 2006).

total factor productivity (TFP) measure that we construct after estimating production functions separately for each 2-digit industry. Our production function estimation approach is in the spirit of Ackerberg, Caves and Frazer (2015), as we use added-value as the dependent variable and intermediate inputs as the proxy, and address the identification issues associated with earlier control function approaches. In practice, we implement the procedure developed by Wooldridge (2009), which performs a consistent estimation but within a single-step generalized method of moments framework (see Rovigatti and Mollisi (2018) for comparisons with empirical applications).

In our industry-level production function estimations, we include year fixed effects and firm fixed effects. Capital stock is a state variable as well as firm age and an indicator for whether the firm is located in a TCZ prefecture 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.

We primarily rely on the insights of Ackerberg et al. (2015) for several reasons. First, using intermediate inputs as the proxy variable as opposed to investments (as in Olley and Pakes (1996)) allows us to alleviate concerns with "lumpy investments," which is common with firm-level data. Second, it corrects the functional dependence problem that both Olley and Pakes (1996) and Levinsohn and Petrin (2003) face. In our robustness checks, we also measure TFP following Levinsohn and Petrin (2003), which still uses intermediate inputs as the proxy variable but assumes that firms adjust immediately after experiencing a productivity shock at no cost. We also estimate the effects on single-factor productivity outcomes—added-value divided by labor, capital stock, or intermediate inputs—as more transparent measures when exploring mechanisms in Section 5.

As is frequently the case, one concern is that our productivity measures are revenue-based rather than quantity-based, so changes in productivity could be associated with firm-specific mark-ups. Unfortunately, data on quantities sold are not available, however we indirectly explore whether mark-ups appear to drive our results later in Sectioon 4.2 by limiting the sample to homogeneous goods markets within which significant mark-ups are less likely.

Determining TCZ Status. We obtained the list of cities designated with TCZ regulatory status from Chinese government documentation (China State Council 1998) and designate firms as regulated if their recorded address is located in a regulated prefecture.[21] The government's designations are made at the prefecture level for acid rain control zones but at the district/county level for SO_2 pollution control zones. We define 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-prefecture are likely to be governed under the same criteria set by the local administration.

Industry-Level Pollution. We gather industry-level SO_2 emissions from the China Statistical Yearbook to designate firms as being in more or less pollution-intensive industries. We define more pollution-intensive industries as those that account 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)).

Prefecture-Level Pollution. Lastly, we use prefecture-level SO_2 data from two sources when examining the regulation's enforcement. We gather SO_2 emissions data from the China Environmental Yearbook, but since the figures are reported by local government officials and may be subject to manipulation (Ghanem and Zhang 2014; Karplus, Zhang and Almond 2018), we also follow Chen, Oliva and Zhang (2022) to derive satellite-based SO_2 concentration levels using data from National Aeronautics and Space Administration (NASA).[22] The satellite-based data are reported monthly at the 60 by 50 kilometer grid level. We match this to prefectures by re-gridding the satellite data to their geolocations using nearest-neighbor remapping, which results in a balanced prefecture-year-month panel from January 1988 through December 2008. See Appendix A for more detail.

Additional Data. We also use other prefecture-level data from the China Statistical Yearbook to examine pre-regulation prefecture characteristics and to include additional control variables in robustness checks.

[21] This assumes the production site is located at the recorded address. We do not observe whether firms have multiple sites, but in their use of the same data, Brandt, Tombe and Zhu (2013) found that more than 95% of observations are single-plant firms.

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

3.3 Summary Statistics

The main estimation sample we use throughout the paper is an unbalanced panel of 127,699 firms and covers the years 1996 through 2006. Of the 97,135 firms located in TCZ prefectures, 36,439 were in more pollution-intensive industries. Of the 30,564 firms located in non-TCZ prefectures, 13,912 were in more pollution-intensive industries.

Table 1 presents summary statistics for key firm-level and prefecture-level variables used throughout this paper for pre-regulation years (1996-1998). As expected, since regulated regions were targeted based upon previous pollution levels and this is strongly correlated with the degree of industrialization, firms in TCZ prefectures are more productive, more capital-intensive, and have higher sales (Panel A). There are also statistical differences in prefecture-level characteristics: on average, TCZ prefectures have higher GDP per capita, populations, and SO_2 emissions (Panel B). Given these differences, an important component of our identification strategy is controlling for how prefectures may evolve differently over time, as discussed in Section 3.

3.4 Preliminary Analysis: Was the Regulation Enforced?

Before moving forward with our primary analyses of productivity, we first explore whether it appears as though the regulation was actually enforced. Lack of enforcement is a common concern in the context of environmental policy due to principal-agent problems. Like in many other cases, the new air pollution rules in the TCZ regulation were defined by the central government while enforcement was delegated to local government officials, who also have incentives to foster economic growth within their jurisdictions. If the regulation was not actually enforced, any differences in TFP that we observe between firms in TCZ and non-TCZ prefectures following implementation may not be actually associated with the regulation or how firms responded.

Previous studies in the literature examining the TCZ regulation's impact on other outcomes, like infant mortality (Tanaka 2015), suggest that the regulation was indeed enforced and reduced pollution. Moreover, documentation of firm closures and development of pollution treatment projects indicate enforcement 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 retrofits, 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).

Nonetheless, we directly examine whether SO_2 declined more in TCZ prefectures relative to non-TCZ prefectures. We use two data from sources that provide pollution information at the prefecture level: emissions data from the China Environmental Yearbook reported by local government officials as well as satellite-based SO_2 concentration data from NASA, since local governments face various competing economic interests that create incentives for manipulating air pollution data (Greenstone et al., 2022; Acemoglu et al., 2020; Fisman and Wang, 2017; others). Misreporting has indeed been documented for the case of air pollution data in China (Ghanem and Zhang 2014; Karplus et al. 2018). Although emissions are the most direct measure of pollution from industrial production, the data on concentrations from NASA satellites provide a complementary objective source.[23]

Starting with a descriptive analysis, we plot the raw SO_2 emissions data from the China Environmental Yearbook (Panel A) as well as SO_2 concentration data from NASA (Panel B) over time for TCZ and non-TCZ regions in Appendix Figure B.1. By both measures, pollution appears to decrease by slightly more in TCZ prefectures after the regulation was implemented relative to non-TCZ prefectures. The decline is particularly steep in Panel A, but the NASA SO_2 concentration data in Panel B also mirror this relationship.[24]

Next, we take a difference-in-differences approach to estimate the effect of the regulation on pollution using NASA's SO_2 concentration data. In addition to not being subject to potential manipulation, the NASA data also cover more prefectures and with a higher time-resolution. We estimate the following model:

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

where $\log(S_{ptm})$ is the log of SO_2 concentration levels (micrograms per square meter) in prefecture p in year t and month m. 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-

[23] Concentration levels are a function of emissions as well as geographic-specific environmental factors.

[24] 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.

implementation period. The coefficient of interest is β_1, capturing the difference in (log) SO_2 concentration for regulated prefectures relative to unregulated prefectures. We include month fixed effects (γ_m) to control for seasonal differences in weather and economic activity, year fixed effects (δ_t) to control for idiosyncratic shocks to economic or industrial activity in all prefectures, and prefecture-year trends ($\mu_p * t$) to control for how industrial activity may change differently over time for prefectures due to local factors.

The results are presented in Appendix Table C.2. When using the full data set covering 1988 through 2008, we find that SO_2 concentration levels decreased by 3.7% in TCZ prefectures relative to non-TCZ prefectures (Column 1). Once limiting the sample to the time period that we study in our firm analysis (1996-2006), the magnitude of the estimate increases 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. The estimates suggest a 4.1% reduction when using data for 1988 through 2008 (Column 3) and a 4.6% reduction when restricting the sample to 1996 through 2006 (Column 4). These results combined with the anecdotal evidence described above suggest that the regulation was likely enforced (at least for some industries and firms).

4 Impact of the TCZ Regulation on Firm Productivity

4.1 Main Results

4.1.1 Graphical Representation of Differences in TFP

We begin our analyses of the effects of the TCZ regulation on firm total factor productivity (TFP) by graphically examining the dynamics before and after the regulation was implemented. We estimate an event study version of Equation 1 (with industry-prefecture fixed effects absorbed) and plot the coefficients along with their 95% confidence intervals in Figure 2. In Panel A, we plot the change in TFP for regulated firms in both more and less pollution-intensive industries *relative to unregulated firms*. That is, the coefficients associated with "cleaner" industries correspond to β_1 of Equation 1, and coefficients associated with "dirtier" industries correspond to the sum of β_1 and β_2. In Panel B, we reproduce the results for "cleaner" industries, but for "dirtier" industries, the coefficients correspond to only β_2 on its own, capturing the change in TFP for these more pollution-intensive industries *relative to less pollution-intensive industries* (rather than relative to regulated industries).

Figure 2 provides three key insights that motivate our empirical estimation approach. First, we detect no statistical differences in TFP in pre-regulation years (conditional on industry-prefecture fixed effects) and the magnitudes of the coefficients are close to zero. This provides some confidence in the first main identifying assumption that, absent the TCZ regulation, there would be no systematic differences in TFP trends for firms in regulated versus unregulated prefectures.[25]

Second, for firms in less pollution-intensive industries, the difference in TFP for firms in regulated prefectures begins to increase quickly relative to unregulated firms following the regulation. There also appears to be no change in TFP for firms in more pollution-intensive industries *relative to unregulated firms* (Panel A). Productivity for both sets of firms begins to decline starting in 2004 relative to those in unregulated non-TCZ prefectures, but given the timing and how this applies to both sets of firms, this is likely due to another shift in the economy around this time (like the removal of tariffs across many industries following China's accession into the WTO).[26]

On the other hand, Panel B of Figure 2 shows that TFP indeed declines for firms in more pollution-intensive industries *relative to firms in more pollution-intensive industries*. This suggests that there may be some reallocation and distributional effects of the regulation across industries within-prefecture. However, relative to unregulated firms within the same industries, there is no decline in the first 4-5 years that the regulation is in place (Panel A). These findings begin to illustrate the importance of estimating the effects of the regulation on regulated firms in both more and less pollution-intensive industries to fully capture the implications for industrial activity.

Lastly, observing an effect on firms in less pollution-intensive industries in TCZ prefectures also indicates that they embody a poor control group. Including them in the control group—as they would be in a standard triple-difference framework—would bias the estimates for firms in more pollution-intensive industries.

[25]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 and are very close to zero.

[26]We conduct several robustness checks later related to entry into the WTO.

4.1.2 Main Econometric Estimates

Table 2 reports our main results from estimating Equation 1, capturing the change in TFP for firms in regulated prefectures relative to firms in unregulated prefectures. We include only firm and year fixed effects in Column 1, and in Column 2, we add industry-by-year fixed effects and prefecture-year linear time trends.[27] In Column 3, we control for how firms in TCZ prefectures may have been affected by China's accession into the WTO differently than those in non-TCZ prefectures because they were already more industrialized.[28]

The findings are consistent with the graphical exposition. Taking Column 3 with the richest set of controls as our preferred estimates, we find that TFP increases by 5.2% for regulated firms in less pollution-intensive industries relative to unregulated firms. For firms in more pollution-intensive industries, there is no change in TFP relative to firms in unregulated prefectures (captured by the sum of the two coefficients). That is, although TFP declines by 4.8% for firms in more pollution-intensive industries less pollution-intensive industries, the change relative to firms in unregulated prefectures is statistically zero.

Column 4 of Table 2 presents results from estimating a triple-difference version of the model, whereby we include prefecture-year fixed effects rather than trends. The change in TFP for firms in less pollution-intensive firms is absorbed and these firms are included in the control group. The coefficient estimate reported therefore captures the change in TFP for firms in more pollution-intensive industries in TCZ-regulated prefectures relative to unregulated firms as well as regulated firms in less pollution-intensive industries. In this case, we find a 5.3% decline in TFP relative to both sets of firms, which is not statistically different from the coefficient estimate found in Column 3.

Estimating the triple-difference model serves three useful purposes. First, it demonstrates the importance of taking a heterogeneous diff-in-diff approach that allows us to estimate changes in TFP for *all* industrial firms in TCZ-regulated prefectures as opposed to only those in the most pollution-intensive industries. Benefits of the regulation for firms in less pollution-intensive industries otherwise would go ignored. Furthermore, with the decline in TFP for firms in more pollution-intensive industries being relative to less pollution-intensive

[27] In Column 1, the interaction between "post" and "dirtier" is also included. All other two-way interactions and main effects are absorbed throughout

[28] The control is an indicator variable equal to one in years post-2001 and zero otherwise interacted with the TCZ treatment indicator.

regulated firms, the findings suggest that the net change in TFP for regulated firms relative to unregulated firms may even be positive. We test this by estimating the difference-in-differences model without allowing the effect to vary based on whether the firm is in a more or less pollution-intensive industry. Appendix Table C.3 reports the results. With the full set of fixed effects and controls, we find that regulated firms experience a 3.1% increase in TFP relative to unregulated firms, on average (Column 3).[29]

Second, we can see that the coefficient estimate for firms in dirtier industries remains nearly the same as the estimate when using only trends (i.e., Column 3), suggesting that the specification with prefecture-year trends effectively deals with selection and adequately controls for how macroeconomic shocks may have impacted firms differently across prefectures over time. That is, the estimate for firms in more pollution-intensive industries is about the same in Column 3 as it is when most flexibly controlling for such shocks using prefecture-year fixed effects in Column 4 (the difference between the coefficient estimates is statistically insignificant). Yet using prefecture-year linear trends provides the advantage of allowing us to identify the effects on *all* industrial firms as opposed to only those in more pollution-intensive industries. We therefore proceed with the heterogeneous difference-in-differences model as our preferred specification, providing a more complete picture of how the regulation impacted productivity across the industrial sector.

4.2 Identification and Robustness Tests

Before exploring the underlying mechanisms of our main estimates, we probe the assumptions of our identification strategy, test the sensitivity of our results to various measurement decisions, and conduct robustness checks related to broader macroeconomic trends.

4.2.1 Spillovers to Unregulated Firms

A key assumption behind our research design is that there are no spillovers, or indirect effects of the regulation, on firms in non-TCZ unregulated prefectures[30] There are a few potential threats to consider in our setting. First, unregulated firms may benefit from costs

[29] This is reasonable given how the sample has about 50k and 77k firms in the more and less pollution-intensive industry categories, respectively.

[30] Or more formally, we asusme that the stable unit treatment value assumption (SUTVA) holds.

being imposed on competitors. Enhanced performance of firms in unregulated prefectures, though, should attenuate our results if this does occur.[31]

Second, spatial sorting could introduce bias if regulated firms shift production to unregulated prefectures, either by moving plants entirely or moving production within-firm to plants in unregulated prefectures for the case of multi-plant firms. Such responses are not uncommon.[32] That said, performance improvements in unregulated prefectures should put downward pressure on our results if this does occur. This type of "leakage" is usually more of a concern when examining the effects of environmental regulations on environmental outcomes for which a reduction is considered an improvement, like emissions. Furthermore, Brandt et al. (2012) reassuringly also finds that about 95% of the firms covered in the data set that we use are single-plant firms.

A third potential threat relates to labor markets. Recent work shows that pollution induced migration away from polluted cities in China, especially for well-educated, higher-skilled workers (Chen et al. 2022; Khanna, Liang, Mobarak and Song 2021). This could bias our estimates in either direction. On the one hand, high-skilled workers may have been moving out of TCZ prefectures around this time given that they were more industrialized (and thus had higher levels of pollution on average relative to non-TCZ prefectures). This could increase productivity of non-TCZ firms, again attenuating the estimates. On the other hand, if such workers moved *to* regulated prefectures in response to pollution reductions (or an increase in demand for their skills), this may dampen productivity for firms in non-TCZ prefectures and put upward pressure on our estimates.

To explore whether our results are contaminated by migration patterns, we designate industries as being "high-tech" based on industry-level technology and human capital intensiveness, and we estimate whether there is a change in the fraction of firms and workers in each prefecture that are in high-tech industries.[33] We aggregate the data to the prefecture-year

[31]Since firms located in TCZ and non-TCZ prefectures may compete in output markets, a related concern could be that unregulated firms are affected indirectly through prices. However, this primarily would threaten our estimates if we used on a sales-based measure of TFP rather than value-added.

[32]For example, Chen, Z., Liu, Suarez Serrato and Xu (2023) study an energy conservation program in China and find that regulated firms cut output and shift production to unregulated firms within-conglomerates.

[33]This is similar to the approach taken by Fu, Viard and Zhang (2021b) in their study of how pollution impacts worker productivity. We use OECD (2011) to guide our assessment of technology intensiveness and Che and Zhang (2017)'s 1995 figures on the percentage of workers with at least a college education in Chinese industries for human capital intensiveness. This results in classifying the following industries as high-tech: chemicals, smelting and pressing of metals, and manufacturing of electronic equipment, telecommunications

level and estimate a difference-in-differences model with year and prefecture fixed effects.[34]

Appendix Table C.4 provides the findings. We find no evidence of the fraction of workers (Column 1) and firms (Column 2) that are in high-tech industries changing differently for TCZ prefectures relative to non-TCZ prefectures. The coefficients are statistically insignificant and their magnitudes are very close to zero. In Columns 3 and 4, we run the same regressions while also controlling for the total number of firms in each prefecture and draw the same conclusion. Although these tests implicitly assume that high-tech workers would stay in high-tech industries if they were indeed to move, the findings provide some reassurance that there is likely little movement of highly-educated and skilled workers and high-tech firms in response to the TCZ regulation.

4.2.2 Data and Variable Construction Choices

We now probe whether our estimates are sensitive to how we constructed the data and key variables of interest. In Column 1 of Appendix Table C.5, we drop the years in which data collection was just being piloted (1996-1997), as the sample size was much smaller and focused more so on state-owned enterprises during these years. The magnitude and statistical significance of the estimate for firms in less pollution-intensive industries decreases by a small amount, but it remains positive and statistically significant, and the effect for firms in more pollution-intensive industries remains statistically zero relative to unregulated firms.

A second potential concern is that the increase in TFP that we observe is driven by price dispersion and firm-level mark-ups since we use a revenue-based measure of TFP (Foster, Haltiwanger and Syverson 2008). Some firms—and particularly those with significant market power—may pass-through regulatory costs to buyers by increasing mark-ups, for example. We do not observe firm-level physical outputs and prices, but to explore this indirectly, we estimate the effects separately for homogenous goods industries versus non-homogenous goods. If we still observe TFP gains for homogenous goods markets and no substantial difference between the two sets of firms, we may be able to assume that mark-ups do not play a significant role, as mark-ups are likely to be less substantial in homogenous goods

equipment, transport equipment, medical and pharmaceutical products, meters and instruments, ordinary machinery, and special purpose equipment.

[34] We drop 1996-97 from this analysis since the data cover much smaller samples during the survey's pilot period. Standard errors are clustered at the prefecture level.

markets. The results are presented in Columns 2-3.[35]

Furthermore, as there are often differences in TFP methods when using alternative methods, we also construct TFP following Levinsohn and Petrin (2003) to ensure our results are not sensitive to our preferred method. We find consistent results (see Column 4). We also examine single-factor productivity measures (value-added over labor, capital, and intermediate inputs) throughout our mechanisms analyses later.

Lastly, we test whether the heterogeneity for firms in less and more pollution-intensive industries is sensitive to the way in which we categorized industries. Rather than using our 1% rule, we instead use the median contribution to sulfur dioxide emissions as the cutoff and find that the results are similar (Column 5).

4.2.3 Macroeconomic Trends

During the period we study, the Chinese economy was undergoing a tremendous economic transition that led to high output growth and reallocation (Song et al. 2011). Much of this has been attributed to the shift in a more market-based economy, as reforms in the late 1980s and 1990s led to the privatization of many previously state-owned firms and state-dominated industries. As part of this, China entered the world trade organization (WTO) in 2001, leading to substantial market expansion for Chinese firms.

Absent the controls that we include throughout our analyses, these underlying trends could confound our estimates if they systematically impacted TCZ-regulated prefectures differently than non-TCZ prefectures. For instance, given how TCZ prefectures were more industrialized, they may have benefited disproportionately from the demand shock following WTO entry. They also may have been affected differently by changes in foreign direct investment (FDI) inflows, although the potential implications are more ambiguous.[36]

In all of our regressions, we control for how such trends overall may have impacted prefectures differently with prefecture-year trends and we control for how WTO's entry may have impacted TCZ prefectures specifically with a variable that interacts the TCZ treatment

[35] We include the following industries in the homogenous goods category: electricity and water production, smelting and pressing of metals, petroleum processing, and mining.

[36] The costs associated with environmental regulation may have deterred foreign investment, but if FDI was coming from a country with stringent environmental regulations as well, they may have been attracted to such regions. The investment inflows could benefit recipients through the financial impacts and/or through learning spillovers.

with an indicator equal to one after 2001. It is also worth noting that Cai et al. (2016) find a decline in FDI for TCZ prefectures relative to non-regulated prefectures, which suggests that these forces may actually attenuate our results.

Nonetheless, we conduct a few additional tests and provide the results in Appendix Table C.6. The results remain consistent with our main findings. In Column 1, we add a control that interacts the WTO-by-TCZ treatment control with the indicator for whether firms are in more pollution-intensive industries in case such industries in particular were impacted differently by WTO entry relative to less pollution-intensive industries. In Column 2, we include additional time-varying prefecture-level controls (population and GDP per capita). In Column 3, we interact these two controls with the indicator equal to one in post-WTO years (and zero otherwise). In Column 4, we interact those controls further with the TCZ treatment indicator. We then examine trade-specific factors by controlling for firm exports in Column 5 and the proportion of capital that is foreign-owned in Column 6. The stability of these results is reassuring.

A final concern relates to the decline of the state-owned sector and privatization, as much of China's economic growth through this period is often argued to have been associated with the shift towards a more market-based economy and reallocation from SOEs to private sector firms. We examine this more closely when testing for heterogeneity across firm ownership in Section 5.4. We then check whether our findings are driven by differences across ownership structures as opposed to the benefits of privatization in Section 5.5, since performance may have been a factor determining which SOEs were privatized. We interpret the findings as suggesting that privatization itself is not a key driver.

5 Mechanisms: Why Did Productivity Increase?

We now turn to examining the channels through which productivity improved, presenting several sets of results indicating that market selection, reallocation, and within-firm upgrading are at play. Given the prominent role of the state sector in China and the implications for firm incentives, we also examine heterogeneity in the mechanisms by ownership, finding that the creative destruction dynamics appear to be driven by private sector firms.

5.1 Firm Exit

Complying with environmental regulations entails large investments in physical and human capital, such as installing pollution abatement equipment and upgrading production technology, processes, and practices. Firms that cannot remain competitive once making such investments may instead choose to exit. If less productive firms exit while more productive firms survive, average firm productivity will increase due to selection. Less productive firms indeed tend to be more sensitive to cost shocks and could be closer to the exit margin. They also face particularly high compliance costs, as energy intensity (and thus emissions intensity) tends to be decreasing in productivity.

Denoting the final year in which a firm appears in our data as the exit year, we construct a binary variable equal to one when a firm exits and all subsequent years.[37] We assign zeros for all pre-exit years for firms that exit as well as for all years if firms never exit during our sample period. Given how our data span eight years following the regulation's implementation, we restrict the sample to include only 1996-2002 (i.e., up to three years post-implementation) when examining firm exit. Shutdowns related to the TCZ regulation are most likely to occur within a few years given the stated timeline requirements to scale down if the necessary steps to reduce pollution were not taken.

As shown in Column 1 of Table 3 (Panel A), we find that, on average, the probability of exit is 1.9 percentage points higher for firms in regulated prefectures relative to unregulated prefectures. This is about an 8% increase over the mean cumulative exit probability of 0.239, and the effect is the same for firms in both more and less pollution-intensive industries.

The propensity to exit is also higher for previously less productive firms, suggesting that market selection indeed contributes to the increase in average productivity. We split the sample based on the distribution of each firm's average pre-regulation productivity and estimate the effects separately for firms in the first and fourth quartiles (see Panel B of Table 3).[38] As shown in Columns 2 and 3, the exit rate was 2.5 percentage points higher for firms in the bottom quartile of the pre-regulation average productivity distribution and 1.6

[37]For the exit regressions only, we extend the panel so that it is balanced from 1998 onwards, whereby the dependent variable is equal to one in all years for firms that exit following the final year that a firm appears in the data.

[38]We first find each firm's average TFP in pre-regulation years and determine quartiles using the distribution of average pre-regulation productivity (rather than using just the final pre-regulation year).

percentage points higher for firms in the top quartile. Although these differential exit rates are consistent with market selection, helping to explain the increase in TFP, observing exit of some of the previously *more* productive firms also raises questions about efficiency. We explore this in Section 6.

5.2 Reallocation of Output and Inputs

Firm exit could lead to lower aggregate production, but it also opens up parts of the market that surviving incumbents—that are more productive—can capture. This would put upward pressure on average productivity without reducing aggregate output. The accompanying enhanced revenue could further enable these firms to invest in capital assets and grow. Reallocation of inputs also can drive productivity growth. For example, high-skilled workers at firms that exit may not have been put to their most productive uses, but when they transition to surviving incumbent firms, they may bring a new set of capabilities.

To examine the role of reallocation, we decompose TFP into its various components. First, though, we estimate the effects on TFP for "stayers" only—defining "stayers" as incumbent firms that survived until at least 2002—since our main initial estimates are likely attenuated by the inclusion of firms that eventually exit. As expected, we find that the magnitude of the effect on TFP increases to 6.1% for firms in less pollution-intensive industries, and as before, TFP does not change for firms in more pollution-intensive industries (Column 1, Panel A of Table 4).

We then estimate the changes in output and production inputs, still restricting the sample to only stayers. Columns 2 and 3 of Panel A report results for value-added and sales, and Panel B provides results for labor (number of employees), capital stock, and intermediate inputs. We find large effects on output for firms in less pollution-intensive industries, with value-added increasing by 7.9% and sales increasing by 4.1%. There is no effect on value-added for firms in more pollution-intensive industries relative to firms in unregulated prefectures but these firms experience a 2% decrease in sales, consistent with how the regulation stipulated that production must scale down if firms do not pursue some particularly costly adjustments. This also suggests that firms must have made some form of efficiency-enhancing adjustments to maintain the same productivity levels while decreasing output.

With respect to inputs, all three increase for firms in less pollution-intensive industries (by 2.6%, 4.3%, and 2% for labor, capital, and intermediate inputs, respectively), consistent with expanding production and growth. The more substantial increase in capital relative to other inputs could be due to either investing in abatement equipment or capital that is put to other productive uses as well, which we explore more in the next sub-section.

For firms in more pollution-intensive industries, the negative sign of the coefficient estimate for labor suggests a slight decrease but it is not statistically different from zero. The positive sign on the coefficient for capital assets for these firms is also consistent with investments in abatement technology but it is not statistically significant either. The more striking difference for firms in more pollution-intensive industries is the 4.4% decrease in intermediate inputs. This could be due to scaling down production, but the decline is more than double the decline in sales. Taken together with no decrease in value-added, these findings begin to suggest more efficient use of intermediate inputs as well (but not labor or capital).

To examine the efficiency of each input directly, we estimate the effects on single-factor productivity measures constructed as (the log of) value-added over labor, capital stock, and intermediate inputs (see Panel C of Table 4). All three improve for firms in less pollution-intensive industries, with more substantial gains for labor and intermediate input productivity than capital productivity. For firms in more pollution-intensive industries, we detect no statistical differences in capital and labor productivity, but strikingly, intermediate input productivity increases by 5.9% relative to unregulated firms. These findings are consistent with how industrial firms often respond to environmental regulation (see Section 2). They also begin to suggest that average firm TFP increases not just because of selection but also within-firm efficiency-enhancing upgrading. We explore this further next.

5.3 Within-Firm Industrial Upgrading

Our findings so far indicate that average firm productivity increased at least in part through an extensive margin effective (i.e., selection and reallocation). We now explore outcomes that allow us to say more about the intensive margin with a focus on the potential compliance strategies described in Section 2.3—technology adoption and energy-efficiency enhancing process innovations, which we broadly refer to as "industrial upgrading."

Table 5 provides results for several outcomes that are consistent with within-firm upgrading. We first examine physical capital investments. We previously found that capital stock levels increased, which could reflect new technology adoption, but it also could indicate production expansion without efficiency-enhancing production technology or technique improvements. To overcome this, we examine capital *age*, constructed as the number of years since the firm last made a "very large" capital investment. We define a large investment period as a year in which the capital is at least three times the firm's average annual. We find that capital age decreases by 0.09 and 0.16 years for firms in less and more pollution-intensive industries, respectively, consistent with firms investing in new technology of some sort (Column 1 of Panel A in Table 5).

Although we cannot determine the specific type of capital in which firms invested, we would not expect productivity improvements to emerge if firms *only* invested in end-of-pipe abatement equipment without making further adjustments. We also would not expect capital age to decrease for firms in more pollution-intensive industries strictly due to an output effect, as sales in these industries decline rather than increase. Therefore, it seems plausible that at least some of the capital investment for both sets of industries includes some degree of upgrading old machinery with more efficient equipment and/or improving processes in other ways that reduce intermediate input use intensity.

Reinforcing this interpretation, the results in Column 2 of Table 5 show that intermediate inputs per unit of capital decrease by 2.3% and 5.5% for firms in less and more pollution-intensive industries, respectively. This could be achieved through production technology upgrades, either by replacing old machinery or improving process efficiency in other ways. Firms in both sets of industries also become more capital-intensive as measured by the capital-labor ratio (Column 3 of Table 5).[39]

Next, we investigate labor inputs more deeply, as technology and process upgrading requires workers in various ways. End-of-pipe pollution abatement equipment requires ongoing labor for its continuous operation, so the increase in the number of workers that we previously found could be associated with hiring new workers to run such machinery.[40] On the

[39] The capital-labor ratio is the (logged) ratio of capital stock over number of employees.

[40] Firms also could reallocate workers away from other activities, but as this would mean moving workers away from production and towards tasks that do not impact output, this might be a more likely explanation for firms in more pollution-intensive industries for which sales decline.

other hand, re-optimizing inputs for more advanced modern equipment and improving process efficiency in other ways may require new capabilities, and in particular, higher-skilled labor. For example, determining optimal fuel mix for new equipment may require specific technical specializations or managerial skills.

We do not observe worker skills or capabilities directly, but we can proxy for quality-adjusted labor by examining firms' total wage bills and average wage rates (total wage bill divided by number of workers). We find that the two measures increase for firms in both more and less pollution-intensive industries (see Columns 4 and 5 of Table 5). The total wage bill and average wage rates increase by 8.8% and 6.3% for firms in less pollution-intensive industries and by 4% and 4.6% for firms in more pollution-intensive industries.

These effects could be driven by a number of forces that are consistent with technological progress. When interpreting them along with the findings in Table 4, they could indicate that firms hire more high-skilled employees that then make up a larger proportion of workers. Productivity-enhancing capital investments may also allow firms to pay higher wages due to more cost-efficient production.

5.4 Heterogeneity by Firm Ownership

The state sector has played a prominent role in China's economy, and local government officials often reaped benefits from the economic successes of SOEs located within their jurisdictions (Barwick et al. 2021). Enforcement may have varied across ownership if officials aimed to protect SOEs, which in turn, can influence firms' response strategies. Through the period that we study, SOEs also frequently benefited from more generous subsidies as well as easier and cheaper access to capital (Harrison et al. 2019; Song et al. 2011; Barwick et al. 2021; Lei 2021), which can be important for financing large capital investments like those associated with pollution abatement. If the effects we find only apply to SOEs, this may signal that the regulation either was not actually enforced or that the effects we find hinge upon additional government support.

Several pieces of evidence suggest that the creative destruction dynamics we observe are driven by private sector firms whereas SOEs may have faced lower regulatory burdens. First, we find that SOEs are far more likely to survive despite pre-regulation productivity levels (see Table 6). The exit rate increases by 3.9 percentage points for private firms in both more and less pollution-intensive industries, whereas it increases only by 1.8 percentage points for SOEs in more pollution-intensive industries and not at all for those in less pollution-intensive industries (Column 1). When splitting the sample by pre-regulation productivity (Columns 2 and 3), we find that the exit rate for less productive private firms is more than double that of more productive private firms (6.4 percentage points versus 2.9 percentage points), consistent with market selection. On the other hand, the estimated coefficients are very small and statistically insignificant for both low and high pre-regulation productivity SOEs.

We also split the samples by firm size (Columns 4 and 5) given China's tradition of "grasping the large" and "letting go of the small" (Hsieh and Song 2015). We define small and large firms as those in the bottom and top quartiles of the pre-regulation firm size distribution.[41] We do find some exit of small SOEs in more pollution-intensive industries, consistent with letting go of the small, but the exit rate is much lower than it is for small private firms. On the other hand, exit of small SOEs is much closer to the exit rate of *larger* private firms, and we find no exit of larger SOEs.

We then examine whether the effects on productivity and the variables related to reallocation and industrial upgrading differ by ownership (see Table 7). As before, we restrict the sample to include only "stayers." For both private firms and SOEs in less pollution-intensive industries, TFP increases, but the effect is 1.5 percentage points stronger for private firms (Column 1). Furthermore, while TFP remains steady for private sector firms in more pollution-intensive industries, it decreases by 3% for SOEs in more pollution-intensive industries. These findings begin to suggest that surviving private firms and SOEs may have responded to the regulation differently.

The underlying mechanisms indeed vary by ownership. Private sector firms appear to both grow and make efficiency-enhancing upgrades. Sales and labor increase by 3.3% and 2.9%, respectively, for firms in less pollution-intensive industries (Columns 2-3, Panel A of Table 7). Their capital stock also increases by 5.6% while intermediate input use remains the same (Columns 4-5, Panel A), suggesting that the efficiency of how such inputs are used improves. Consistent with this, capital age and intermediate input use per unit of capital

[41] This leads us to defining small firms as those with fewer than 74 workers and large firms are those with more than 354 workers.

both decrease (Columns 6-7, Panel A) and average wages increase (Column 8, Panel A).

Private sector firms in more pollution-intensive industries scale back slightly but also appear to upgrade. Sales and labor decrease by 2% and 1.4%, but their capital stock increases by 1.5%. Intermediate input use declines by 5.5%, a much larger effect than the decrease in output and other inputs, and capital age also declines by the same amount as it does for firms in less pollution-intensive industries. These results suggest that firms may be investing in capital or process improvements that allow them to survive and maintain their current levels of productivity.[42] Consistent with this, they also experience the same decline in intermediate input use per unit of capital and increase in wages as private firms in less pollution-intensive industries (Columns 7 and 8 of Panel A).

The story for SOEs diverges from this narrative (Panel B of Table 7). Although TFP increases by 4.1% for SOEs in less pollution-intensive, they experience no change in output, labor, or intermediate inputs. Their capital stock increases but there is no statistically significant change in capital age. At the same time, intermediate input use per unit of capital declined, reflecting some efficiency improvements, and average wages increase, suggesting that they may make some process adjustments that contribute to productivity gains. On the other hand, SOEs in more pollution-intensive industries experienced a 3% decline in TFP that is driven by a 7.6% decrease in sales. Despite this, there is no accompanying decline in labor. Capital age and intermediate input use per unit of capital stock also decrease (with the latter effect being particularly large due to not just a decrease in intermediate inputs but also an increase in capital), and they see no change in average wages.

How can SOEs in pollution-intensive industries make capital investments, maintain their workforce, and survive while bearing losses in sales? It could be that they were not previously fully optimizing such that they had the resources to invest but did not have the incentive to do so absent regulatory pressure. But given the decline in productivity, another potential explanation is that they survived thanks to government subsidies. We estimate the effect on government subsidies, measured as an indicator variable equal to one if the firm received any subsidy income and zero otherwise. Indeed, the likelihood of receiving government subsidies increased by 2.2% for regulated SOEs in more pollution-intensive industries relative to unregulated firms but not for private sector firms (Column 9 of Table 7).

[42]This either could be due to higher compliance costs or perhaps being closer to the frontier.

To summarize, the productivity effects for private sector firms appear to be achieved through the channels we previously explored for the full sample: exit of less productive firms, reallocation, and industrial upgrading. For SOEs, firms in less pollution-intensive industries also appear to have potentially invested in efficiency-enhancing capital and higher-skilled workers without subsidies. On the other hand, SOEs in more pollution-intensive industries appear to have benefited from preferential treatment, with an increase in subsidies supporting investments that ultimately were not productivity-enhancing.

5.5 Alternative Explanations

5.5.1 Human Health and Worker Performance

Lastly, given the increasing evidence of pollution's negative effects on cognition and worker productivity, a reduction in pollution from the regulation also could be a channel through which firm productivity improves. We explore this by testing whether productivity gains vary for firms in high- versus lower-tech industries as defined in Section 4.2. If improved cognition is a first-order driver of our results, we would expect productivity gains to be larger in high-tech industries that tend to employ more high-skilled workers. If the physical health effects of pollution for manual workers (such as asthma-related symptoms) are at play, we might expect the productivity gains to be larger in low-tech industries. In both cases, we expect any such differences to be particularly pronounced for firms in more pollution-intensive industries given the higher exposure to pollution absent regulation.

We find no statistical differences in TFP gains when interacting the policy treatment variables with an indicator equal to one if the firm is in a high-tech industry and zero otherwise (see Column 1 of Appendix Table C.7). The same is true when examining labor productivity as the outcome—the component of overall firm productivity that most closely reflects worker performance (Column 2).

5.5.2 Privatization

Finally, given the significant amount of privatization that occurred through our sample period, one may worry that the heterogeneity across firm ownership was due to a *change* in ownership (and the broader trend towards a market-based economy) rather than differences in incentives and constraints that may arise from preferential treatment.[43] If this is the case, we would expect productivity gains to be larger for firms that go from being SOEs to private relative to those that were already private in the pre-regulation period.

We find that this is not the case (see Appendix Table C.8). In Column 1, we provide the estimates for firms that were already private in pre-regulation years, finding that firms in less and more pollution-intensive industries experience increases in productivity of 6.3% and 1.7%, respectively. We then examine firms that privatize between our pre- and post-regulation periods, defining "privatized firms" as those that were SOEs in pre-regulation years and then shift to having at least 25% or 50% non-state ownership in any post-regulation year (Columns 2 and 3). Productivity gains are a bit lower for firms in less pollution-intensive industries, and productivity declines by 4% for firms in more pollution-intensive industries that privatize according to our definitions.

6 Implications for Allocative Efficiency

Our findings indicate that productivity of regulated firms increased relative to unregulated firms due at least in part to reallocation. This business dynamism is consistent with a process of creative destruction—in our case, by surviving incumbents—which can enhance aggregate productivity and growth (Schumpeter 1942). This may be especially the case when market shares and factors of production are otherwise misallocated, as is often the case in developing country contexts and has been shown to account for much of the disparities in across countries (Hsieh and Klenow 2009; Song et al. 2011; Restuccia and Rogerson 2017). Factor market distortions have been shown to be a key source of TFP losses in China through the 1990s, especially capital misallocation within provinces, which most likely is driven by how government officials favor SOEs (Hsieh and Klenow 2009; Brandt et al. 2013).

Our intention in this paper is not to comprehensively study how the TCZ regulation impacted allocative efficiency, but we explore the potential implications a bit by examining TFP dispersion, which is frequently associated with losses due to misallocation. We remain cautious in our interpretation, though, as this is not always necessarily the case (Syverson

[43] Evidence as to the effects of privatization on firm productivity in China are mixed (Brandt et al. 2012; Hsieh and Song 2015; Chen, Igami, Sawada and Xiao 2021).

2011; De Loecker and Syverson 2021).

We split the sample by initial (pre-regulation) productivity levels and estimating the effect for low and high productivity firms.[44] The results are presented in Table 8, with initially low productivity firms in Panel A and high productivity firms in Panel B. Column 1 provides the estimates for TFP, and in Columns 2-4, we estimate the effects on single-factor productivity. For all productivity measures, we find that the difference in productivity appears to diminish for firms in more pollution-intensive industries. Productivity increases substantially more for previously less productive firms relative to previously more productive firms. For example, in Column 1, we find that TFP increases by 7.5% while there is no statistically significant effect for more pollution-intensive previously productive firms. These findings suggest the regulation may have catalyzed some technological "catch-up" amongst previously low productivity firms.

On the other hand, for firms in less pollution-intensive industries, productivity increases for both low and high productivity firms but the effects are larger for firms that were already more productive. This indicates a slight increase in dispersion. While we must remain speculative, this could suggest that firms in these industries were already closer to the technological frontier and the progress emerging from their investments pushed the frontier outwards. On the other hand, for firms in more pollution-intensive industries, the regulatory pressure may have induced low productivity firms to "catch up."

7 Conclusion

This paper advances the literature on how environmental policies impact economic activity through a study of an air pollution regulation on industrial firm productivity in China. To do so, we focus on unpacking the substantial heterogeneity in how firms respond to more stringent regulation both across- and within industries. We find that firms in less pollution-intensive industries experienced a 5% increase in productivity relative to unregulated firms, and while productivity for firms in more pollution-intensive industries declined relative to regulated firms in "less dirty" industries, it remained steady relative to unregulated firms.

[44] We find firms' average TFP in pre-regulation years and use that distribution to determine previously "low" productivity (the bottom quartile) and "high" productivity (the top quartile) firms.

Results from our exploration of the underlying mechanisms suggest that a combination of market selection, reallocation, and within-firm industrial upgrading drives the results. However, the creative destruction dynamics appear to be concentrated amongst private sector firms as opposed to state-owned enterprises. Whether regulation induces innovative activity that then translates into productivity gains, therefore, may hinge upon the underlying institutions and the incentives they create. Taken together, our findings highlight the importance of considering the effects across the entire sector when evaluating how environmental regulation shapes economic activity.

Our paper comes with timely policy implications, as industrial policy has garnered renewed interest from both policymakers and economists, and it frequently embeds "green" objectives. We also study this question at a time when China was experiencing both tremendous economic growth and degrading environmental quality. With numerous other countries undergoing similar transitions today, our findings may be especially of interest to policymakers in developing and emerging economies who are striving to foster economic growth while also tackling urgent environmental challenges. Policy goals aiming to foster economic growth and protect the environment are often framed as being at odds. Our study suggests that this need not be the case.

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MAIN TEXT TABLES

Table 1: Summary Statistics of Firm and Prefecture Characteristics in the 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)	5.55	5.41	0.14***	1.57	1.64	122,243	38,609
Labor (log)	5.47	5.49	-0.02***	1.33	1.40	122,243	38,609
Capital (log)	9.19	9.08	0.11***	1.88	1.88	122,243	38,609
Capital-Labor Ratio	87.51	69.38	18.13***	308.33	226.06	122,243	38,609
Sales (millions)	76.24	64.33	11.91***	418.49	630.60	122,243	38,609
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 Productivity Results - Change in TFP for Regulated Firms Relative to Unregulated Firms

Outcome Variable (log):	TFP (1)	TFP (2)	TFP (3)	TFP (4)
TCZ * Post	0.038***	0.042***	0.052***	
	(0.009)	(0.009)	(0.009)	
TCZ * Post * Dirtier	-0.039***	-0.048***	-0.048***	-0.053***
	(0.013)	(0.012)	(0.012)	(0.013)
Observations	762,957	762,957	762,957	762,922
Mean Dep. Var.	5.676	5.676	5.676	5.676
Firm FEs	x	x	x	x
Year FEs	x			
Industry x Year FEs		x	x	x
Prefecture x Year Trends		x	x	
WTO Control			x	x
Prefecture x Year FEs				x

Notes: Table reports estimates for the effect of the TCZ regulation on (log) TFP following Equation 1. The model in Column 1 includes firm and year fixed effects. In Column 2, we add industry-year fixed effects and prefecture-by-year linear trends. In Column 3, we add a control for how firms in TCZ prefectures may be affected differently by WTO entry (an indicator variable equal to one in years after 2001 interacted with the TCZ treatment indicator). In Column 4, we include prefecture-by-year fixed effects, which is akin to a triple-difference model and absorbs the estimate for firms in less pollution-intensive industries. The two-way interaction between "post" and "dirtier" is included in the model for Column 1. All other interactions and main effects drop out. Standard errors are clustered at the firm level. Asterisks denote *p <0.10, **p <0.05, ***p <0.01.

Table 3: Change in Exit Rates for Regulated Firms Relative to Unregulated

Estimation Sample:	**Full Sample** (1)	**Low Pre-Reg TFP** (2)	**High Pre-Reg TFP** (3)
TCZ * Post-Policy	0.019***	0.025***	0.016**
	(0.004)	(0.007)	(0.007)
TCZ * Post-Policy * Dirtier	0.003	0.013	0.001
	(0.005)	(0.012)	(0.010)
Observations	672,871	163,702	174,892
Mean Dep. Var.	0.239	0.305	0.180
Firm FEs	x	x	x
Industry x Year FEs	x	x	x
Prefecture x Year Trends	x	x	x
WTO Control	x	x	x

Notes: Table reports estimates for the change in the propensity to exit for regulated firms relative to unregulated. Dependent variable is an indicator equal to one the year a firm exits (i.e., when it last appears in our data) and all subsequent years, and zero otherwise. Sample is restricted to 1996 through 2002. All firms are included in Column 1. We then estimate the effects separately for firms in the bottom (Column 2) and top (Column 3) quartiles of the average pre-regulation TFP distribution. Standard errors are clustered at the firm level. Asterisks denote *$p<0.10$, **$p<0.05$, ***$p<0.01$.

Table 4: Evidence of Reallocation - Changes in TFP, Output, Inputs, and Single-Factor Productivity for Surviving Incumbents Relative to Unregulated Firms

	(1)	(2)	(3)
Panel A: Effects on TFP and Output			
Outcome Variable (log):	TFP	Value Added	Sales
TCZ * Post-Policy	0.061***	0.079***	0.041***
	(0.010)	(0.011)	(0.009)
TCZ * Post-Policy * Dirtier	-0.058***	-0.077***	-0.060***
	(0.015)	(0.016)	(0.013)
Observations	619,044	619,044	618,713
Panel B: Effects on Input Levels			
Outcome Variable (log):	Labor	Capital	Intermed. Inputs
TCZ * Post-Policy	0.026***	0.043***	0.020**
	(0.006)	(0.009)	(0.009)
TCZ * Post-Policy * Dirtier	-0.033***	-0.032**	-0.064***
	(0.009)	(0.013)	(0.014)
Observations	619,044	619,044	619,044
Panel C: Effects on Single-Factor Productivity			
Outcome Variable (log):	VA/Labor	VA/Capital	VA/Intermed. Inputs
TCZ * Post-Policy	0.053***	0.036***	0.059***
	(0.011)	(0.013)	(0.010)
TCZ * Post-Policy * Dirtier	-0.045***	-0.045***	-0.014
	(0.015)	(0.017)	(0.013)
Observations	619,044	619,044	619,044

Notes: Table reports estimates for changes in various outcomes related to reallocation for surviving incumbent firms in regulated prefectures relative to those in unregulated prefectures. "Stayers" are defined as incumbents that survived until at least 2002. In Panel C, single-factor productivity measures are constructed as value-added over each input (number of workers, capital stock, and intermediate inputs). All regressions include firm fixed effects, industry-year fixed effects, prefecture-year linear trends, and the WTO-by-TCZ treatment control. Standard errors are clustered at the firm level. Asterisks denote *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

Table 5: Evidence of Industrial Upgrading by Surviving Incumbents

Outcome Variable (log):	Capital Age (1)	Intermed./K (2)	K-L Ratio (3)	Tot. Wage Bill (4)	Avg. Wage Rate (5)
TCZ * Post-Policy	-0.087***	-0.023**	0.017*	0.088***	0.063***
	(0.026)	(0.011)	(0.009)	(0.008)	(0.007)
TCZ * Post-Policy * Dirty	-0.077**	-0.032**	0.001	-0.048***	-0.017*
	(0.031)	(0.016)	(0.013)	(0.012)	(0.010)
Observations	619,044	619,044	619,044	617,176	617,176
Mean Dep. Var.	2.911	0.845	3.612	7.488	2.139

Notes: Table reports estimates for various measures related to technology and production process upgrading. The dependent variables from left to right are capital age, intermediate inputs, capital-labor ratio, total wage bill, and average wages constructed as described in Section 5. All regressions include firm fixed effects, industry-year fixed effects, prefecture-year linear trends, and the WTO-by-TCZ treatment control. Standard errors are clustered at the firm level. Asterisks denote *p <0.10, **p <0.05, ***p <0.01.

Table 6: Heterogeneity in Exit by Firm Ownership, Initial Productivity, and Initial Size

Estimation Sample:	Full Sample	Pre-Regulation TFP		Pre-Regulation Size	
		Low	High	Small	Large
	(1)	(2)	(3)	(4)	(5)
Panel A: Private Firms					
TCZ * Post-Policy	0.039***	0.064***	0.029***	0.069***	0.023***
	(0.005)	(0.011)	(0.009)	(0.011)	(0.008)
TCZ * Post-Policy * Dirtier	-0.005	0.023	-0.009	-0.019	-0.008
	(0.007)	(0.019)	(0.012)	(0.014)	(0.013)
Observations	459,406	83,117	122,595	106,544	105,517
Mean Dep. Var.	0.227	0.272	0.186	0.296	0.156
Panel B: State-Owned Enterprises					
TCZ * Post-Policy	-0.001	0.007	-0.004	-0.004	0.008
	(0.006)	(0.009)	(0.011)	(0.012)	(0.009)
TCZ * Post-Policy * Dirtier	0.018**	0.020	0.024	0.039**	0.005
	(0.009)	(0.015)	(0.016)	(0.017)	(0.013)
Observations	201,961	76,751	49,752	47,434	82,732
Mean Dep. Var.	0.254	0.328	0.157	0.362	0.175

Notes: Table reports estimated heterogeneous effects on exit rates by firm ownership, pre-regulation firm-level TFP, and pre-regulation firm size (number of employees). Dependent variable is an indicator equal to one the year a firm exits and all subsequent years and zero otherwise. Sample is limited to include years 1996 through 2002 and all regressions include firm fixed effects, industry-year fixed effects, prefecture-year trends, and the WTO-by-TCZ treatment control. Column 1 includes all firms. In Columns 2 and 3, we include only firms in the bottom and top quartiles of the pre-regulation average TFP distribution, respectively. In Columns 4 and 5, we include only the bottom and top quartiles of the pre-regulation average firm size (number of employees) distribution. Standard errors are clustered at the firm level. Asterisks denote $^{*}p<0.10$, $^{**}p<0.05$, $^{***}p<0.01$.

Table 7: Heterogeneity in Mechanisms by Firm Ownership

Dep. Var. (log):	TFP (1)	Sales (2)	L (3)	K (4)	M (5)	Cap. Age (6)	M/K (7)	Avg. Wage (8)	Subsidies (9)
Panel A: Private Firms									
TCZ * Post	0.056***	0.033***	0.029***	0.056***	0.016	-0.172***	-0.040***	0.066***	0.003
	(0.013)	(0.011)	(0.008)	(0.013)	(0.012)	(0.034)	(0.015)	(0.010)	(0.005)
TCZ * Post * Dirtier	-0.052***	-0.053***	-0.043***	-0.041**	-0.055***	-0.026	-0.014	-0.001	-0.010
	(0.018)	(0.016)	(0.012)	(0.018)	(0.017)	(0.040)	(0.020)	(0.013)	(0.006)
Observations	450,846	450,745	450,846	450,846	450,846	450,846	450,846	449,767	450,146
Mean Dep. Var.	5.931	10.307	5.258	8.769	10.022	3.032	1.253	2.202	0.141
Panel B: State-Owned Enterprises									
TCZ * Post	0.041**	0.016	0.006	0.037***	-0.011	-0.026	-0.048***	0.048***	-0.008
	(0.017)	(0.014)	(0.008)	(0.013)	(0.016)	(0.042)	(0.018)	(0.011)	(0.007)
TCZ * Post * Dirtier	-0.071***	-0.076***	-0.019	-0.011	-0.084***	-0.120**	-0.074***	-0.051***	0.022**
	(0.026)	(0.023)	(0.015)	(0.020)	(0.025)	(0.052)	(0.026)	(0.017)	(0.010)
Observations	163,421	163,195	163,421	163,421	163,421	163,421	163,421	162,655	163,052
Mean Dep. Var.	5.531	9.588	5.584	9.471	9.209	2.590	-0.262	1.969	0.174

Notes: Table reports results from estimating the effects of the TCZ regulation on key outcomes separately for private sector firms and SOEs to examine heterogeneity in mechanisms by ownership type. Dependent variables in Columns 1-5 are TFP, sales, number of workers, and intermediate inputs, respectively. Columns 6-8 use variables related to industrial upgrading as dependent variables: capital age, intermediate inputs per unit of capital stock, and average wages. In Column 9, we examine whether firms received government subsidies, using an indicator equal to one if they did and zero otherwise as the dependent variable. All regressions include firm fixed effects, industry-year fixed effects, prefecture-year trends, and the WTO-by-TCZ treatment control. Standard errors are clustered at the firm level. Asterisks denote *p <0.10, **p <0.05, ***p <0.01.

Table 8: TFP Dispersion - Heterogeneity in Productivity Effects by Initial Productivity

Outcome Variable:	TFP (1)	VA/L (2)	VA/K (3)	VA/M (4)
Panel A: Less Productive Pre-Regulation				
TCZ * Post	0.075***	0.068***	0.072***	0.077***
	(0.023)	(0.024)	(0.027)	(0.023)
TCZ * Post * Dirtier	0.049	0.043	0.071	0.081*
	(0.046)	(0.048)	(0.050)	(0.042)
Observations	123,591	123,591	123,591	123,591
Panel B: More Productive Pre-Regulation				
TCZ * Post	0.125***	0.126***	0.095***	0.117***
	(0.019)	(0.019)	(0.023)	(0.020)
TCZ * Post * Dirtier	-0.116***	-0.095***	-0.117***	-0.021
	(0.025)	(0.025)	(0.030)	(0.024)
Observations	184,280	184,280	184,280	184,280

Notes: Table reports results from estimating the changes in productivity separately for firms that were initially less versus more productive to examine the effects on productivity dispersion. The dependent variable is TFP in Column 1. In Columns 2-4, we examine single-factor productivity as measured by value-added over labor (number of workers), capital stock, and intermediate materials. All regressions include firm fixed effects, industry-year fixed effects, prefecture-year trends, and the WTO-by-TCZ treatment control. Standard errors are clustered at the firm 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

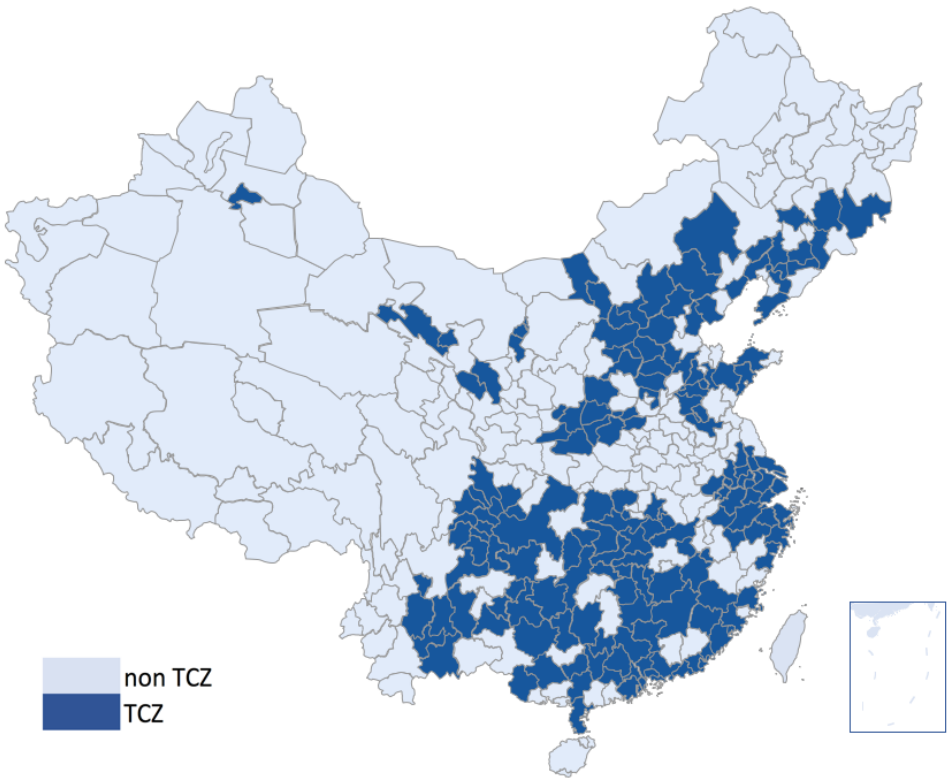
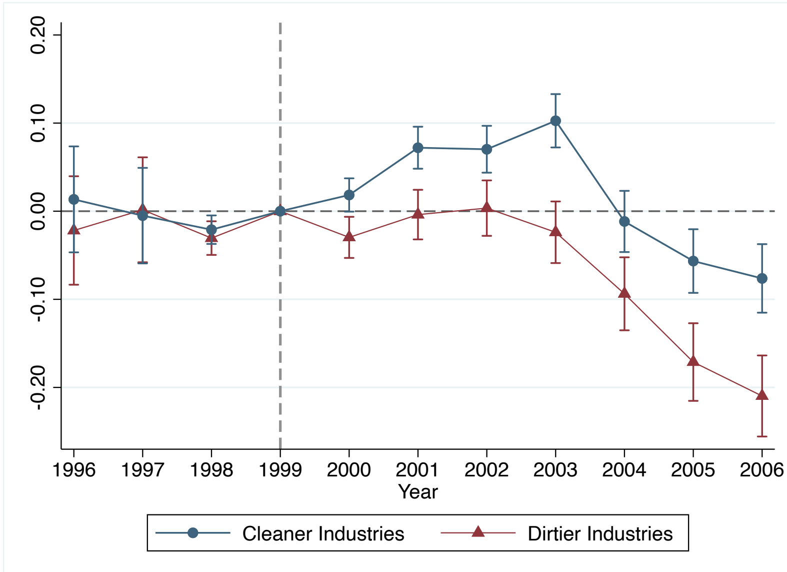
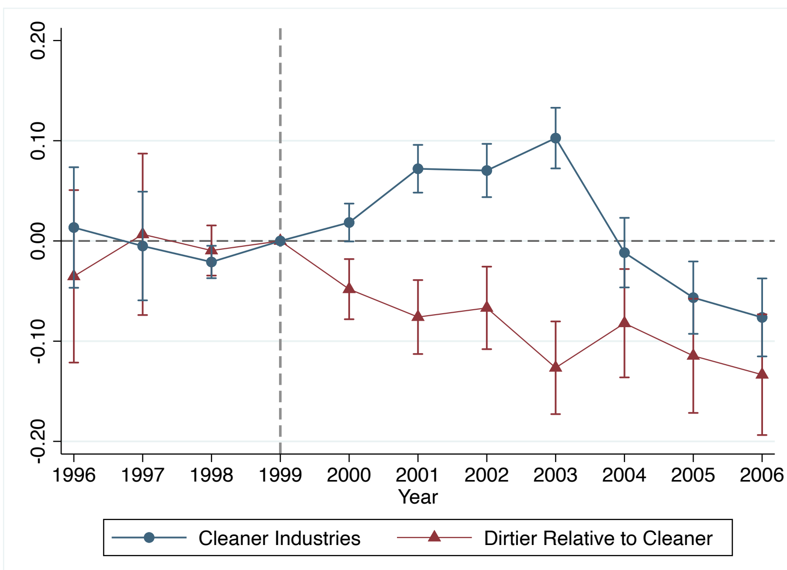


Figure 2: Changes in TFP for Firms in More and Less Pollution-Intensive Industries



(a) Total Effects for Firms in TCZ Prefectures Relative to Non-TCZ Prefectures



(b) Total Effect for Firms in Less Pollution-Intensive Industries and Effect on More Pollution-Intensive *Relative to Less Pollution-Intensive* Industries

Note: Figures plot estimated coefficients and their 95% confidence intervals from a dynamic version of Equation 1 that interacts the TCZ treatment with annual indicator variables (absorbing industry-prefecture fixed effects). Dependent variable is (log) firm TFP. Panel A presents differences in TFP for firms in less and more pollution-intensive industries located in TCZ-regulated prefectures relative to firms located in non-TCZ prefectures (i.e., the "total" effects). Cleaner industry estimates correspond to β_1 from Equation 1 and dirtier industry estimates correspond to the sums of β_1 and β_2. In Panel B, estimates for dirtier industries are the effects *relative to firms in less pollution-intensive industries located in TCZ prefectures* (i.e., β_2 only).

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. Our main results are robust to dropping 2003 as well. We do not provide these estimations in the paper but can do so upon request.

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 (about 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 (about 0.08%).

Firm Ownership

We categorize firms as being either "state-owned" or "private" according to their capital sources. We consider firm to be state-owned enterprises (SOEs) if they receive more than 50% of paid-up capital from state sources in that year. All other firms are considered private, including those with foreign capital as long as the foreign capital and internal capital sums to more than 50% of the total. When conducting the heterogeneity analyses for SOEs versus private sector firms, we group firms into static categories as being SOEs or private based on their modal ownership structure throughout the sample period. For example, if a firm is considered private according to the above definition for 6 of the 10 years for which we have data, we define it as a private sector firm. We omit firms in the heterogeneity analyses if they are bimodal.

In robustness checks, we consider an alternative definition based only on pre-regulation years to capture firms' original ownership structure when the regulation was implemented. This helps rule out the potential for privatization itself to have driven the results as opposed to differences in incentives and constraints between ownership structures. To do so, we carry out a similar approach of finding the modal ownership structure but use only the years 1996 to 1998 rather than the full sample period.

When examining mechanisms, we also examine whether privatization explains our results (as opposed to differences in initial ownership structure) by testing whether the regulation impacted outcomes differently for firms that went from being SOEs prior to the regulation and became private firms at any point afterwards. For this analysis, we define firms as being originally SOEs as before and then consider them privatized if, for any post-regulation year, at least 25% or 50% of paid-up capital is accounted for by private or foreign 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.[45]

[45] 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).

Firm Age

We calculate firm age using firms' reported founding year. We assume all firms are founded after the year 1800 and consider founding year missing if firms reported an earlier founding year. If firms indicated different founding years at different points when surveyed, we use the mode to calculate firm age.

A.2 Prefecture-level data

We collect socio-demographic prefecture-level data from the China City Statistical Yearbook. We primarily use this data to examine the pre-policy period prefecture-level characteristics, such as GDP per capita and population, and also when carrying out robustness checks. We also collect prefecture-level SO_2 data from the China Environmental Yearbook and NASA MERRA-2 when examining the effectiveness of the policy in reducing SO_2 pollution.

A.3 Industry-level data

We use industry-level SO_2 emissions and coal consumption data to assign industries as being more or less pollution-intensive. 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.

We gather data on SO_2 emissions from the China Statistical Yearbook 2002, which contains data for the year 2001. Unlike prefecture-level emissions data, which is subject to potential misreporting by local government officials (Karplus et al. 2018), industry-level data is less likely to be manipulated as emission levels of different industries are inherently heterogeneous, depending on the industry's characteristics. Although using pre-regulation data would be idea for categorizing whether industries were initially more or less pollution-intensive, as the 2001 may data may reflect changes that occurred in response to the regulation, earlier data is much more limited. The 2001 data contain 40 industries compared to just 20 in 1997 data (i.e., pre-regulation). The 2001 also includes the number of firms in each industry, which allows us to compute average SO_2 emissions per firm in each industry.

To alleviate the concern that our categorization of industries may have been different if we had our ideal data, we examine the correlation between the data we do have for 1997 and 2001 and find that it is very high (0.98 for SO_2 emissions and 0.99 for coal consumption, which is a key determinant of SO_2 emissions). This provides us with more confidence that the 2001 data is a reasonable good proxy for industry pollution levels, as suggested by Cai et al. (2016).

After calculating each industry's share of total SO_2 emissions and coal consumption, we follow Greenstone (2002) by using a 1 percent share cutoff to categorize industries as being more or less pollution-intensive. In our context, if the industry's shares of total emissions and coal consumption are 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, we include thermal power in the more pollution-intensive group whereas we define other power generation industries (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) defines 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. Our categorization also includes four more industries that also qualify: 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").

A.4 Final Sample Preparation

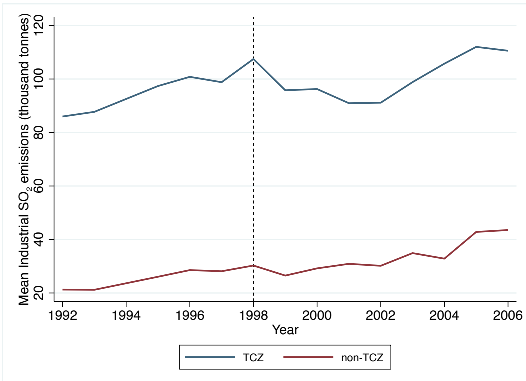
All nominal values (except industrial intermediate input) in CIED dataset and China City Statistical Yearbook are converted to real values in 1998 by using output deflators. The output deflators are constructed following Yang (2015) by using price indexes extracted from the "Urban Price Yearbook 2011" 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.

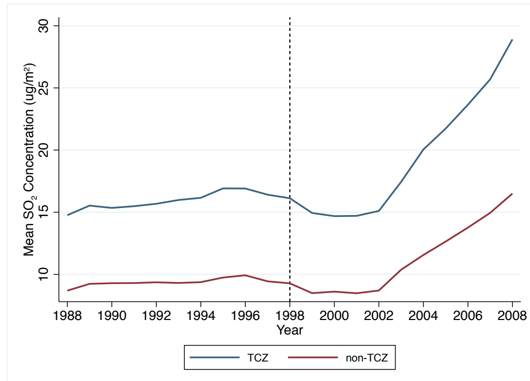
Once we match all of the aforementioned data and keep observations for which we have the information needed to determine treatment status, we keep only firms for which we observe at least once before and once after the policy was implemented, since we conduct a within-firm analysis. This is the step 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. Descriptive statistics for our final estimation sample are provided in Table 1 and discussed in Section 3.2 of the main text.

B Appendix: Additional Figures - Online Only

Figure B.1: SO_2 Emissions and Concentration Over Time



(a) SO_2 Emissions (Reported Data)



(b) SO_2 Concentration (NASA Data)

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

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 (CIC 4411)

Notes: Table provides list of industries included in our sample, their pollution-related information, and indication of whether we define them as being one of the “dirtiest” (pollution-intensive) industries in our main analyses. Firms in industries accounting for more than 1% of China’s coal consumption are defined as being in the “dirtiest” category.

Table C.2: SO_2 Concentration for TCZ Prefectures Relative to Non-TCZ Prefectures

Sample Period:	1988-2008	1996-2006	1988-2008	1996-2006
Dep. Var. (log):	SO_2	SO_2	SO_2	SO_2
	(1)	(2)	(3)	(4)
TCZ * Post	-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: Average Change in Productivity Across All Industries for Regulated Firms Relative to Unregulated

Outcome Variable (log):	TFP	TFP	TFP
	(1)	(2)	(3)
TCZ * Post	0.027***	0.021***	0.031***
	(0.006)	(0.007)	(0.007)
Observations	762,957	762,957	762,957
Mean Dep. Var.	5.676	5.676	5.676
Firm FEs	x	x	x
Year FEs	x		
Industry x Year FEs		x	x
Prefecture x Year Trends		x	x
WTO Control			x

Notes: Table reports estimates for the average change in (log) TFP for regulated firms in all industries relative to unregulated firms. The model in Column 1 includes firm and year fixed effects. In Column 2, we add industry-year fixed effects and prefecture-by-year linear trends. In Column 3, we add the WTO-by-TCZ treatment control. Standard errors are clustered at the firm level. Asterisks denote *p <0.10, **p <0.05, ***p <0.01.

Table C.4: Addressing Potential SUTVA Concerns (Spillovers to Unregulated Firms)

Outcome Variable:	% Workers High-Tech (1)	% Firms High-Tech (2)	% Workers High-Tech (3)	% Firms High-Tech (4)
TCZ * Post-Policy	0.000	0.005	-0.006	-0.001
	(0.008)	(0.004)	(0.007)	(0.004)
Observations	3,089	3,089	3,089	3,089
Mean Dep. Var.	0.313	0.284	0.313	0.284
Prefecture FEs	x	x	x	x
Year FEs	x	x	x	x
Firm Count Control			x	x

Notes: Table reports results from estimating the effects of the TCZ regulation on the percentage of workers (Columns 1 and 3) and percentage of firms (Columns 2 and 4) within prefectures that are "high-tech" as defined in the main text when conducting robustness checks. Data are at the prefecture-year level and do not include pilot years (i.e., the sample is from 1998 through 2006). Standard errors clustered by prefecture. Asterisks denote *p <0.10, **p <0.05, ***p <0.01.

Table C.5: Robustness Checks Related to Data and Variable Construction

Estimation Sample: / *Outcome Variable (log):*	1998-2006 TFP (1)	Homog. Goods TFP (2)	Non-Homog. TFP (3)	Full LP TFP (4)	Full TFP (5)
TCZ * Post	0.045***	0.075***	0.044***	0.053***	0.048***
	(0.009)	(0.019)	(0.010)	(0.009)	(0.009)
TCZ * Post * Dirtier	-0.044***	-0.043*	-0.053***	-0.050***	-0.037***
	(0.012)	(0.024)	(0.015)	(0.013)	(0.012)
Observations	728,305	158,668	604,289	762,957	762,957

Notes: Table reports results from robustness checks examining whether the results are sensitive to measurement and data issues. In Column 1, we drop 1996-97 (i.e., when the firm-level data collection was still in a pilot stage). In Columns 2-4, we probe the limitations of using a revenue-based productivity measure. We estimate the effects separately for homogenous and non-homogenous goods markets (Columns 2-3), assuming that mark-ups are less common in homogenous goods markets. In Column 4, we use the approach of Levinsohn and Petrin (2003) to construct TFP rather than Ackerberg et al. (2015). In Column 5, we use the median of industry-level pollution intensity to define less and more pollution-intensive industries rather than the one percent rule employed throughout most of the analyses. All regressions include firm fixed effects, industry-year fixed effects, and prefecture-year trends as well as the WTO-by-TCZ treatment control. Standard errors are clustered at the firm level. Asterisks denote *p <0.10, **p <0.05, ***p <0.01.

Table C.6: Robustness Checks Related to WTO Entry and Growth Trends

Outcome Variable (log):	TFP (1)	TFP (2)	TFP (3)	TFP (4)	TFP (5)	TFP (6)
TCZ * Post	0.042***	0.048***	0.047***	0.050***	0.040***	0.051***
	(0.009)	(0.011)	(0.011)	(0.011)	(0.009)	(0.009)
TCZ * Post * Dirtier	-0.025**	-0.042***	-0.042***	-0.042***	-0.043***	-0.049***
	(0.012)	(0.015)	(0.015)	(0.015)	(0.012)	(0.013)
Observations	762,957	666,323	666,323	666,323	728,304	755,569

Notes: Table reports results from additional robustness checks that probe whether our estimates are contaminated by broader macroeconomic trends and factors related to China's entry into the WTO. In Column 1, we include an additional control to account for the possibility that more and less pollution-intensive industries in TCZ prefectures may have been affected by the WTO entry differently. In Column 2, we control for prefecture-level (log) population and GDP per capita. In Column 3, we interact the prefecture level controls with an indicator equal to 1 for years following WTO entry and zero otherwise. In Column 4, we interact the prefecture level controls with the WTO-by-TCZ treatment indicator. In Columns 5 and 6, we control for firm exports and the proportion of capital accounted for by foreign sources, respectively. All regressions include firm fixed effects, industry-year fixed effects, and prefecture-year trends as well as the WTO-by-TCZ treatment control. Standard errors are clustered at the firm level. Asterisks denote *p <0.10, **p <0.05, ***p <0.01.

Table C.7: No Differences in TFP Gains for High- versus Low-Tech Industries

Outcome Variable (log):	TFP (1)	VA/L (2)
TCZ * Post	0.059***	0.060***
	(0.013)	(0.013)
TCZ * Post * Dirtier	-0.056***	-0.047***
	(0.017)	(0.018)
TCZ * Post * High-Tech	0.005	-0.017
	(0.021)	(0.021)
TCZ * Post * Dirtier * High-Tech	-0.006	-0.003
	(0.034)	(0.035)
Observations	619,044	619,044

Notes: Table reports results from estimating whether changes in productivity vary for firms in high- versus low-tech industries, which support the discussion in Section 5 of how migration of high-skilled workers likely does not drive our main results. We define high-tech industries as those that are particularly technology-intensive or human capital-intensive. The dependent variable is (log) TFP in Column 1 and labor productivity (value-added over number of workers) in Column 2. All regressions include firm fixed effects, industry-year fixed effects, and prefecture-year trends as well as the WTO-by-TCZ treatment control. Standard errors are clustered at the firm level. Asterisks denote *p <0.10, **p <0.05, ***p <0.01.

Table C.8: Changes in Productivity for Firms Starting in the Private Sector versus SOEs that Privatized in the Post-Regulation Period

Outcome Variable (log):	TFP (1)	TFP (2)	TFP (3)
TCZ * Post	0.063***	0.041*	0.042*
	(0.014)	(0.023)	(0.024)
TCZ * Post * Dirtier	-0.046**	-0.076**	-0.081**
	(0.019)	(0.031)	(0.033)
Observations	412,413	107,576	98,144
Mean Dep. Var.	5.904	6.040	6.041
Ownership	Private Pre-Regulation	Privatized (25%)	Privatized (50%)

Notes: Table reports results comparing the changes in productivity for firms that were already private sector in the pre-regulation period (Column 1) versus those that were SOEs in the pre-regulation period and then "privatized" to some degree in the post-regulation period (Columns 2-3). We define privatized as having at least 25% or 50% non-state ownership in Columns 2 and 3, respectively. The results support the discussion in Section 5 of how the heterogeneity across ownership types that we find likely is driven by differences in incentives and constraints across ownership rather than benefits of privatization over time. All regressions include firm fixed effects, industry-year fixed effects, and prefecture-year trends as well as the WTO-by-TCZ treatment control. Standard errors are clustered at the firm level. Asterisks denote *p <0.10, **p <0.05, ***p <0.01.