Evaluating Air Pollution Regulation: Separating Firm Competitiveness and Ambient Effects*

V. Brian Viard
Cheung Kong Graduate School of Business
brianviard@ckgsb.edu.cn

Guanglai Zhang
Jiangxi University of Finance and Economics
zhangguanglai@jxufe.edu.cn

Ning Zhang
Cambridge University
nz293@cam.ac.uk

Peng Zhang
The Chinese University of Hong Kong, Shenzhen
zhangpeng@cuhk.edu.cn

This version: 12/9/2025

Abstract

Measuring environmental regulation's effect on firm competitiveness is central to designing optimal policies. Existing studies find that air pollution regulations reduce total factor productivity (TFP). A parallel literature finds that air pollution lowers TFP through its ambient effect on workers' physical and mental health. Because proximate unregulated firms also enjoy this ambient effect, extant difference-in-differences estimation approaches mis-estimate the policy effect. We develop a difference-in-difference-in-distances approach to address this. Unregulated firms adjacent to regulated areas enjoy the ambient effect via spillovers while those far away do not. Comparing regulated firms to the former estimates the competitiveness effect and to the latter the ambient plus competitiveness effects. Applying this approach to a major air pollution regulation in China, the extant approach yields a 4.3% TFP decline and annual policy cost of CNY 226.5 billion. The true competitiveness effect is 5.8% and ambient effect 2.2%, yielding a true policy cost of CNY 179.8 billion.

JEL Codes: Q52; Q51; Q53; L51
Key words: environmental regulation, environmental costs and benefits, firm competitiveness, productivity

* We thank Guojun He, Kelsey Jack, Irene Jacqz, and Joseph Shapiro and seminar attendees at Hong Kong University of Science and Technology and The Chinese University of Hong Kong for helpful comments.

1. Introduction

Theoretically, binding environmental regulations can raise or lower firms' costs. Regulations impose compliance costs on firms, such as pollution abatement equipment and compliance personnel. On the other hand, regulations may increase productivity if it leads firms to rationalize their production processes or spurs innovations that lower costs or improve quality (Porter, 1991).[1] The direction and magnitude of the competitiveness effect is important for several reasons. Most directly, it is an important input in the cost-benefit analysis of environmental policies. Whether regulations raise or lower costs directly affects these calculations. Second, if environmental regulations affect firms' costs then they affect a country's trade position and balance of payments vis-a-vis other countries. Third, from a political economy perspective, the answer to the question determines whether, and how strongly, firms will resist or encourage the enactment of environmental regulations.

Given the theoretical uncertainty about the direction of the competitiveness effect, empirical estimates are critical. Greenstone *et al.* (2012) estimate the effects of the 1970 US Clean Air Act Amendments (CAAA) on manufacturing productivity using a large plant-level data set from 1972 to 1993 and employing a difference-in-differences (DD) approach.[2] The Act imposed pollution standards across multiple pollutants on plants not in compliance. Comparing non-attainment with attainment plants, the paper finds a 2.6% decline in total factor productivity (TFP) among surviving plants that were in non-attainment due to any pollutant.

A parallel literature (Graff Zivin and Neidell, 2012; Chang *et al.*, 2019; He *et al.*, 2019; Fu *et al.*, 2021) estimates how air pollution reduces productivity due to effects on the physical and mental stamina of workers. This implies that regulations that reduce air pollution will yield productivity improvements. Because air pollution drifts spatially, these productivity improvements accrue not to a specific firm but rather to all firms in the proximate area regardless of whether they must comply with the regulations. We call this the "ambient effect". The standard DD approach for evaluating regulations compares regulated firms which experience both effects to unregulated firms some of which, but not all, experience the ambient effects. Since some unregulated firms are contaminated by the ambient effect, the DD approach estimates neither the competitiveness effect nor the net (competitiveness plus ambient) policy effect. This is a violation of the stable unit treatment value

[1] The original evidence for this "Porter Hypothesis" was case-study based (Porter and van der Linde, 1995). Later formal justifications derive from regulations addressing X-inefficiency (Leibenstein, 1966), strategic trade models (Simpson and Bradford, 1996), and regulation addressing principal-agent inefficiencies between owners and management (Ambec and Barla, 2002).

[2] Berman and Bui (2001) and Gowrisankaran *et al.* (2020) estimate effects in particular industries. Jaffe and Palmer (1997) examine regulatory effects on innovation.

assumption (Angrist *et al.*, 1996): firms in the control group should be unaffected by the treatment. However, in this case, control firms are indirectly "treated" through the ambient spillover, biasing the conventional DD estimator.

Socially-optimal air pollution regulations require separating the two effects so as to obtain an accurate cost-benefit analysis. Consider an environmental regulation aimed at reducing manufacturing emissions and suppose it imposes a net regulatory cost on regulated firms. The regulation imposes a competitiveness effect, which is a private cost, and an ambient effect, which is a public benefit. Any associated air pollution reductions will convey the benefit of the ambient effect to all firms in and near the targeted areas. However, the cost of achieving the reduction (the competitiveness effect) is borne only by the firms that must comply with the regulation. To determine the optimal level of regulation, the competitiveness effect should be included as a cost (but applied only to regulated firms) and the ambient effect should be included as a benefit (applied to regulated and proximately-located unregulated firms).

To illustrate, consider a simple numerical example. A regulation imposes a competitiveness effect of -6% and an ambient effect of 2%, and pollution drifts such that not only the regulated but also one-fourth of unregulated firms (located near regulated firms) enjoy the ambient effect. A DD estimate would yield a policy effect of -3.5%.[3] This equals neither the competitiveness effect nor the net policy effect (-4%). The bias of the DD estimates depends on the mix of regulated and unregulated firms, and their geographic locations. We illustrate this in our setting and find that policy effects based on the decomposition of competitiveness and ambient effects is quite different than that based on naïve DD estimates.

Isolating the two effects also has relevance for the theoretical debate concerning the Porter Hypothesis. For example, Greenstone *et al.* (2012) estimate a small (1.7 to 2.2%) productivity increase for firms in non-attainment for carbon monoxide in response to the CAAA. This could be consistent with the Porter Hypothesis, but if the average ambient effect embedded in the estimate exceeds 2.2% this would be inconsistent.

From a policy perspective, firms' spatial distribution plays a crucial role in determining both the social efficiency and political feasibility of environmental regulation. Regions with higher firm densities generate greater ambient spillovers, allowing more firms to benefit from cleaner air. As a result, the average regulatory burden per firm is lower, and the implementation of air-pollution controls becomes more cost-effective and politically sustainable.

---

[3] In the DD comparison, one-fourth of the control firms would differ from the treatment firms by -4% (the competitiveness plus ambient effects) while three-fourths would differ by -6% (the competitiveness effect). The estimated effect would be ¼ x -4% + ¾ x -6% = -3.5%.

To disentangle the competitiveness and ambient effects we develop a difference-in-difference-in-distances (DD-Di) approach and apply it to a major air pollution regulation in China. We identify a "border" subsample of firms that are geographically close enough to each other that they face the same ambient pollution concentrations, some of which are subject to the regulation (the treatment group) and others of which are not (the control group).[4] We then compare the response of these two types of firms to the advent of the regulation (the treatment). The regulated and unregulated firms experience the same air pollution concentrations before and after the policy implementation but differ in their regulatory status after its advent. Comparing the productivity of these two types of firms through a DD-Di estimation yields the competitiveness effect.

To identify the ambient effect, we estimate the combined effect and then subtract the competitiveness effect from it. To identify the combined effect, we use a "non-border" sub-sample. These firms are geographically far enough from each other that the regulated firms experience a discontinuous drop in ambient pollution concentrations with the advent of the regulation, but the unregulated firms do not. For the non-border firms, the treatment and control groups experience the different ambient pollution levels, in addition to the difference in regulatory costs, with the regulation's advent. Comparing their productivity before versus after the start of the regulation yields the combined effect. The ambient effect is then obtained by subtracting the estimated competitiveness effect from the estimated combined effect. The typical DD approach does not distinguish border and non-border sub-samples and yields some combination of the two effects based on their mix and geographic placement.

To illustrate our approach, consider four firms in two cities A and B (Figure 1). City A is subject to a regulation while city B is not. A DD-Di estimate comparing firms 2 and 4 (non-border firms) quantifies the combined effect (competitiveness plus ambient effect). Firm 2 suffers from the competitiveness effect but also enjoys the ambient effect, while Firm 4 experiences neither given its far distance from the treatment city. A DD-Di estimate comparing firms 1 and 3 (border firms) isolates the competitiveness effect. Firm 3 enjoys the ambient effect because it is close to the treatment city but does not bear the competitiveness effect. Firm 1 benefits from the ambient effect but must also bear the competitiveness effect. The difference between the non-border and border DD-Di estimates equals the ambient effect.

[Insert Figure 1 here]

[4] We refer to the two groups of control-treatment firm pairs as "border" and non-border" for simplicity even though they are based on distances between firms rather than from a border.

We apply our approach to a regulation known as the "Plan of Key Cities Designation for Air Pollution Control" (KCAPC) which imposed air pollution controls on selected cities.[5] We confirm through a DD estimate that ambient pollution concentrations are not significantly different before versus after the policy for treatment-control firm pairs that are within five kilometers of each other. This group of pairs comprises the border sub-sample. For pairs that are more than five kilometers from each other, ambient pollution concentrations experience a statistically significant fall for treatment firms relative to control firms with the policy start. All such pairs comprise the non-border sub-sample. We apply our DD-Di approach to these two groups to estimate the policy's competitiveness and ambient effects on TFP in China's manufacturing sector. The DD-Di estimation yields a competitiveness effect of -5.8% and a combined effect of -3.6%, implying an ambient effect of 2.2%. Applying the competitiveness effect to all regulated firms and the ambient effect to all regulated firms plus unregulated firms within five kilometers of a regulated region, yields annual policy costs of CNY 179.8 billion. The naïve DD approach estimates a productivity decline of -4.3% due to the policy. Applying this to all firms implies higher annual policy costs of CNY 226.5 billion.

We examine the robustness of these estimates to several possible mitigating factors. Distinguishing exiting firms in the sample reveals that the competitiveness effects are greater for them (-8.0%), consistent with their being greater regulatory costs that induce their exit. The competitiveness effects for surviving firms are slightly lower than the baseline estimates (-5.1%). We provide evidence that the competitiveness effects are not amplified by agglomeration effects but the ambient effects are compounded by the treatment-firm density in a local area. We must use a deflated revenue measure rather than physical output; and therefore price changes in response to the policy would confound the estimates. We apply the De Loecker and Warzynski (2012) production function approach to estimate the relationship between the policy and markups in the sample. Their approach is convenient because the correlation between markups and firm-level characteristics (the policy variable in our setting) is identified even if markup levels are not. We do not find a significant effect of the policy on markups although the error bands are somewhat large.

This paper is most closely related to the literature estimating the effects of air quality regulations on competitiveness, in particular Greenstone *et al.* (2012). It differs in that the focus is on developing a method to isolate the competitiveness and ambient effects. As Greenstone *et al.* (2012) note, there is little other empirical evidence concerning the competitiveness effect of air pollution regulations on productivity except for specific industries (Gollop and Roberts, 1983; Ryan, 2012). A subsequent exception is He *et al.* (2020) for water pollution in China. It employs a regression

[5] In Chinese, the regulation is named "大气污染防治重点城市划定方案."

discontinuity (RD) approach comparing the productivity of firms immediately upstream of a water quality monitoring station to those immediately downstream in response to an increase in regulatory stringency in 2003. Upstream firms are affected by the regulation while downstream firms are not, because upstream pollution is measurable while downstream is not. The paper finds a 24% reduction in TFP for firms subject to monitoring versus those not.[6] There are two key differences between this paper and ours. First, it is unclear whether there are significant productivity effects of cleaner water (the equivalent of the ambient effect). To the extent that polluted water needs to be purified before it can be used as a productive input, this "ambient" effect would be a byproduct of the regulation.[7] Second, but related, the purpose of He *et al.* (2020) is not to isolate the competitiveness and ambient effects.

Besides this, our paper relates to two other areas of literature. First, we link the literature on competitiveness effects with that quantifying the direct effects of air pollution on productivity – the ambient effect. This area of literature began by focusing on specific occupations or industries (Graff Zivin and Neidell, 2012; Chang *et al.*, 2016; Adhvaryu *et al.*, 2019; Chang *et al.*, 2019; He *et al.*, 2019) and then expanded to estimate nationwide or supra-national effects (Dechezleprêtre *et al.*, 2018; Fu *et al.*, 2021). These papers motivate the need to develop a method to separately identify the competitiveness and ambient effects. In particular, Fu *et al.* (2021) shows that pollution has significant effects on TFP nationwide in China's manufacturing sector, emphasizing the need to account for an ambient effect in evaluating China's environmental regulations.

Second, there is a large literature that attempts to explain productivity dispersion among firms (Bartelsman and Doms (2000) and Syverson (2011) provide surveys). Environmental regulation is a contributing factor. However, quantifying this based on naïve DD estimates masks variation because there are two underlying contributions that are being averaged (and not necessarily in the correct proportion). The competitiveness effect applies to firms subject to a regulation while the ambient effect is experienced by other firms depending on their proximity in or near regulated regions. Naïve DD estimates are unhelpful for this because it does not give unbiased estimates of even the average policy effect on regulated firms.

The remainder of the paper proceeds as follows. The next section describes a conceptual framework for our analysis. Section 3 describes the institutional background and Section 4 our estimation approach. Section 5 describes the data to

[6] Wang *et al.* (2018) estimate effects for water pollution in a region of China.
[7] Firms immediately upstream of a monitoring station might or might not experience an "ambient" effect if one is present. It depends on whether the purified water is re-used in their production processes versus floating downstream. This differs from our setting where pollution affects all firms in close proximity.

which we apply the estimation approach. Section 6 discusses identification and presents the results. We conclude in Section 7.

2. Conceptual framework

The conceptual model closely follows that in Greenstone *et al.* (2012), which shows how environmental regulations affect firm productivity. We augment their model to separate the combined effect into the competitiveness and ambient effects. We assume a manufacturing firm (also plant)[8] i produces a product according to a constant-returns-to-scale Cobb-Douglas production function employing $\tilde{L}$ units of labor and K units of capital:

$$Q_i = A_i \tilde{L}_i^{\alpha} K_i^{1-\alpha}, \text{ (1)}$$

where Q is the firm's output, A is a Hicks-neutral technology shifter, and K is quantity of capital used in production. $\tilde{L}$ is production-effective labor – the quantity actually used in production.[9] Observed units of labor (L) may differ because regulation may require firms to employ ineffective inputs in the production process such as compliance officers. Effective are related to observed units by:

$$\tilde{L}_i = \lambda_L(r, \Omega) L_i, \text{ (2)}$$

where $\lambda_L \leq 1$ is a proportionality factor that reflects the regulatory effect on input usage. r denotes regulatory stringency and Ω the ambient pollution faced by the firm. The direct effect of the regulation on λ_L is the competitiveness effect which could be positive or negative: $\partial\lambda_L/\partial r <> 0$. At the same time, more stringent regulations may reduce pollution $\partial\Omega/\partial r \leq 0$ and generate an ambient effect. This may indirectly increase input effectiveness: $\partial\lambda_L/\partial\Omega \leq 0$. To determine the effects on productivity, substitute into the production function:

$$Q_i = A_i \lambda_L(r, \Omega)^{\alpha} L_i^{\alpha} K_i^{1-\alpha}. \text{ (3)}$$

The firm's TFP is output divided by weighted inputs:

$$TFP_i = \frac{Q_i}{L_i^{\alpha} K_i^{1-\alpha}} = A_i \lambda_L(r, \Omega)^{\alpha}. \text{ (4)}$$

Taking the derivative of logged TFP with respect to r gives the effect of regulation on TFP for a firm. These effects will depend on whether the firm is regulated or not and its geographic position. A regulated firm suffers from the competitiveness effect (first term) but also benefits from the ambient effect (second term):

---

[8] Only 5.2% of firms in our data set are multi-plant and we exclude them from estimation.

[9] For purposes of illustration we assume pollution does not affect capital because most of the evidence of pollution's effect on productivity is via labor. The model can easily be adapted to accommodate regulatory effects on capital.

$$\frac{\partial ln(TFP_i)}{\partial r} = \alpha \frac{\partial ln(\lambda_L)}{\partial r} + \alpha \frac{\partial ln(\lambda_L)}{\partial \Omega} \frac{\partial \Omega}{\partial \mathrm{r}}. \text{ (5a)}$$

An unregulated firm that is close enough to a regulated area enjoys the ambient effect but does not suffer from the competitiveness effect:

$$\frac{\partial ln(TFP_i)}{\partial r} = \alpha \frac{\partial ln(\lambda_L)}{\partial \Omega} \frac{\partial \Omega}{\partial \mathrm{r}}. \text{ (5b)}$$

Finally, an unregulated firm that is not close to a regulated area experiences neither effect:

$$\frac{\partial ln(TFP_i)}{\partial r} = 0. \text{ (5c)}$$

Let BOR be the number of unregulated firms that experience the ambient effect and FAR be the number that do not, a DD estimate using regulated firms as the treatment group and unregulated firms as the control group estimates the regulation's effect on TFP as:

$$\alpha \frac{\partial ln(\lambda_L)}{\partial r} + \alpha \frac{FAR}{BOR+FAR} \frac{\partial ln(\lambda_L)}{\partial \Omega} \frac{\partial \Omega}{\partial \mathrm{r}}. \text{ (6)}$$

This is neither the competitiveness effect nor the average policy effect. The bias is introduced because some control firms are contaminated by the ambient effect. An unbiased estimate of the competitiveness effect can only be obtained if all the control firms are on the border ($FAR = 0$). If all control firms are far from the border ($BOR = 0$), then the combined (competitiveness plus ambient) effect would be estimated. Our DD-Di approach separately compares the border and non-border control firms to the treated firms. The former isolates the competitiveness effect (the first term in Equation (5a)) by comparing regulated and unregulated firms subject to the same pollution concentrations (Ω). The latter estimates the combined effect (both terms in Equation (5a)) because both the regulatory stringency (r) and pollution concentration levels (Ω) change in response to the policy. Subtracting the competitiveness effect from this yields the ambient effect (the second term in Equation (5a)).

Since we are unable to measure quantities we must rely on revenue measures of output. If regulations affect marginal cost and firms have market power, our revenue-based measure of productivity may be confounded by changes in margins in response to marginal cost changes. Marginal cost, as derived in Appendix A by augmenting Greenstone *et al.* (2012), is:

$$MC_i = \frac{1}{A_i \lambda_L(r, \Omega)^{\alpha}} \phi w_i^{\alpha} r_i^{1-\alpha}, \text{ (7)}$$

where ϕ is a constant that depends on α. Marginal cost is decreasing in λ_L so that regulations requiring more compliance-related inputs (and therefore a greater gap between observed and effective inputs) will increase marginal cost. At the same time, marginal cost is increasing in Ω so that pollution reductions due to the regulation will decrease marginal cost. In the absence of market power, such changes in marginal cost will not bias the estimates as revenue-based productivity will scale one-for-one with quantity-based productivity. In the presence of market power, margins could either increase or decrease as marginal cost changes. If this is the case, then estimates using revenue-based productivity will not reflect effects on quantity-based productivity. We check whether the KCAPC policy affects margins when we present our results.

3. Institutional background

On September 5, 1987, the State Environmental Protection Administration (SEPA) issued the "Air Pollution Prevention and Control Law of the People's Republic of China". The policy, implemented on January 1, 1988, specified air pollution reductions for 47 "key" cities. The law was regarded as being of limited effectiveness because it specified no formal pollution targets or monitoring mechanism.[10] As a consequence, it was revised in 1995 and again in 2000. We focus on this last revision issued on April 29, 2000.

On December 2, 2002 as a part of implementing this last revision, SEPA formally issued the KCAPC policy. It identified 113 cities that were subject to regulations with the goal of meeting air quality targets by 2005.[11] The target was China's Class II air quality standard (formally designated GB3095-2000) with respect to six air pollutants: sulfur dioxide (SO_2), nitrogen dioxide (NO_2), total suspended particulate (TSP), ozone (O_3), carbon monoxide (CO), and particulate matter smaller than 10 micrometers in diameter (PM_{10}).[12] The standard specified maximum average annual, daily and hourly concentrations of these pollutants as shown in Appendix B.

The 113 cities subject to regulation under KCAPC were among the 338 cities with air pollution monitoring stations in 2000. They were chosen based on the city not meeting the GB3095-2000 standard in 2000 along with other criteria, such as whether the city was a national key-tourism or culturally-protected city, and its demographic and economic conditions. These are the treatment cities and all other cities (numbering 225) are control cities. The cities are defined by the four-digit level of the

[10] See http://www.gov.cn/gongbao/content/2000/content_60224.htm (in Chinese).
[11] A detailed description is at http://www.mee.gov.cn/gkml/zj/wj/200910/t20091022_172141.htm (in Chinese).
[12] The ambient air quality standard GB3095-2000 has three classes. Class II applies to residential, commercial, and traffic activities located in general industrial and rural areas. Class I is the strictest and applies to scenic areas and nature preserves. Class III is the least restrictive and applies to specialized industrial areas.

Administrative Division Codes of the PRC.[13] Appendix C shows the locations of the treatment and control cities.

The KCAPC policy did not go into effect until January 6, 2003 when SEPA issued its formal implementation.[14] We therefore take 2003 as the policy implementation threshold for our analysis. After the policy went into effect a city continued to be subject to regulation or not for the duration of our sample period.[15] The treatment cities were subject to oversight and restrictions while the control cities were not. The restrictions included promoting clean-energy use, barring high-polluting fuels, developing co-generation and central heating, controlling coal pollution, restricting motor-vehicle emissions, controlling construction and transportation dust, shutting down high-polluting plants, and requiring firms to establish environmental management systems.

SEPA supervised implementation at the national level. The policy targets were incorporated into the evaluation and promotion of government officials at the local level and treatment cities were subject to frequent inspections. Both the national and local governments had enforcement powers to ensure compliance. Local city officials were required to regularly release information on the concentrations of each of the pollutants and their performance influenced promotions and demotions. The KCAPC policy achieved significant emissions reductions. By 2005, 48 of the treatment cities had met the Class II standard. We also confirm that the policy significantly reduced pollution when we present our results.

4. Estimation approach

4.1 Overall approach

We first describe the econometric model corresponding to the naïve DD approach to compare to the previous literature. We then describe our DD-Di approach for isolating the competitiveness and ambient effects.

[13] The six-digit administrative code is published by the NBS' Administrative Division: http://www.stats.gov.cn/tjsj/tjbz/xzqhdm/201401/t20140116_501070.html (in Chinese). The first two digits identify one of the 31 provinces and the third and fourth digits the prefecture or major city.

[14] This is called "Notice on the Work of Air Pollution Prevention and Control in Key Cities to Meet the Deadline." A detailed description is at http://www.mee.gov.cn/gkml/zj/bgt/200910/t20091022_173815.htm (in Chinese).

[15] The treatment cities' performance was formally evaluated in 2005. In 2005, the KCAPC's goals switched to a different standard (based on emissions rather than concentrations). The treatment cities, regardless of whether they had met the Class II standard by 2005 or not, continued to be subject to controls though the end of the sample period while the control cities were not.

4.2 Naïve DD estimation

A typical approach to estimate the effects of air pollution regulations on productivity is a DD approach with regulated firms as the treatment group and unregulated as the control group. We include all firms in the sample that have data in at least one year before the policy and at least one year after. We do so because firms that appear only before or after do not contribute to identifying the policy effects (firm fixed effects absorb them) and we want the summary statistics to reflect only data that aids in identification. We estimate:

$$log(Prod_{it}) = \beta^D Post2003_t * KCAPC_{ct} + \eta_i + \theta X_{it} + \varepsilon_{it}, \quad (8)$$

where i indicates firm, t indicates year, and c indicates city. $Prod_{it}$ is firm i's productivity in year t. The firm fixed effects (η_i) capture time-persistent firm characteristics that affect productivity so that the DD effect is identified from inter-temporal variation within firms.[16] X_{it} includes province-by-year and industry-by-year fixed effects and in some specifications weather controls. The province-by-year fixed effects control for province-specific unobservables within a year and the industry-by-year fixed effects control for industry-specific unobservables within a year that affect productivity. ε_{it} captures firm-year specific shocks to productivity. We cluster standard errors by province-year to allow for correlation across firms in a province within a year.

The key variables are the two indicators. $Post2003_t$ equals zero prior to the imposition of the KCAPC and one after. It captures the pre- versus post-policy periods. $KCAPC_{ct}$ equals one if the city in which firm i is located is regulated under KCAPC and zero otherwise. β^D captures the naïve DD effect of the KCAPC policy on productivity: the differential effect of the policy on firms subject to its provisions versus those not, regardless of their geographic placement vis-à-vis the ambient effect.

4.3 DD-Di estimation

To isolate the competitiveness and ambient effects, we distinguish border and non-border subsamples and estimate the differential effect of the policy on control versus treatment firms within each subsample. This estimation exploits the spatial discontinuities in regulations between treatment and control cities to causally estimate the competitiveness and combined effects. In the border sub-sample, the firms are in sufficiently close proximity that the two types of firms are exposed to the same ambient pollution concentrations, but only those in regulated areas must incur

[16] Since firms rarely change cities (only 0.7% of observations) and rarely change industries (only 1.1% using the 4-digit industry code) over the sample period, we do not include city or industry fixed effects as they are nearly collinear with the firm fixed effects.

costs of complying with the KCAPC. In the non-border sample, the two types of firms are sufficiently far apart that the regulated firms incur the costs of complying with the KCAPC but also benefit from the reduction in pollution it causes; while the unregulated firms experience neither.

To identify the border and non-border sub-samples to be used in the DD-Di estimation, we estimate the following regression with $PM_{2.5}$ concentration in grid g in year t $\left(PM25_{gt}\right)$ as the dependent variable. $PM_{2.5}$ concentrations is convenient for this because it is available nationwide at a high degree of geographic specificity:[17]

$$log\left(PM25_{gt}\right) = \beta_1\left(Post2003_t * Band_g^d\right) + \beta_2\left(Post2003_t * Band_g^d * KCAPC_{gt}\right) + \eta_g + \rho_t + \varepsilon_{gt}. \quad (9)$$

$KCAPC_{gt}$ equals one if the policy applied to grid g in year t and zero otherwise. We determine a grid's treatment status based on the treatment status of the firms located in it, which are matched using firm latitude and longitude. η_g is a grid fixed effect which captures any time-persistent, grid-specific factors affecting concentrations; ρ_t is a year fixed effect which captures any factors affecting concentrations common to all grids in a year; and ε_{gt} captures unobserved grid-year factors affecting concentrations. $Band_g^d$ is set to one if the grid is more than $d - 1$ but less than d kilometers from the nearest border between control and treatment regions. The grids are one square kilometer. A grid may span the boundary between a treatment and a control region; however, this occurs in only forty of the 280,699 grid-year observations. We therefore set $KCAPC_{gt}$ to the median value of $KCAPC_{it}$ across all firms i in grid g.[18]

We estimate the equation beginning with $d = 1$ kilometer and increase it by one kilometer until we find a threshold distance $\left(\bar{d}\right)$, below which concentrations are the same in control and treatment regions ($\beta_2 = 0$) and above which concentrations in treatment regions are below those in control regions ($\beta_2 < 0$).[19] The graph in Figure 2 shows β_2 and 95% confidence intervals from estimating Equation (9) for d ranging from one to twenty kilometers. The threshold appears to be $\bar{d} = 5$. At or below this, the coefficients are small and not significantly different from zero (except at one kilometer). Above this, the coefficients are significantly different from zero and average -2.7%. That is, when treatment and control firms are within five kilometers of a border, they exhibit similar changes in $PM_{2.5}$ concentrations in response to the policy. Above this cutoff, the treatment firms experience a decline of about three

[17] The SO_2 data cannot be used for this since it is emissions rather than concentrations. Although the KCAPC policy covered multiple pollutants, concentrations of different air pollutants are highly correlated (Arceo *et al.*, 2016).
[18] We estimated using the mean value rather than median and the results are indistinguishable.
[19] Appendix D verifies the parallel trends assumption for this estimation.

percent in $PM_{2.5}$ emissions relative to control firms with the policy onset. We therefore define the border sub-sample as pairs of treatment/control firms within five kilometers of each other and the non-border sub-sample as those more than five kilometers from each other.

[Insert Figure 2 here]

For the DD-Di approach, we estimate the following with BOR_i denoting that firm i is in the border sub-sample and FAR_i indicating that firm i is in the non-border sample:[20]

$$log(Prod_{it}) = \gamma(Post2003_t * FAR_i) + \beta^B(Post2003_t * KCAPC_{ct} * BOR_i) + \beta^F(Post2003_t * KCAPC_{ct} * FAR_i) + \rho_i + \delta X_{it} + \epsilon_{it}. \quad (10)$$

β^B captures the competitiveness effect of the KCAPC policy on productivity: the differential effect of the policy on firms subject to it versus not but facing the same ambient pollution reduction due to the policy. β^F captures the combined effect: the differential effect of the policy on firms subject to it but enjoying the ambient effect versus firms that experience neither. γ captures the baseline change in productivity for non-border relative to border firms with the policy change.[21] ρ_i are firm fixed effects that capture time-persistent differences in firm productivity. X_{it} includes the same controls as in the naïve DD estimation. ϵ_{it} captures firm-year specific shocks to productivity. We cluster standard errors by province-year to allow for spatial correlation across firms within a province-year as provincial governments have policies that likely affect productivity for all firms within a province. We also implement the non-parametric, randomization inference test specified in Bertrand *et al.* (2004). This test makes no assumptions about the structure of the error term and is valid even in finite samples.

We found that treatment and control firms within five kilometers of a border between a treatment and a control region experience the same ambient pollution levels while those further away do not. We therefore define the cutoff distance for defining the border firms as five kilometers. To increase the power of our estimates, we include firms in the border sub-sample only if they have another firm of the

---

[20] This is a standard difference-in-difference-in-differences estimation. Equation (10) is equivalent to: $log(Prod_{it}) = \beta^F(Post2003_t * KCAPC_{ct}) + (\beta^B - \beta^F)(Post2003_t * KCAPC_{ct} * BOR_i) - \gamma(Post2003_t * BOR_i) + \rho_i + \delta X_{it} + \epsilon_{it}$. We formulate Equation (10) so as to directly estimate the competitiveness effect, rather than having to back it out from other coefficients.

[21] All other interaction terms are collinear with included terms. $KCAPC_{ct}$, BOR_i, and FAR_i are absorbed by the firm fixed effects since firms do not move. $Post2003_t$ is absorbed by the year fixed effects contained in X_{it}. $KCAPC_{ct} * BOR_i$ and $KCAPC_{ct} * FAR_i$ are absorbed by the firm fixed effects since all firms are either in BOR_i or FAR_i, are either regulated or not, and do not move. $Post2003_t * BOR_i$ is a linear combination of the year fixed effects contained in X_{it} and $Post2003_t * FAR_i$. $Post2003_t * KCAPC_{ct}$ is a linear combination of $KCAPC_{ct} * Post2003_t * BOR_i$ and $KCAPC_{ct} * Post2003_t * FAR_i$.

opposite type (control versus treatment) within a maximum distance of five kilometers.[22] We do this rather than include all firms within a certain distance of the physical boundary between treatment and control areas. The latter would include many firms that do not have a corresponding firm of the opposite type in close enough proximity that they face similar ambient pollution levels. While including these firms would not bias the estimates it would add noise and reduce efficiency. We define the non-border firms analogously: pairs of firms for which the closest firm of the opposite type is more than five kilometers away.

To illustrate, suppose treatment firm (A) has a control firm (B) located four kilometers away. This pair would be included in the border sub-sample given our cutoff of five kilometers. However, firm A might be three kilometers from the border and firm B one kilometer. This is why we do not apply an RD approach. We must include both a treatment and a control firm for each pair; however, they are not necessarily equidistant from the border making it impossible to define a unique distance. An advantage of this approach is that it can be applied in settings in which regulations apply to some but not all firms within the same geographic jurisdiction even if there is no defined physical boundary.

5. Data

Our estimation combines data on firm characteristics, pollution, and weather in China from 1998 to 2007. The policy change occurs in 2003.

5.1 Firm productivity data

Firm-level output and characteristics data is from the Annual Survey of Industrial Firms (ASIF) collected by China's National Bureau of Statistics (NBS). The survey includes all state-owned enterprises (SOEs) regardless of size and all non-SOEs whose annual sales exceed CNY 5 million (USD 0.75 million)[23] and contains detailed information on firm location, accounting measures, and firm characteristics. The survey includes only manufacturing firms so our results do not apply to the power generation sector or services firms. The survey captures 90.7% of China's total manufacturing output in the later years (Brandt *et al.*, 2012). We use the algorithm in Brandt *et al.* (2012) to match firms over time to form an unbalanced panel. This matching process is careful and avoids interpreting name changes as different firms. The panel is unbalanced because firms enter and exit during the sample period and non-SOEs may drop below or rise above the CNY 5 million threshold. We also

[22] Since firms rarely move in the sample (fewer than 0.7% of observations), we base the pairs on the closest firm over all sample years.
[23] A 2022 exchange rate of 6.7 is used throughout the paper.

follow Brandt *et al.* (2012) in converting nominal into real values using industry-level price indices.

We drop observations with missing or unreliable data following the previous literature (Cai and Liu, 2009; Brandt *et al.*, 2012).[24] These represent 10.3% of observations and 7.9% of total manufacturing output. Also following the previous literature (Cai and Liu, 2009), we winsorize the top and bottom 0.5% of data based on each of the values of output, value added, employment, and capital because of the risk that these involve data entry or reporting errors. Each firm is classified in an industry using the Chinese Industry Classification (CIC) code.[25]

We use the six-digit administrative code of the firm to assign it to a city and, in turn, to the treatment or control group. We use the address provided in ASIF to determine the firm's latitude and longitude and use these to calculate the distance between firms when locating the nearest firm of the opposite type. For most firms, ASIF contains the street address; however, for 16.5% of firms, it contains only the county or district. We drop these from the sample since this is not specific enough to calculate a distance from the nearest firm of the opposite type. We drop multi-plant firms (5.2% of the data) because we are unable to allocate their productivity to a specific location.

We use three alternative methods to estimate TFP: the OP (Olley and Pakes, 1996), LP (Levinsohn and Petrin, 2003), and ACF (Ackerberg, *et al.*, 2015) methods. We abstract from intermediate inputs and use value added as the output measure. ASIF directly reports value added as the firm's total production (including sales and inventory) of all goods produced in the year valued at their market prices less the cost of all intermediate inputs employed in producing them. We face two issues that many other papers have in estimating productivity based on manufacturing surveys or censuses. First, if there is market power in either the primary or input market and the KCAPC policy affects marginal cost; our use of a revenue-based measure of output could be confounded. If prices do not reflect market power then monetary- are preferred over quantity-based measures as they reflect quality differences (Syverson, 2011). We provide evidence when we discuss our results that margins are not significantly affected by the KCAPC policy. Second, estimates for multi-product firms will be confounded if their product mix is affected by the KCAPC policy. Only 1.3% of firms report more than one product so we exclude these from the sample.

---

[24] We drop observations with missing or negative values for output, value added, employment, or capital; firms with fewer than eight employees as they may have unreliable accounting systems; and firms violating accounting identities such as the components of net assets exceeding total assets or current depreciation exceeding cumulative depreciation.

[25] We use the National Economic Industry Classification (GB/T4754-2002) defined by the National Bureau of Statistics. This is similar to the US Standard Industrial Classification (SIC) code.

Appendix E provides summary statistics for the estimation sample which includes 87,930 firms and 541,845 firm-year observations or 6.2 years of data per firm on average. This table includes only firms that aid in identification (present before and after the policy change). The three productivity measures reveal significant variation and are highly correlated with each other.[26] Appendix F compares characteristics of the treatment versus control firms for the border (defined by a five-kilometer maximum distance) and non-border sub-samples. All of the differences are statistically significantly given the large amount of data. The treatment firms are somewhat larger than the control firms; while the productivity of the two sub-samples is very similar. In our estimation, we include firm fixed effects to absorb any systematic differences across firms.

5.2 Pollution data

Although we are interested primarily in productivity effects, we use two different pollution measures to confirm that the KCAPC policy was effective, to check whether pollution is similar for treatment and control firms in the border sub-sample, and to check for manipulation of emissions sources vis-à-vis political boundaries. The first is firm-specific SO_2 emissions from the Annual Environmental Survey of Polluting Firms (AESPF) of China. The AESPF includes 85% of total emissions volume in each county. The second is $PM_{2.5}$ concentrations. $PM_{2.5}$ annual concentrations are derived from satellite-based Aerosol Optical Depth (AOD) retrieval techniques maintained by the National Aeronautics and Space Administration (NASA).[27] The concentrations are calculated following van Donkelaar *et al.* (2016) and van Donkelaar *et al.* (2018). This data has been used in other studies of China's air pollution (Freeman *et al.*, 2019; Greenstone *et al.*, 2021). The data are reported in 1- by 1-kilometer grids. For pollution faced by a firm we use the latitude and longitude of the firm's address to place it in a grid. For cities, we take the average of all grid pollution levels within its boundaries as its exposure.

5.3 Weather data

In some specifications we include data for weather because it has been found to affect firm productivity (Zhang *et al.*, 2018) and also affects pollution levels. We include this only as a robustness check because it will only confound our estimates if weather conditions are correlated with the policy implementation. We obtain daily, station-level weather variables from the National Meteorological Information Centre

---

[26] The Pearson correlation coefficients for the OP and LP measures is 0.79 and for ACF measure with respect to OP and LP measures is 0.81 and 0.94, all significant at better than the 0.1% level.
[27] The AOD data are obtained from the Global Annual PM2.5 Grids from MODIS, MISR and SeaWiFS Aerosol Optical Depth (AOD) with GWR, v1 (1998–2016) released by the Socioeconomic Data and Applications Center of NASA (https://beta.sedac.ciesin.columbia.edu/data/set/sdei-global-annual-gwr-pm2-5-modis-misr-seawifs-aod).

of China.[28] We aggregate the data to the city level using the inverse-distance weighting method (Deschênes and Greenstone, 2011) to give less weight to stations more distant from the geographic centroid. We then compute an annual average of temperature, relative humidity, wind speed, sunshine duration, and barometric pressure and a cumulative annual value for precipitation.

6. Results

We first obtain naïve DD estimates for comparison before proceeding with the DD-Di estimation of the competitiveness and ambient effects. We discuss identification for each as we proceed.

6.1 Naïve DD estimates

The KCAPC policy imposed regulatory measures only on selected regions. This provides the basis for control and treatment groups and allows firm-specific shocks to productivity to be separately identified from regulatory effects. The identifying assumption for the DD estimates is that the pre-existing trends for the control and treatment groups are parallel prior to the policy intervention. Figure 3 shows coefficients and 95% confidence intervals for event studies (substituting year-by-year coefficients β_t^D for $\beta^D Post2003_t$ with 2002 normalized to zero in Equation (8)) for the three different TFP measures. The coefficients (normalized to zero in 2002) show no significant differential trends prior to 2003 and display a downward trend beginning in 2003 that becomes significant in 2005 for all three measures. This time lag is similar to that found in Greenstone *et al.* (2012) which notes that it can take plants a couple years to implement abatement actions.

[Insert Figure 3 here]

Table 1 shows the DD estimates (β^D coefficient in Equation (8)) for the three productivity measures. All specifications include firm fixed effects, province-by-year fixed effects, and industry-by-year fixed effects at the four-digit level.[29] The results are very significant and fairly consistent across specifications: the KCAPC policy reduces TFP by 3.4% to 4.3%. We use the results for the ACF measure (4.3%) as our headline result since the ACF measure addresses some of the identification concerns with the OP and LP approaches.

[Insert Table 1 here]

[28] Available at http://data.cma.cn (in Chinese).
[29] There are 425 four-digit industry codes.

6.2 DD-Di identification

There are three separate identification conditions for the DD-Di estimation. We have already confirmed the first: that the border sub-sample includes treatment and control firms that are close enough to each other that they experience the same ambient pollution before and after the policy change; while the firms in the non-border sub-sample are far enough from each other that the treatment firms experience an ambient pollution decrease but control firms do not. Second, the pre-existing trends in productivity for the control and treatment groups are parallel prior to the policy for both the border and non-border sub-samples. Third, there are no confounding factors coincident with the KCAPC policy that affect pollution or productivity differentially on different sides of the treatment-control borders. We consider these two additional identification conditions before discussing the results.

Identification – parallel trends

To check for parallel trends for treatment and control firms in the pre-policy years, we allow for year-by-year coefficients for the border and non-border sub-samples in Equation (10) (i.e., substituting β_t^B for $\beta^B * Post2003_t$ and β_t^F for $\beta^F * Post2003_t$ with 2002 normalized to zero). Panel A of Figure 4 shows coefficients and 95% confidence intervals for the border firms using the ACF measure. The interaction terms show a slight, but insignificant, downward trend prior to 2001, then a leveling off before a more rapid downward trend beginning in 2003 that becomes significant in 2005. That is, the competitiveness effect becomes significant in 2005. This time lag is similar to that found in Greenstone *et al.* (2012) which notes that it can take plants a couple years to implement abatement actions. The pattern for the non-border firms (Panel B) is similar, with the combined effect becoming significant in 2005. Panel C shows the time trend for the difference in the coefficients ($\beta_t^B - \beta_t^F$) and confirms that the downward trend is more pronounced for the border than the non-border firms. The difference between the two is the ambient effect. Appendices G and H present corresponding results for the OP and LP measures. The patterns are similar.

[Insert Figure 4 here]

Identification – confounding factors

Other factors that coincide with the KCAPC policy and affect pollution or productivity differentially on different sides of the treatment-control borders, will bias the results. A major concern in this regard is empirical evidence that dirtier pollution sources are placed near political boundaries so as to “export” pollution to nearby jurisdictions. Most evidence concerns water pollution (Sigman, 2002; Sigman, 2005; Kahn *et al.*, 2015; Cai *et al.*, 2016; Lipscomb and Mobarak, 2017; He *et al.*, 2020) but there is also evidence for air pollution (Wang and Wang, 2021). If the incentive to do so changes with the implementation of the KCAPC, this would confound the estimates. To check for this, we test whether SO_2 emissions of firms near the outskirts of treatment regions respond differently to the KCAPC policy than those in the interior of a treatment region. The same comparison cannot be made within control regions because the ambient effect of the policy affects firms that are near the outskirts of control regions next to treatment regions but not those in the interior.

Appendix I shows the results from estimating Equation (8) with log SO_2 emissions as the dependent variable but further interacting the policy-treatment variable with an indicator set to one if the firm is within a certain distance of the outer edges of a region and zero otherwise.[30, 31] The KCAPC policy significantly reduces emissions by 15% to 18%. However, the policy has no differential effect on firms near the outskirts of a treatment region relative to those in the interior of treatment regions, consistent with no manipulation in response to the policy.

6.3 DD-Di estimates

Main results

Table 2 displays results of estimating Equation (10) for the three TFP measures with firm, province-by-year, and 4-digit industry code-by-year fixed effects and using a five-kilometer distance for the matched pairs of firms to define the border sub-sample, consistent with our identification analysis. The coefficient on the non-border, policy-treatment interaction yields the causal estimate of the KCAPC policy's combined effect; while the coefficient on the border, policy-treatment interaction yields the causal estimate of its competitiveness effect. The difference between the two yields the casual estimate of its ambient effect. The ACF and LP measures yield very similar estimates that are somewhat larger (in absolute value terms) than those for the OP measure. However, the patterns are similar across all three measures.

The combined and competitiveness effects are highly significant for all three measures and the ambient effect is significant at better than the 10% level. We base our headline estimates on the ACF measure: a combined effect of a 3.6% decline in TFP, a competitiveness effect of a 5.8% decline in TFP, and an ambient effect of a 2.2% increase in TFP annually. To check the robustness of our estimates to the arbitrary clustering of errors spatially and intertemporally, we implement the non-parametric test of Bertrand *et al.* (2004). We randomly assign 113 out of 338 cities as the treatment firms and estimate the baseline model (Equation (10)) using ordinary least squares, repeating the procedure 500 times. Figure 5 shows our baseline coefficient

---

[30] This differs from Equation (10) which compares how concentrations change for treatment firms relative to control firms both of which are near a border. This instead estimates how emissions change with the policy for treatment firms close to a border relative to treatment firms that are not.
[31] Appendix J shows the parallel trends tests for the SO_2 emissions measure.

estimates relative to the distributions of coefficients obtained from this test for the competitiveness and combined effects for the ACF measure (Appendix K shows the same for the LP and OP measures). The coefficient estimates lie well outside the test distributions for all of the measures.

[Insert Table 2 here]

[Insert Figure 5 here]

Vis-à-vis estimation error, we can relate these estimates to the naïve DD estimates adjusted for the fraction of unregulated firms that experience the ambient effect. There are FAR =132, 490 firm-year observations in non-border control areas and BOR =41,986 firm-year observations in border control areas. Applying Equation (6) to our estimates of the competitiveness and ambient effects yields -4.2%, which is close to the naïve DD estimate of -4.3%.

Price effects

Since our results use revenue-based productivity measures, they may not reflect changes in quantity-based measures if price-cost margins are affected by the regulation (see Appendix A). To provide some evidence of whether this is the case, we follow the production function approach specified in De Loecker and Warzynski (2012) to infer markups and then see whether they are correlated with the KCAPC policy. De Loecker and Warzynski (2012) show that even though absolute markups may not be identifiable, if one uses deflated revenues and input expenditures, as we do, then the correlation between markups and firm-level characteristics are still identified. In our case the firm-level characteristic is whether the KCAPC applied to a firm or not. We therefore estimate whether the policy is correlated with markups at the firm level.

Following the procedure in their paper, we first estimate the production function industry-by-industry at the three-digit level using a translog production function and the LP method with ACF correction. We include the policy variable in the control function for materials since the regulatory policy may affect unobserved productivity. After computing predicted markups, we estimate:

$$log(markup_{it}) = \alpha + \gamma_1 log(L_{it}) + \gamma_2 log(K_{it}) + \gamma_3 log(TFPK_{it}) + \gamma_4 Post2003_t + \gamma_5 KCAPC_{ct} + \gamma_6 Post2003_t * KCAPC_{ct} + \varepsilon_{it}. \quad (11)$$

γ_1, γ_2, and γ_3 control for the effects of labor, capital and TFP, respectively, on marginal cost. γ_4 controls for factors affecting markups that coincide with the timing of the KCAPC policy, and γ_5 controls for markup differences for regulated versus unregulated firms across the sample period. γ_6 is the key coefficient of interest and captures changes in average markups in response to the policy. Appendix L shows the estimates of the correlation of the policy with markups (γ_6) for all firms (Column (1)) and for the border sub-sample (Column (2)), clustering standard errors at the province-year level. The coefficients for both are close to zero and insignificant, although the confidence intervals are fairly large. This is suggestive evidence that markups are not affected by the policy.

Survival selection bias

Firms that experience large productivity declines may be more likely to exit. If so, our estimates may be biased upward toward zero. Because our data set also omits non-SOE firms below the CNY 5 million threshold, "exit" could also entail a non-SOE moving below this threshold. We perform two robustness checks to see whether our estimates might be affected by firm exit. We estimate a DD regression at the city-year level:

$$Y_{ct} = \gamma Post2003_t * KCAPC_{ct} + \rho_c + \delta_t + \epsilon_{it}, \quad (12)$$

where Y_{ct} is a measure of exit in city c and year t. The city fixed effects ρ_c capture time-persistent city-level factors that affect exit while year fixed effects δ_t capture year-specific unobservables that affect exit across all cities. ϵ_{it} captures city-year specific shocks to exit. We cluster standard errors at the province-year level to allow for correlation in unobservables within a province and year.

Column (1) of Appendix M shows estimates of Equation (12) with firms leaving the sample as a fraction of all firms in the city-year. The point estimate is close to zero and insignificant. Since SOEs are included in the sample regardless of size, focusing on them will isolate the effect of actual exit from threshold crossings. Column (2) estimates with SOE exits as a fraction of SOEs in the city-year as the dependent variable. The estimate is again close to zero and insignificant. Column (3) estimates using non-SOEs that leave the sample as a fraction of all non-SOEs. The estimate is small and insignificant. These results are consistent with the KCAPC policy not affecting firm survival.

The second robustness check re-estimates our baseline equation (Equation (10)) but adds a further interaction of the policy-treatment interactions with a dummy variable set to one if the firm either exited or fell below the CNY 5 million threshold between 2004 and 2007, and zero otherwise. Columns (1), (3), and (5) of Appendix N repeats the baseline results for each of the productivity measures while Columns (2), (4), and (6) present the extended model. The combined effect for firms that exit is not significantly different from the stand-alone effects, although the estimates are not very precise. The exiting firms suffer a greater competitiveness effect than surviving firms under the KCAPC policy. For the former, the ACF estimates imply a -9.0% competitiveness effect. This is consistent with firms that are more affected by the regulation being more likely to exit. The competitiveness effect for the surviving firms is slightly smaller in absolute value (-5.1%) than the baseline estimates. The combined effect for surviving firms remains roughly the same (-3.8%).

The KCAPC policy may also affect firm entry by reducing the expected profits of potential entrants. Because the data also omits non-SOEs below CNY 5 million in revenues, "entry" could include moving from below to above this threshold. To see if our estimates might be affected by either, Column (4) of Appendix M estimates Equation (12) with firms appearing as a fraction of all firms in the city-year as the dependent variable. The estimate is close to zero and insignificant consistent with the KCAPC policy not having an appreciable effect on firm entry or threshold crossings.[32]

Robustness

We re-estimated the baseline model (Equation (10)) weighting observations by firm value added in each year. The results, using the ACF measure, are shown in Column (2) of Appendix O. The results are very similar to the baseline results (Column (1)). Column (3) weights instead by firm employment in each year. The results are again similar to the baseline. Column (4) adds the weather control variables which produces very similar results to the baseline, consistent with the policy not being correlated with weather patterns.

Agglomeration effects

Productivity declines for treatment firms in response to the KCAPC may spill over to geographically-proximate treatment and control firms. This will not bias the DD-Di estimates but the estimates would incorporate both effects.[33] Policymakers may wish to distinguish the direct competitiveness effects from the ensuing spillovers. If the spillovers are confined within industries then the industry-by-year fixed effects will absorb them. However, Greenstone *et al.* (2010) find spillovers are driven by more general labor and technological linkages between firms. To test for agglomeration effects, we follow that paper's conceptual model and assume that the TFP of a firm is

---

[32] We cannot estimate the differential competitiveness and combined effects for entering firms (equivalent of Appendix N) because a firm must appear both before and after the policy to identify the treatment effect.

[33] The policy could be correlated with firm density (e.g., areas with higher firm density are more polluted and therefore targeted by the policy) or firm densities could change differentially on average for treatment versus control firms. Neither of these would bias the DD-Di estimates since the comparison is within a small geographic area in which treatment and control firms face the same firm density at any point in time. It could affect out-of-sample extrapolation of the policy effects since the DD-Di sample may naturally include higher density areas. This provides another reason for the robustness check.

affected by the number of proximate firms.[34] We estimate a difference-in-DD-Di (D-DD-Di) model that compares policy effects in border areas with high relative to low densities of control firms controlling for the D-DD-Di with respect to the density of treatment firms. Agglomeration spillovers resulting from the policy will affect control and treatment firms that are near each other similarly, but the number of treatment firms will also amplify the ambient effects. This is not an issue for the control firms since their emissions are not directly affected by the regulation. Therefore, the D-DD-Di with respect to the number of control firms (N_{Ci}) isolates the agglomeration effect if we simultaneously control for the D-DD-Di with respect to the number of treatment firms (N_{Ti}). Appendix P provides the conceptual model underlying this estimation.

We modify Equation (10) to add the D-DD-Di terms:

$$ln(Prod_{it}) = \lambda Post2003_t * FAR_i + Post2003_t[\gamma_B^T BOR_i log(N_{Ti}) + \gamma_B^C BOR_i log(N_{Ci}) + \gamma_F^T FAR_i log(N_{Ti})] + Post2003_t * KCAPC_{ct}[\beta_B^T BOR_i log(N_{Ti}) + \beta_B^C BOR_i log(N_{Ci}) + \beta_F^T FAR_i log(N_{Ti})] + \eta_i + \theta X_{it} + \varepsilon_{it}. \quad (13)$$

λ controls for differences in non-border relative to border firms after the policy onset. γ_B^T, γ_B^C, and γ_F^T control for any amplification of productivity changes after the policy relative to before due to agglomeration effects for border firms in response to surrounding control firms, for border firms in response to surrounding treatment firms, and for non-border firms in response to treatment firms respectively. β_B^T controls for the amplification of ambient effects in border areas in response to the policy due to the number of treatment firms. β_B^C is the main coefficient of interest and captures any agglomeration effect changes in response to the policy, which could be positive or negative. β_F^T controls for the amplification of ambient effects in non-border areas in response to the policy due to the number of treatment firms.[35] We use a five-kilometer radius to define proximate firms (N_{Ci} and N_{Ti}) to match the definition of border firms so that we can see how they interact. An interaction between the number of control firms and the FAR_i dummy cannot be identified because treatment firms in non-border areas have no proximate control firms. All border firms have a positive number of proximate control and treatment firms since we use matched pairs of firms. We exclude FAR_i firms with zero proximate treatment firms in this estimation. These firms do not aid in identification and

---

[34] We are unable to follow their empirical approach because we do not have the same input-output, labor sharing, or technology linkage data.

[35] Since we choose the closest firm of the opposite type over the whole sample period, an interaction of both firm density measures with the treatment dummy are absorbed by the firm fixed effects. All other interaction terms are collinear with included terms. $KCAPC_{ct} * BOR_i * log(N_{Ti})$, $KCAPC_{ct} * BOR_i * log(N_{Ci})$, and $KCAPC_{ct} * BOR_i * log(N_{Ti})$ are absorbed by the firm fixed effects since firms do not move and the number of surrounding firms measure does not change. $KCAPC_{ct} * Post2003_t * FAR_i * log(N_{Ti})$ is collinear with $Post2003_t * FAR_i$ and $Post2003_t * FAR_i * log(N_{Ti})$.

excluding them allows the use of log number of firms while avoiding the bias introduced in using transformations to overcome this (Chen and Roth, 2023).

Appendix Q shows the results using the three TFP measures. For all three measures, the treatment effect does not vary significantly with the number of proximate control firms consistent with no significant agglomeration effects in response to the policy. The treatment effect is decreasing in the number of proximate treatment firms for all three measures consistent with ambient density effects amplifying the competitiveness effect convexly (see Appendix P).

6.4 Illustrative policy evaluation

To illustrate the importance of distinguishing competitiveness and ambient effects, we conduct a back-of-the-envelope calculation of the KCAPC policy's net effect. We illustrate how the decomposition into competitiveness and ambient effects allows the average policy effects to be calculated. In doing so, we quantify the effects for the firms included in the sample and use our estimates inclusive of exiting firms.

There are three components: First, the cost of the competitiveness effect to the 367,369 regulated firms in the full sample. Applying the estimated competitiveness effect (-5.8%) and average value added per regulated firm (CNY 14.369 million), yields a cost of CNY 306.2 (USD 45.7) billion annually. Second, is the benefit of the ambient effect (2.2%) applied to these regulated firms: CNY 114.8 (USD 17.1) billion annually. The third component is the ambient effect on proximate unregulated firms, which have an average value added per firm of CNY 12.74 million. Applying the estimated ambient effect (2.2%) to the 41,986 control firms within five kilometers of a treatment city yields a benefit of CNY 11.6 (USD 1.7) billion annually. The three components imply a net policy cost of CNY 179.8 (USD 26.8) billion annually. This is quite different from naïvely applying the estimated combined effect (-4.3%) to the average value added of the regulated firms (CNY 226.5, USD 33.8 billion annually).

7. Conclusion

Choosing optimal environmental regulations requires an accurate cost-benefit analysis of their impact. This paper provides a method to isolate the net private costs to firms from complying with a regulation from the spillover benefits of improved productivity that accrue to all proximately-located firms regardless of whether they are regulated. Failing to separate these effects understates the private costs to regulated firms and the public benefits to other firms. Unbiased policy effects can only be obtained by isolating the competitiveness and ambient effects and then grossing up the effects given the number and placement of firms. Our results also imply that the net cost of environmental regulations is lower when applied in areas with high firm density.

While this paper has applied the approach to a geographically-targeted regulation, the approach works even in the absence of explicit physical boundaries. For example, it is applicable to a virtual boundary such as an industry-targeted regulation in which the private costs accrue to the industry but spillover benefits accrue to proximately-located firms in all industries or to a regulation targeting only specific firms but proximate firms benefit from the ambient spillover effects. For example, US EPA regulations often target specific firms. In setting pollution levels optimally, the private costs (the competitiveness effect) must be applied only to the targeted firms while the public spillover (ambient effect) should be applied to all proximate firms.

Our paper examines only manufacturing firms. A similar decomposition may be necessary for services firms. For example, regulating emissions from transportation and distribution industries would impose compliance costs on these firms but also benefit other firms in improved productivity from reduced pollution concentrations. With slight modification, the approach developed in the paper could be applied to water pollution to determine whether productivity spillovers are significant and whether these productivity benefits also accrue to the regulated firms.

References

Ackerberg, D. A., K. Caves, G. Frazer (2015). “Identification Properties of Recent Production Function Estimators,” *Econometrica*, 83, 2411 – 2451.

Adhvaryu, A., N. Kala, A. Nyshadham (2019). “Management and Shocks to Worker Productivity,” *Journal of Political Economy*, 130, 1 – 47.

Ambec, S., P. Barla (2002). “A Theoretical Foundation of the Porter Hypothesis,” *Economics Letters*, 75, 355 – 360.

Angrist, J. D., G. W. Imbens, D. B. Rubin (1996). “Identification of Causal Effects Using Instrumental Variables: Rejoinder”, *Journal of the American Statistical Association*, 91, 468 – 472.

Arceo, E., R. Hanna, P. Oliva (2016). “Does the Effect of Pollution on Infant Mortality Differ Between Developing and Developed Countries? Evidence from Mexico City,” *The Economic Journal*, 126, 257 – 280.

Bartelsman, E. J., M. Doms (2000). “Understanding Productivity: Lessons from Longitudinal Microdata,” *Journal of Economic Literature*, 38, 569 – 94.

Berman, E., L. T. M. Bui (2001). “Environmental Regulation and Productivity: Evidence from Oil Refineries,” *Review of Economics and Statistics*, 83, 498 - 510.

Bertrand, M., E. Duflo, S. Mullainathan (2004). “How Much Should We Trust Differences-in-Differences Estimates?”, *The Quarterly Journal of Economics*, 119, 249 – 275.

Brandt, L., J. Van Biesebroeck, Y. Zhang (2012). “Creative Accounting or Creative Destruction? Firm-Level Productivity Growth in Chinese Manufacturing,” *Journal of Development Economics*, 97, 339 – 351.

Cai, H., Y. Chen, Q. Gong (2016). “Polluting thy Neighbor: Unintended Consequences of China' s Pollution Reduction Mandates,” *Journal of Environmental Economics and Management*, 76, 86-104.

Cai, H., Q. Liu. (2009). “Competition and Corporate Tax Avoidance: Evidence from Chinese Industrial Firms," *The Economic Journal*, 119, 764 – 795.

Chang, T., J. G. Zivin, T. Gross, M. Neidell. (2016). “Particulate Pollution and the Productivity of Pear Packers,” *American Economic Journal: Economic Policy*, 8, 141 – 169.

Chang, T., J. G. Zivin, T. Gross, M. Neidell. (2019). “The Effect of Pollution on Worker Productivity: Evidence from Call-Center Workers in China,” *American Economic Journal: Applied Economics*, 11, 151 – 172.

Chen, J. J. Roth. (2023). “Logs with Zeros? Some Problems and Solutions. *The Quarterly Journal of Economics*, 139. 891 – 936.

De Loecker J., Warzynski F. (2012). “Markups and Firm-Level Export Status,” *American Economic Review*, 102, 2437 – 71.

Dechezleprêtre A., N. Rivers, B. Stadler (2018). “The Economic Cost of Air Pollution: Evidence from Europe,” SSRN working paper.

Deschênes, O., M. Greenstone (2011). “Climate Change, Mortality, and Adaptation: Evidence from Annual Fluctuations in Weather in the US,” *American Economic Journal: Applied Economics*, 3, 152 – 185.

Freeman, R., W. Liang, R. Song, C. Timmins (2019). “Willingness to Pay for Clean Air in China,” *Journal of Environmental Economics and Management*, 94, 188 – 216.

Fu, S., V. B. Viard, P. Zhang (2021). “Air Pollution and Manufacturing Firm Productivity: Nationwide Estimates for China,” *The Economic Journal*, 131, 3241 – 3273.

Gollop, F. M., M. J. Roberts (1983). “Environmental Regulations and Productivity Growth: The Case of Fossil-fueled Electric Power Generation, *Journal of Political Economy*, 91, 654 – 674.

Gowrisankaran, G., M. Greenstone, A. Hortaçsu, M. Liu, C. Shen, B. Zhang (2020). “Discharge Fees, Pollution Mitigation, and Productivity: Evidence from Chinese Power Plants,” working paper.

Graff Zivin, J., M. Neidell (2012). “The Impact of Pollution on Worker Productivity,” *American Economic Review*, 102, 3652 – 3673.

Greenstone, M., G. He, S. Li, E. Zou (2021). "China's War on Pollution: Evidence from the First 5 years," *Review of Environmental Economics and Policy*, 15, 281 – 299.

Greenstone, M., R. Hornbeck, E. Moretti (2010). "Identifying Agglomeration Spillovers: Evidence from Winners and Losers of Large Plant Openings," *Journal of Political Economy*, 118, 536 – 598.

Greenstone, M., J. A. List, C. Syverson (2012). "The Effects of Environmental Regulation on the Competitiveness of U.S. Manufacturing," NBER working paper 18392.

He, G., S. Wang, B. Zhang (2020). "Watering Down Environmental Regulation in China," *Quarterly Journal of Economics*, 135, 2135 – 2185.

He, J., H. Liu, A. Salvo. (2019). "Severe Air Pollution and Labor Productivity: Evidence from Industrial Towns in China," *American Economic Journal: Applied Economics*, 11, 173 – 201.

Jaffe, A. B., K. Palmer (1997). "Environmental Regulation and Innovation: A Panel Data Study," *The Review of Economics and Statistics*, 79, 610 – 619.

Kahn, M. E., P. Li, D. Zhao (2015). "Water Pollution Progress at Borders: The Role of Changes in China's Political Promotion Incentives," *American Economic Journal: Economic Policy*, 7, 223 – 242.

Leibenstein, H. (1966). "Allocative Efficiency vs. X-Efficiency," *American Economic Review*, 56, 392 – 415.

Levinsohn, J. and A. Petrin (2003). "Estimating Production Functions using Inputs to Control for Unobservables," *Review of Economic Studies*, 70, 317 – 342.

Lipscomb, M., A. M. Mobarak (2017). "Decentralization and Pollution Spillovers: Evidence from the Re-drawing of County Borders in Brazil," *The Review of Economic Studies*, 84, 464 – 502.

Olley, G. S. and A. Pakes (1996). "The Dynamics of Productivity in the Telecommunications Equipment Industry," *Econometrica*, 64, 1263 – 1297.

Porter, M. E. (1991). "America's Green Strategy," *Scientific American*, 264, 168.

Porter, M. E., C. van der Linde (1995). "Toward a New Concept of the Environment-Competitiveness Relationship," *Journal of Economic Perspectives*, 9, 97 – 118.

Ryan, S. P. (2012). "The Costs of Environmental Regulation in a Concentrated Industry," *Econometrica*, 80, 1019 – 1061.

Sigman, H. (2002). "International Spillovers and Water Quality in Rivers: Do Countries Free Ride?" *American Economic Review*, 92, 1152 – 1159.

Sigman, H. (2005). “Transboundary Spillovers and Decentralization of Environmental Policies,” *Journal of Environmental Economics and Management*, 50, 82 – 101.

Simpson, D., R. Bradford III (1996). “Taxing Variable Cost: Environmental Regulation as Industrial Policy,” *Journal of Environmental Economics and Management*, 30, 282 – 300.

Syverson, C. (2011). “What Determines Productivity,” *Journal of Economic Literature*, 49, 326 – 365.

van Donkelaar, A. *et al.* (2016). “Global Estimates of Fine Particulate Matter using a Combined Geophysical-Statistical Method with Information from Satellites, Models, and Monitors,” *Environmental Science & Technology*, 50, 3762 – 3772.

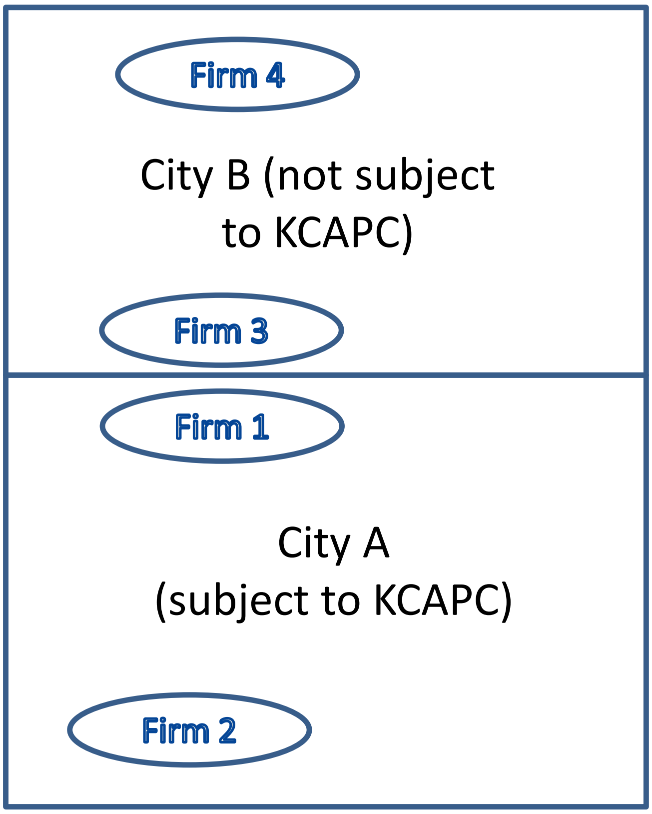
van Donkelaar, A., *et al.* (2018). “Documentation for the Global Annual PM2.5 Grids from MODIS, MISR and SeaWiFS Aerosol Optical Depth (AOD) with GWR, 1998-2016,” Palisades NY: NASA Socioeconomic Data and Applications Center.

Wang, C., J. Wu, B. Zhang (2018). “Environmental Regulation, Emissions and Productivity: Evidence from Chinese COD-Emitting Manufacturers,” *Journal of Environmental Economics and Management*, 92, 54 – 73.

Wang, S., Z. Wang (2021). “The Environmental and Economic Consequences of Internalizing Border Spillovers,” working paper.

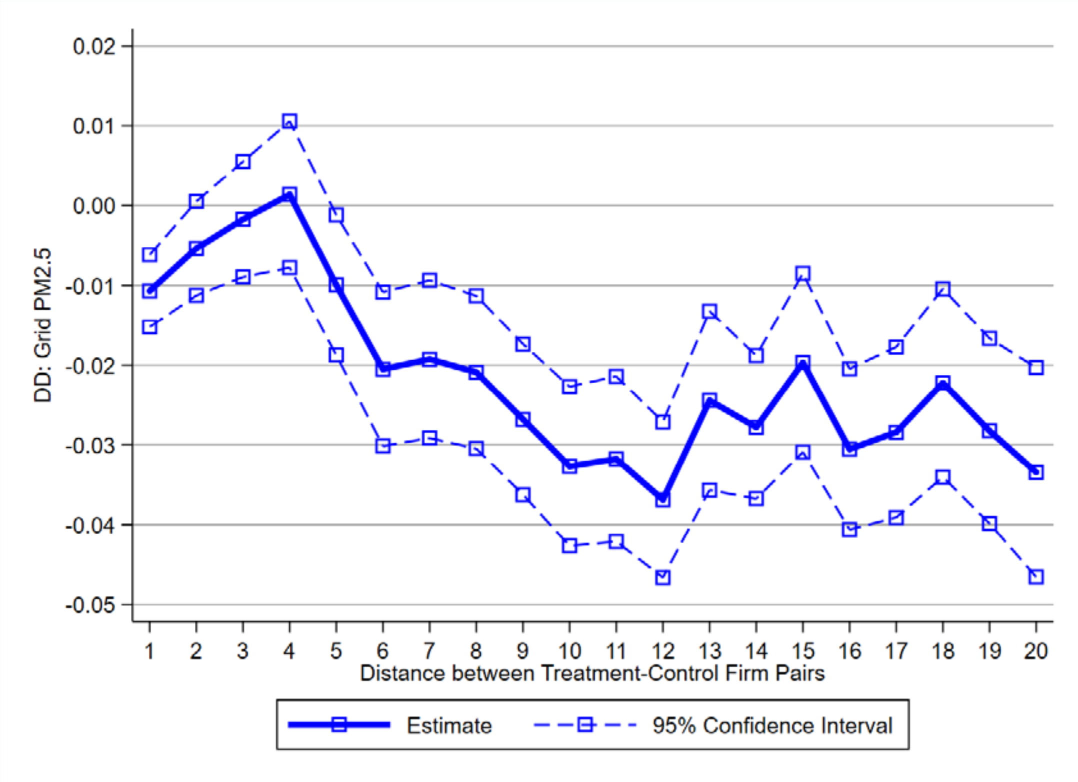
Zhang, P., J. Zhang, O. Deschênes, K. Meng (2018). “Temperature Effects on Productivity and Factor Reallocation: Evidence from a Half Million Chinese Manufacturing Plants,” *Journal of Environmental Economics and Management*, 88, 1 – 17.

Figure 1: Illustrative example of estimating combined, competitiveness, and ambient effects



Comparing firms 2 and 4 yields the combined effect while comparing firms 1 and 3 yields the competitiveness effect. The difference between the two equals the ambient effect.

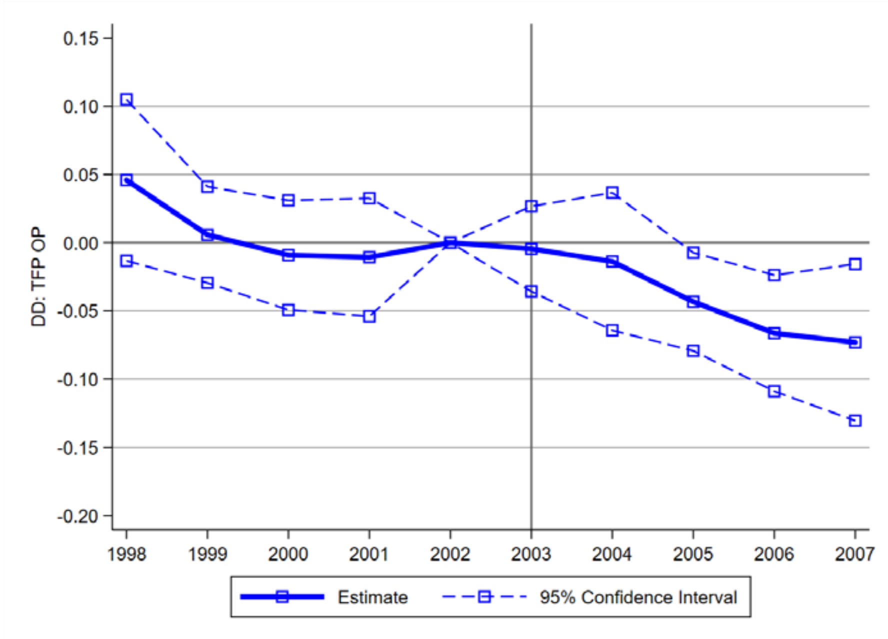
Figure 2: $PM_{2.5}$ concentrations as a function of distance between treatment and control firm pairs before versus after KCAPC policy (N = 280,699)



Point estimates and 95% confidence intervals for estimates of β_2 from estimating Equation (9) for $PM_{2.5}$ concentrations and values of $Band_g^d$ ranging from 1 to 20.

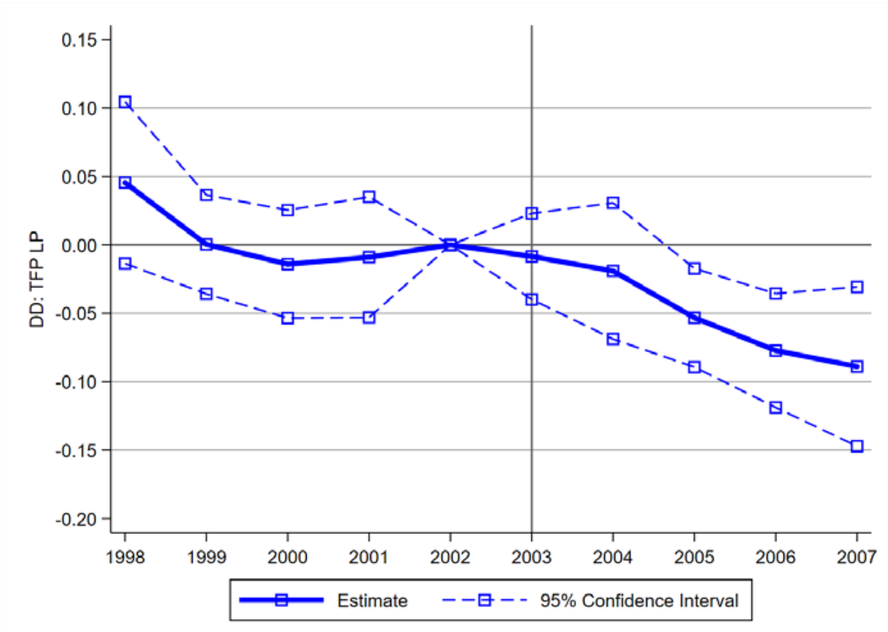
Figure 3: Test of parallel trends for DD estimation (control versus treatment cities) (N = 541,845)

Panel A: TFP (OP estimates)



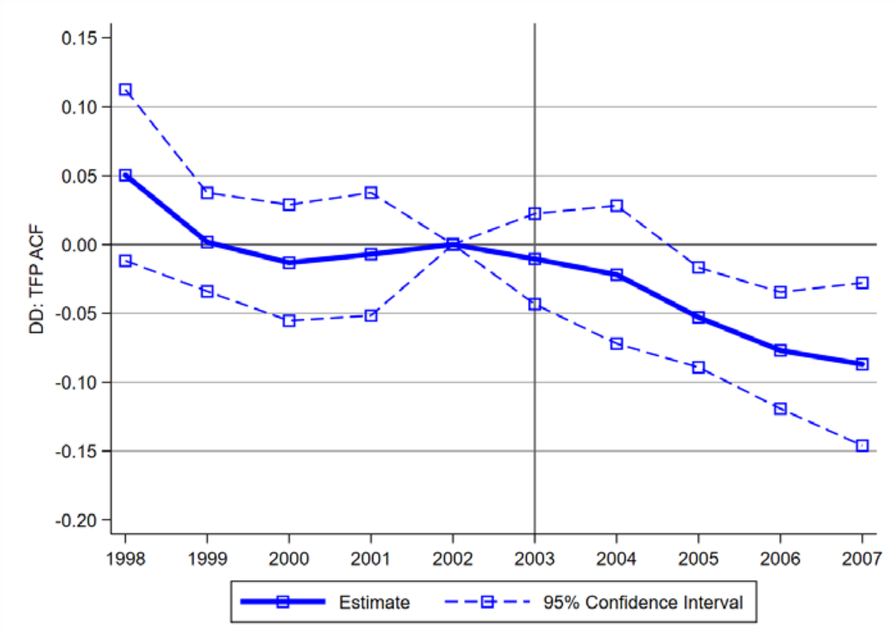
Coefficients and 95% confidence intervals for event studies (substituting year dummies for *Post2003* in Equation (8) of the main text) in the DD estimation.

Panel B: TFP (LP estimates)



Coefficients and 95% confidence intervals for event studies (substituting year dummies for *Post2003* in Equation (8) of the main text) in the DD estimation.

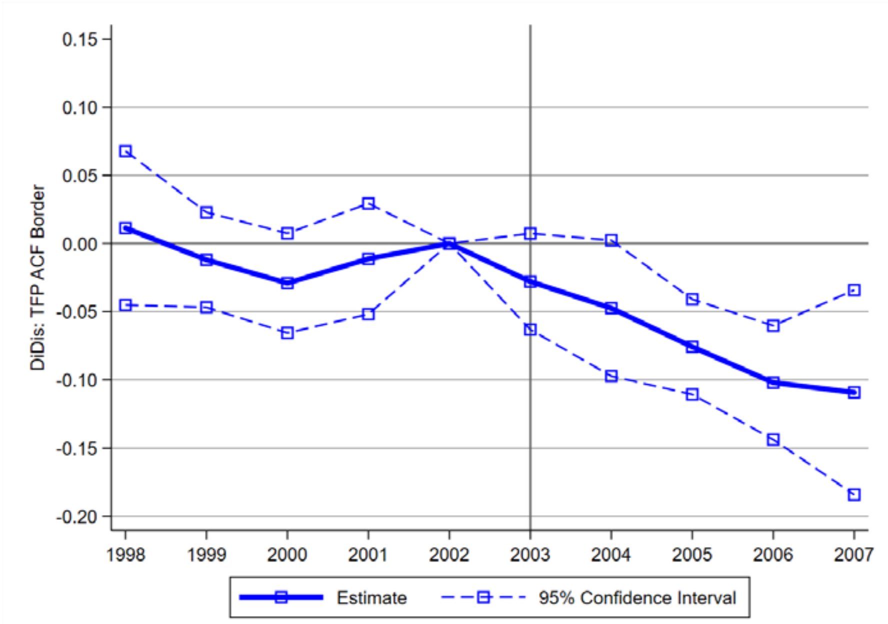
Panel C: TFP (ACF estimates)



Coefficients and 95% confidence intervals for event studies (substituting year dummies for *Post2003* in Equation (8) of the main text) in the DD estimation.

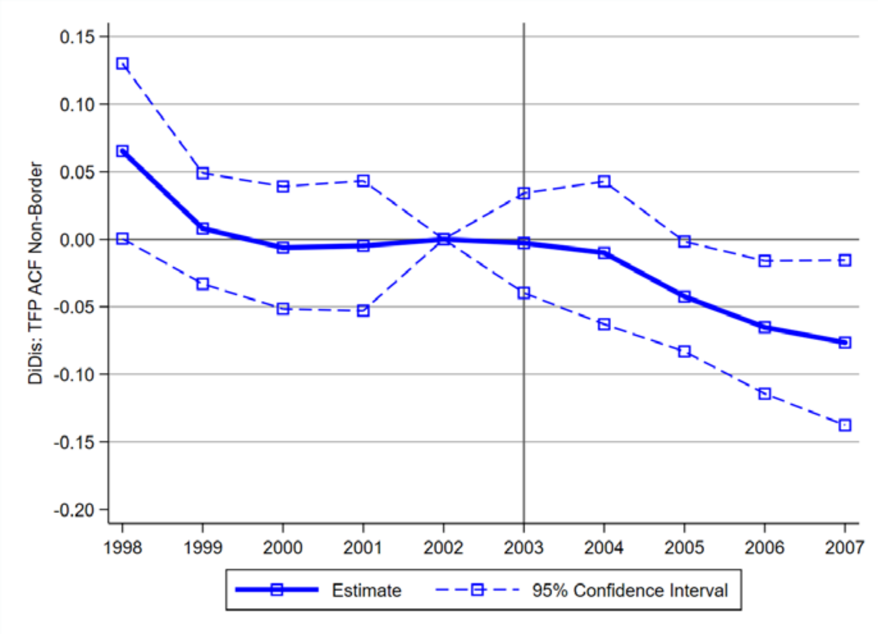
Figure 4: Pre-treatment trends and policy effects for ACF productivity measure for border firms, non-border firms, and differences between the two in the DD-Di estimation (N = 541,845)

Panel A: TFP (ACF estimates) border firms



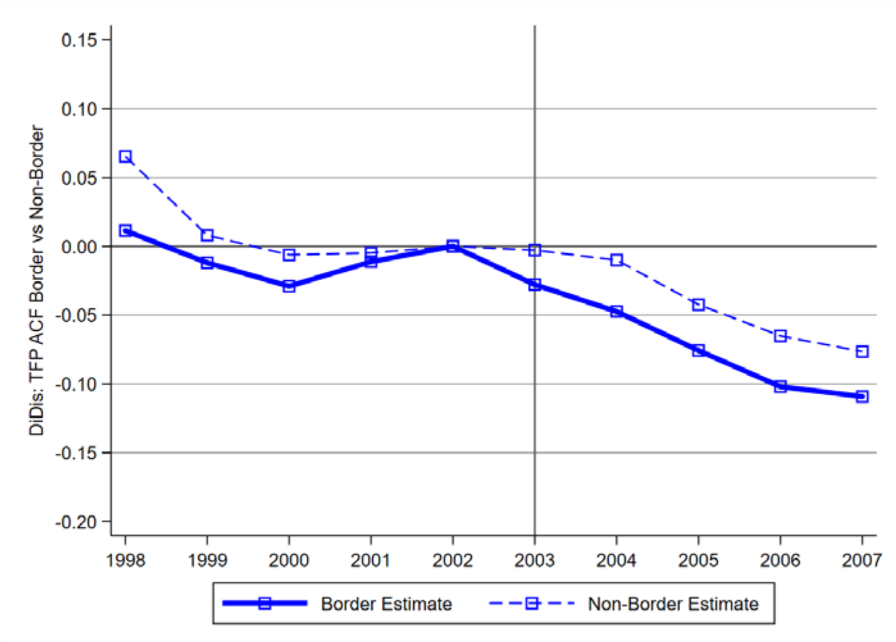
Coefficients and 95% confidence intervals for β_t^B in event study (substituting year-by-year coefficients β_t^B and β_t^F for $\beta^B * Post2003_t$ and $\beta^F * Post2003_t$ in Equation (10) of the main text).

Panel B: TFP (ACF estimates) non-border firms



Coefficients and 95% confidence intervals for β_t^F in event study (substituting year-by-year coefficients β_t^B and β_t^F for $\beta^B * Post2003_t$ and $\beta^F * Post2003_t$ in Equation (10) of the main text).

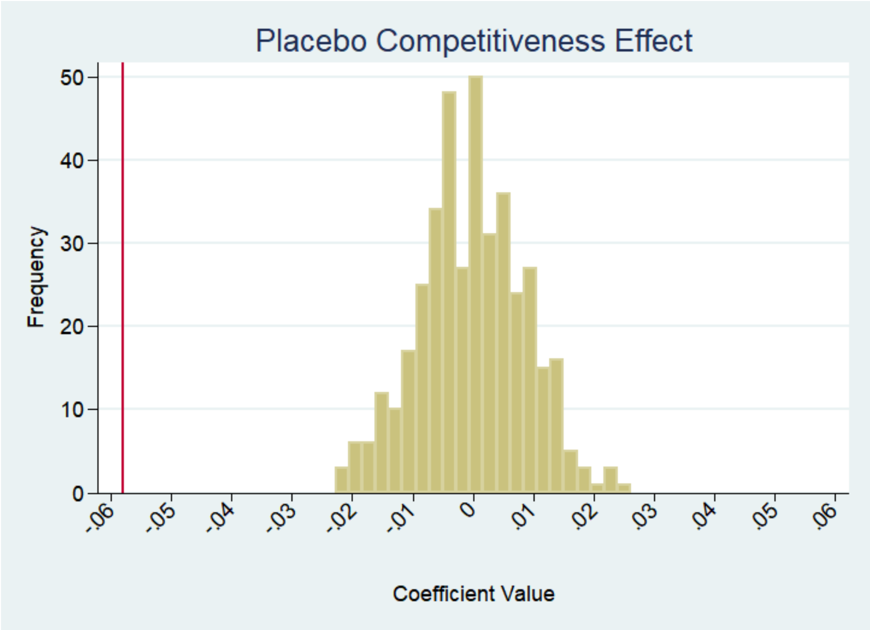
Panel C: TFP (ACF estimates) border versus non-border firms



Difference between β_t^B and β_t^F in event study (substituting year-by-year coefficients β_t^B and β_t^F for $\beta^B * Post2003_t$ and $\beta^F * Post2003_t$ in Equation (10) of the main text).

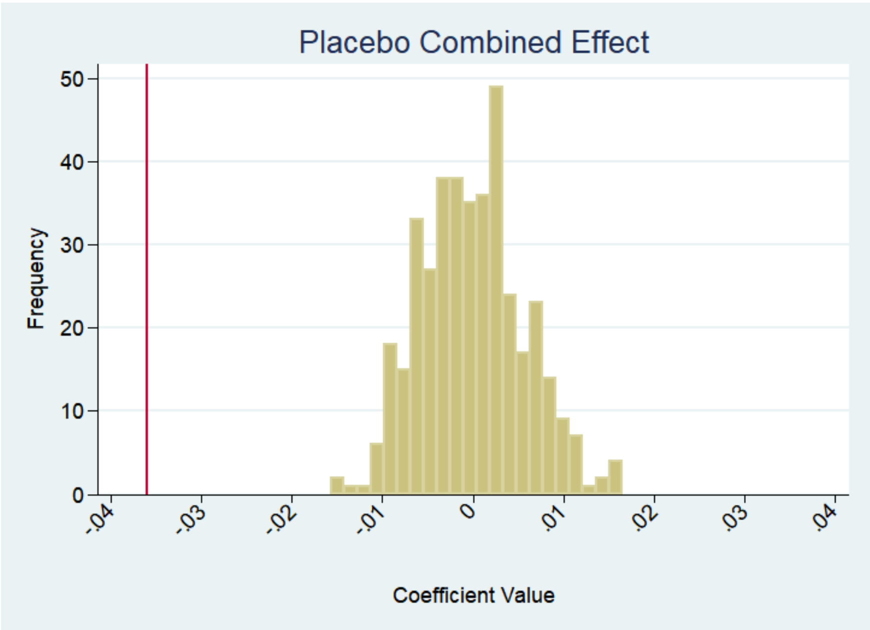
Figure 5: Non-parametric test of standard error clustering for DD-Di estimates using ACF measure for border firms and non-border firms

Policy effect – border firms (ACF estimates)



Probability density distributions of coefficients from estimating β^B in the DD-Di model using the ACF measure in Table 2, but assigning 113 out of 338 cities randomly as the treatment regions in 500 iterations. The red, vertical lines represent the coefficients estimated in Table 2.

Policy effect – non-border firms (ACF estimates)



Probability density distributions of coefficients from estimating β^F in the DD-Di model using the ACF measure in Table 2, but assigning 113 out of 338 cities randomly as the treatment regions in 500 iterations. The red, vertical lines represent the coefficients estimated in Table 2.

Table 1: Effect of KCAPC policy on productivity – naïve DD estimation

	Firm TFP OP Method (1)	Firm TFP LP Method (2)	Firm TFP LP ACF Method (3)
Policy*treatment	-0.034 ***	-0.041 ***	-0.043 ***
	(0.012)	(0.012)	(0.012)
Number of observations	541,845	541,845	541,845
Firm FE	YES	YES	YES
Province-by-year FE	YES	YES	YES
4-digit-sector-by-year FE	YES	YES	YES

This table reports results from estimating alternative versions of Equation (8) in the main text which regresses firms' productivity on the policy dummy interacted with a treatment dummy along with fixed effects identified at the bottom of the table using data from 1998 to 2007. The entries are the coefficients and standard erorrs (in parentheses) of the policy-treatment interaction. Standard errors are clustered at the province-year level. *** p<0.01, ** p<0.05, * p<0.1.

Table 2: Effect of KCAPC policy on productivity – DD-Di estimation

	(1)	(2)	(3)
	OP - Cobb Douglas	LP - Cobb Douglas	LP ACF - Translog
A: Far*policy*treatment	-0.027 **	-0.034 **	-0.036 ***
(Combined effect)	(0.013)	(0.013)	(0.014)
B: Border*policy*treatment	-0.049 ***	-0.057 ***	-0.058 ***
(Competitiveness effect)	(0.013)	(0.012)	(0.012)
Ambient effect (A - B)	0.021 *	0.023 *	0.022 *
	(0.013)	(0.012)	(0.012)
Number of observations		541,845	
Number of observations (border sub-sample)		146,818	
Firm FE	YES	YES	YES
Province-by-year FE	YES	YES	YES
4-digit-sector-by-year FE	YES	YES	YES

This table reports results from estimating alternative versions of Equation (10) in the main text which regresses firm productivity on the policy dummy interacted with a treatment dummy and then further interacted with a border (below 5 kilometers) and a non-border (above 5 kilometers) indicator along with alternative sets of fixed effects identified at the bottom of the table. Regressions use data from 1998 to 2007 and use different productivity measures identified at the top of the column. The entries are the coefficients and standard erorrs (in parentheses) of the policy-treatment-border interaction and policy-treatment-far interaction as well as the implied ambient effects based on the difference between the two coefficients (and its calculated standard error). Standard errors are clustered at the province-year level. *** p<0.01, ** p<0.05, * p<0.1.

Online Appendix

Appendix A: Marginal cost in conceptual framework

The firm's cost minimization problem is:

$$\min_{L_i, K_i} C_i = w_i L_i + r_i K_i \; st \; Q_i = A_i {\lambda_L}^{\alpha} L_i^{\alpha} K_i^{1-\alpha}, \text{ (A1)}$$

where w_i and r_i are the wages and interest rates faced by the firm. We have suppressed the dependence of λ_L on r and Ω for ease of exposition. Taking the first-order conditions:

$$\frac{\partial Q_i}{\partial L_i} = \alpha A_i {\lambda_L}^{\alpha} L_i^{\alpha-1} K_i^{1-\alpha} = w_i, \text{ (A2a)}$$
$$\frac{\partial Q_i}{\partial K_i} = (1-\alpha) A_i {\lambda_L}^{\alpha} L_i^{\alpha} K_i^{-\alpha} = r_i. \text{ (A2b)}$$

Dividing Equation (A2a) by (A2b):

$$\frac{\alpha K_i}{(1-\alpha) L_i} = \frac{w_i}{r_i}. \text{ (A3)}$$

And:

$$K_i = \frac{w_i}{r_i} \frac{(1-\alpha)}{\alpha} L_i. \text{ (A4)}$$

Substituting into the production function:

$$L_i = \frac{1}{A_i {\lambda_L}^{\alpha}} \left(\frac{w_i}{r_i} \frac{(1-\alpha)}{\alpha} \right)^{\alpha-1} Q_i. \text{ (A5)}$$

And substituting back into Equation (A4):

$$K_i = \frac{1}{A_i {\lambda_L}^{\alpha}} \left(\frac{w_i}{r_i} \frac{(1-\alpha)}{\alpha} \right)^{\alpha} Q_i. \text{ (A5)}$$

So that:

$$C_i = \frac{1}{A_i {\lambda_L}^{\alpha}} \phi w_i^{\alpha} r_i^{1-\alpha} Q_i. \text{ (A6)}$$

where $\phi = \left[\left(\frac{(1-\alpha)}{\alpha} \right)^{\alpha-1} + \left(\frac{(1-\alpha)}{\alpha} \right)^{\alpha} \right]$.

Finally:

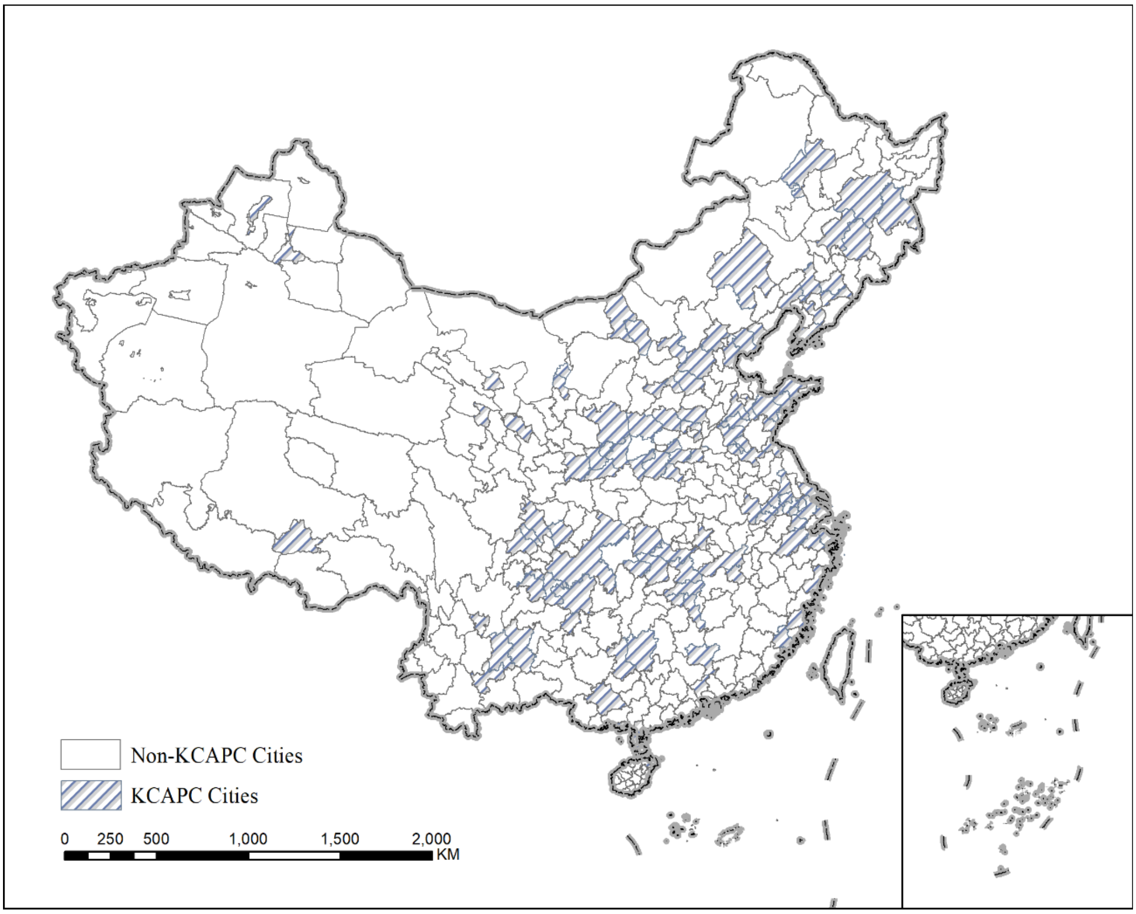
$$MC_i = \frac{dC_i}{dQ_i} = \frac{1}{A_i {\lambda_L}^{\alpha}} \phi w_i^{\alpha} r_i^{1-\alpha}. \text{ (A7)}$$

Appendix B: Class II pollution standards under GB3095-2000 (in mg/m³)

	Annual	Daily	Hourly
SO_2	0.06	0.15	0.50
NO_2	0.08	0.12	0.24
O_3	N/A	N/A	0.20
TSP	0.20	0.30	N/A
PM_{10}	0.10	0.15	N/A
CO	N/A	4.00	10.00

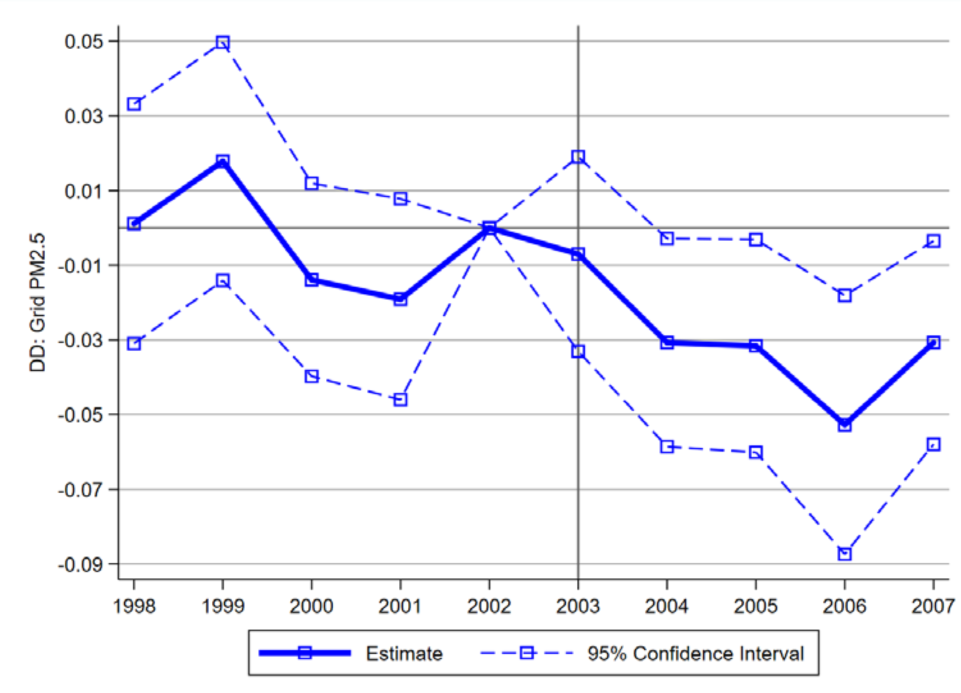
Based on Ministry of Environmental Protection of China (MEP) https://www.mee.gov.cn/gkml/zj/wj/200910/t20091022_171965.htm (in Chinese). N/A indicates not applicable.

Appendix C: Treatment and control cities under the KCAPC policy



Map displays the 113 treatment cities (shaded) subject to regulation under the KCAPC policy and the 225 control cities not subject to the regulation (unshaded).

Appendix D: Pre-treatment trends and policy effects for grid-level $PM_{2.5}$ concentrations



Coefficients and 95% confidence intervals for event studies (substituting year dummies β_t^D for $\beta^D Post2003_t$ in Equation (8) of the main text) using grid-year log $PM_{2.5}$ pollution concentrations in treatment and control cities as the dependent variable. Includes all grids in treatment areas and all grids in control areas except for those within five kilometers of a treatment area (N = 259,740).

Appendix E: Summary statistics for estimation sample (N = 541,845)

	(1) Mean	(2) St Dev	(3) Min	(4) Max
Log TFP (OP estimates)	2.87	0.99	-2.67	8.40
Log TFP (LP estimates)	5.42	0.94	0.57	9.86
Log TFP (LP ACF estimates)	7.94	0.87	3.49	12.30
Employment (persons)	227	297	10	3,013
Sales (CNY 1,000)	48,768	90,251	0	7,983,558
Value added (CNY 1,000)	13,845	24,258	74	366,426
Wages (CNY 1,000)	2,741	4,792	1	299,283
Capital (CNY 1,000)	15,881	30,869	64	350,534
Intermediate Inputs (CNY 1,000)	37,364	70,884	0	1,681,560
Number of firms		87,930		

Firm-level data from 1998 to 2007 used in estimation (firms that appear in at least one year before and one year after the policy change in 2003).

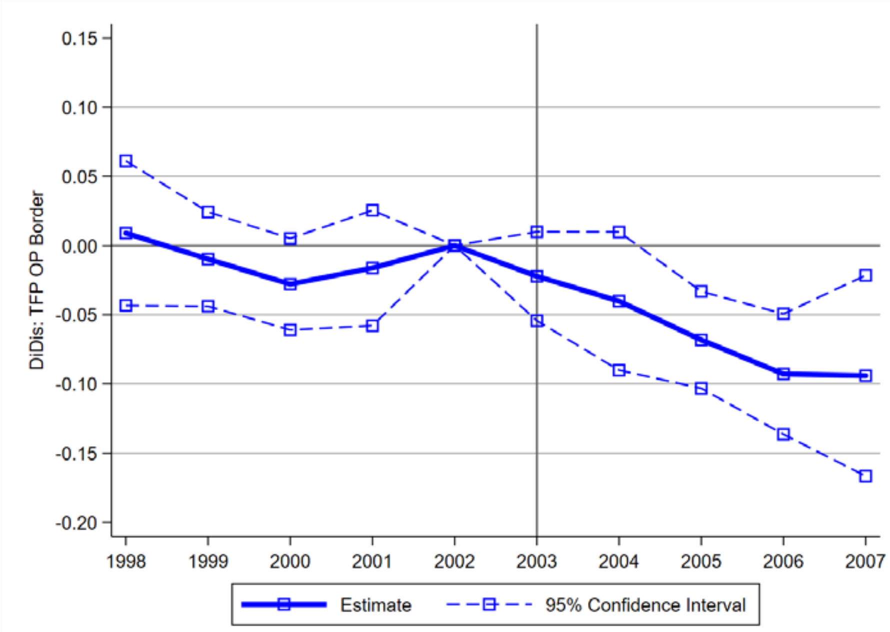
Appendix F: Characteristics of firms in treatment versus control cities for the border and non-border sub-samples

	(1)	(2)	(3)		(4)	(5)	(6)	(7)		(8)
	Border Sub-Sample					Non-Border Sub-Sample				
	Treatment	Control	Difference			Treatment	Control	Difference		
Log TFP (OP estimates)	2.902	2.844	0.058	***	2.0%	2.889	2.812	0.077	***	2.7%
	0.876	0.865	(0.005)			1.021	1.059	(0.006)		
Log TFP (LP estimates)	5.398	5.336	0.062	***	1.2%	5.468	5.351	0.117	***	2.2%
	0.857	0.850	(0.005)			0.961	0.989	(0.006)		
Log TFP (ACF estimates)	7.894	7.835	0.059	***	0.8%	7.997	7.889	0.108	***	1.4%
	0.769	0.769	(0.004)			0.894	0.926	(0.005)		
Employment (persons)	207.9	219.2	-11.32	***	-5.3%	231.4	236.5	-5.13	***	-2.2%
	274.3	293.5	(1.7)			300.8	308.4	(1.8)		
Sales (CNY 1,000)	49,167	43,793	5,374	***	11.6%	51,878	43,864	8,014	***	16.7%
	91,176	80,296	(483)			95,553	80,910	(493)		
Value added	12,472	11,371	1,101	***	9.2%	15,126	13,175	1,951	***	13.8%
(CNY 1,000)	22,558	20,626	(122)			25,914	23,048	(138)		
Wages (CNY 1,000)	2,869	2,584	286	***	10.5%	2,908	2,357	551	***	20.9%
	4,716	4,342	(26)			5,089	4,335	(26)		
Capital (CNY 1,000)	15,295	13,170	2,126	***	14.9%	17,091	14,803	2,288	***	14.3%
	31,233	27,368	(165)			32,436	28,220	(170)		
Intermediate input	38,351	34,376	3,974	***	10.9%	39,213	33,863	5,350	***	14.6%
(CNY 1,000)	72,944	64,843	(388)			73,909	64,491	(389)		
Number of Firms	16,337	6,767				42,085	22,743			
Number of Observations	104,831	41,987				262,547	132,490			

Firm-level data from 1998 to 2007 for treatment (Column (1)) versus control (Column (2)) firms in the border sub-sample defined by a five-kilometer maximum distance and for treatment (Column (5)) versus control (Column (6)) in the non-border subsample. Standard deviations in parentheses. Columns (3) and (7) provide the difference in means and its standard error in parentheses and Columns (4) and (8) the percentage difference. * = 10% significance, ** = 5% significance, *** = 1% significance for a two-sided t-test.

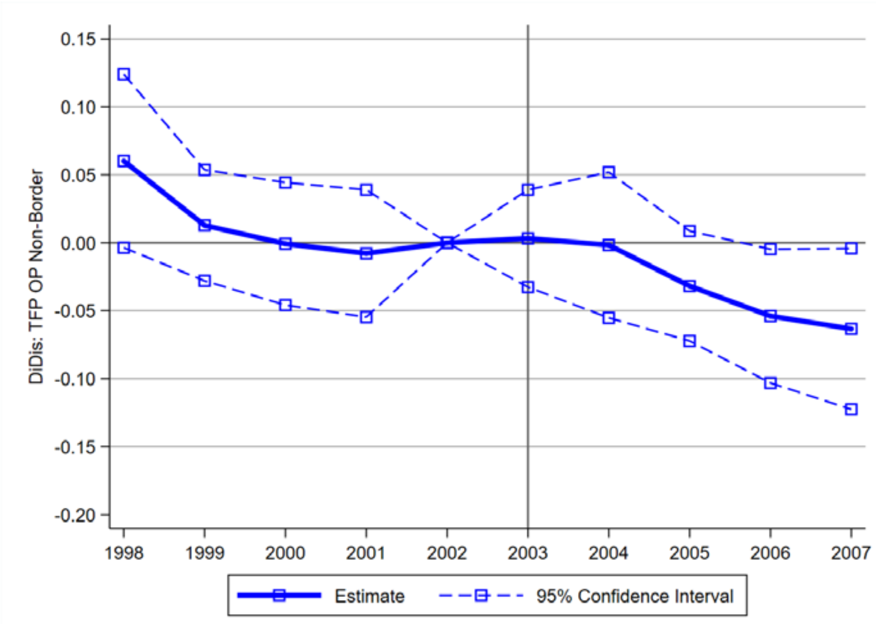
Appendix G: Pre-treatment trends and policy effects for OP productivity measure for border firms, non-border firms, and differences between the two in the DD-Di estimation (N = 541,845)

Panel A: TFP (OP estimates) border firms



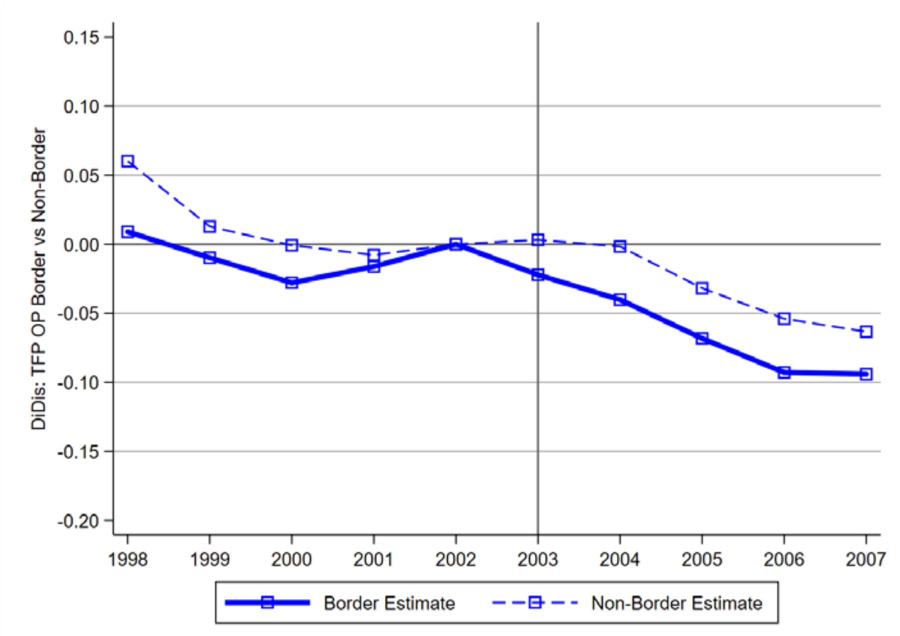
Coefficients and 95% confidence intervals for β_t^B in event study (substituting year-by-year coefficients β_t^B and β_t^F for $\beta^B * Post2003_t$ and $\beta^F * Post2003_t$ in Equation (10) of the main text).

Panel B: TFP (OP estimates) non-border firms



Coefficients and 95% confidence intervals for β_t^F in event study (substituting year-by-year coefficients β_t^B and β_t^F for $\beta^B * Post2003_t$ and $\beta^F * Post2003_t$ in Equation (10) of the main text).

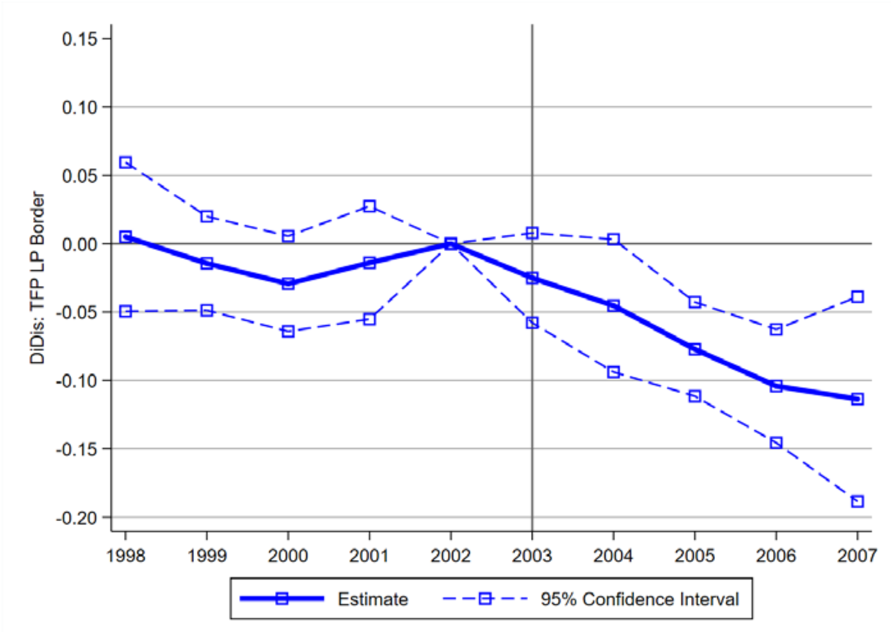
Panel C: TFP (OP estimates) border versus non-border firms



Difference between β_t^B and β_t^F in event study (substituting year-by-year coefficients β_t^B and β_t^F for $\beta^B * Post2003_t$ and $\beta^F * Post2003_t$ in Equation (10) of the main text).

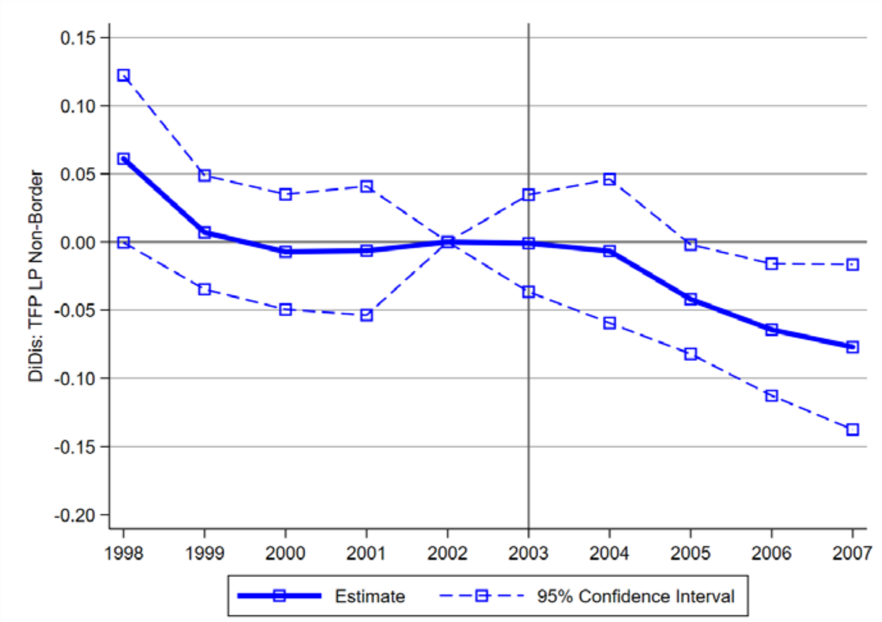
Appendix H: Pre-treatment trends and policy effects for LP productivity measure for border firms, non-border firms, and differences between the two in the DD-Di estimation (N = 541,845)

Panel A: TFP (LP estimates) border firms



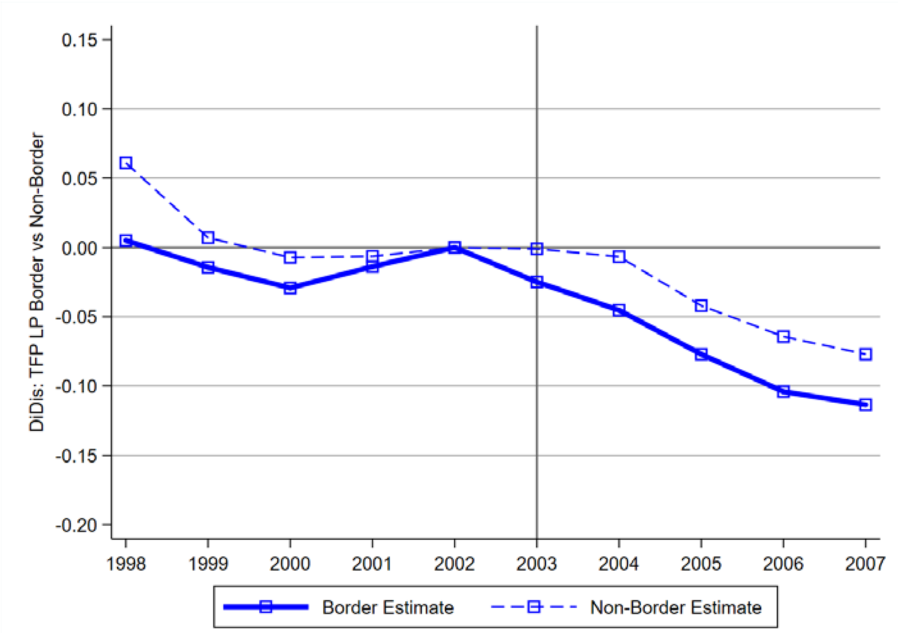
Coefficients and 95% confidence intervals for β_t^B in event study (substituting year-by-year coefficients β_t^B and β_t^F for $\beta^B * Post2003_t$ and $\beta^F * Post2003_t$ in Equation (10) of the main text).

Panel B: TFP (LP estimates) non-border firms



Coefficients and 95% confidence intervals for β_t^F in event study (substituting year-by-year coefficients β_t^B and β_t^F for $\beta^B * Post2003_t$ and $\beta^F * Post2003_t$ in Equation (10) of the main text).

Panel C: TFP (LP estimates) border versus non-border firms



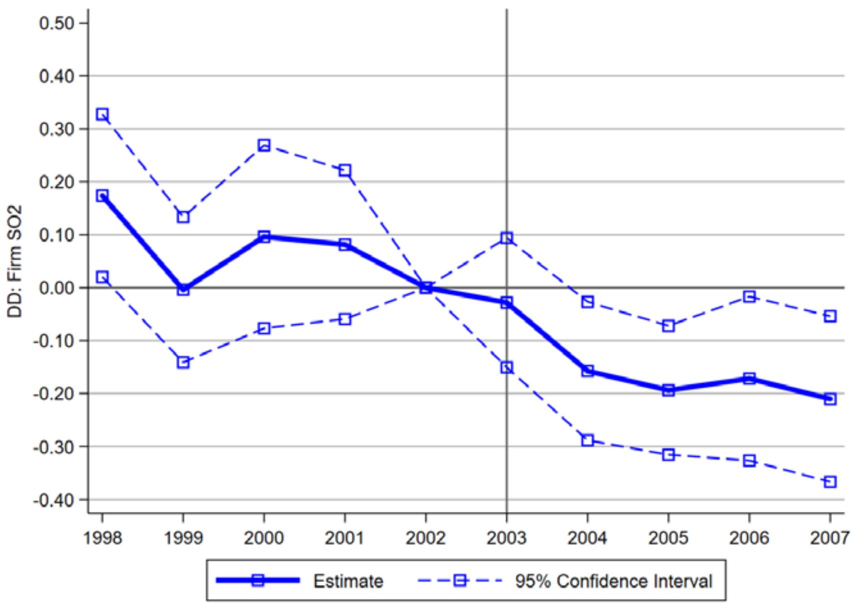
Difference between β_t^B and β_t^F in event study (substituting year-by-year coefficients β_t^B and β_t^F for $\beta^B * Post2003_t$ and $\beta^F * Post2003_t$ in Equation (10) of the main text).

Appendix I: Test of pollution manipulation on outskirts of treatment regions using firm-level SO_2 emissions data

	(1) Within 1 Kilometer	(2) Within 5 Kilometers	(3) Within 10 Kilometers
Policy*treatment	-0.1508 **	-0.1835 ***	-0.1514 ***
	(0.0642)	(0.0524)	(0.0446)
Policy*treatment*outskirts	-0.0297	0.0079	-0.0470
	(0.0569)	(0.0513)	(0.0499)
Number of observations	46,428		
Firm FE	YES	YES	YES
Province-by-year FE	YES	YES	YES
4-digit-sector-by-year FE	YES	YES	YES

This table reports results from estimating Equation (8) in the main text with log SO_2 emissions as the dependent variable regressed on the policy dummy interacted with a treatment dummy and this variable further interacted with an outskirts indicator along with sets of fixed effects identified at the bottom of the table using data from 1998 to 2007. Columns 1 through 3 show the results with the outskirts indicator set to one if the firm is within 1, 5, and 10 kilometers of the outer edges of a region respectively and zero otherwise. Entries are the coefficient and standard errors (in parentheses) of the policy interactions. Standard errors are clustered at the city-year level. *** p<0.01, ** p<0.05, * p<0.1.

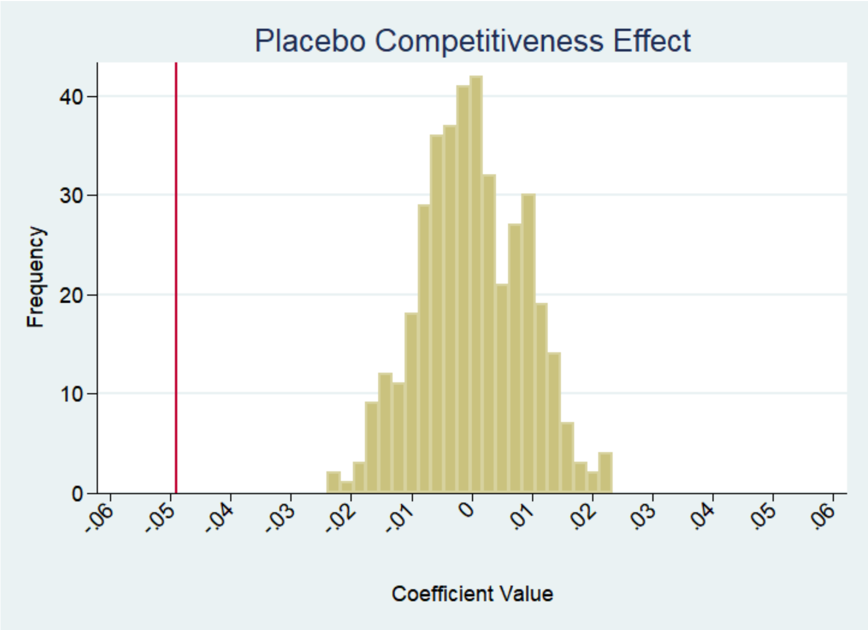
Appendix J: Pre-treatment trends and policy effects for firm-level SO_2 emissions



Coefficients and 95% confidence intervals for event studies (substituting year dummies β_t^D for $\beta^D Post2003_t$ in Equation (8) of the main text) using firm-year log SO_2 emissions as the dependent variable (N = 46,428).

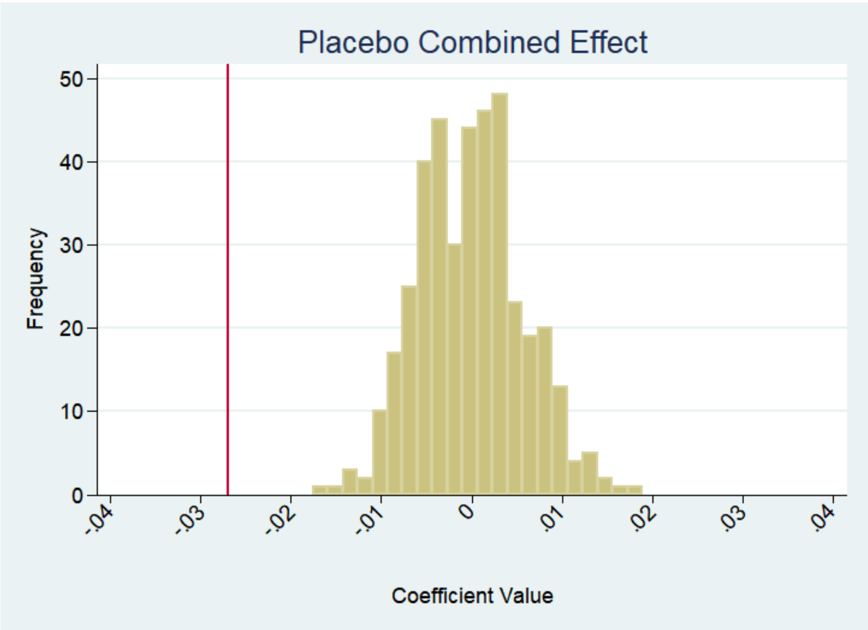
Appendix K: Non-parametric test of standard error clustering for DD-Di estimates for border firms and non-border firms (OP and LP measures)

Policy effect – border firms (OP estimates)



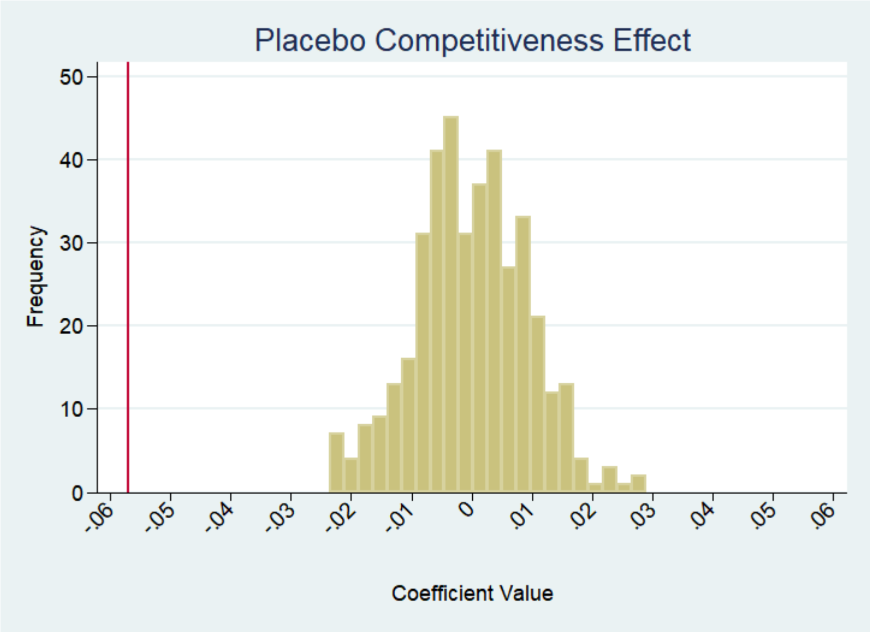
Probability density distributions of coefficients from estimating β^B in the DD-Di model using the OP measure in Table 2, but assigning 113 out of 338 cities randomly as the treatment regions in 500 iterations. The red, vertical lines represent the coefficients estimated in Table 2.

Policy effect – non-border firms (OP estimates)



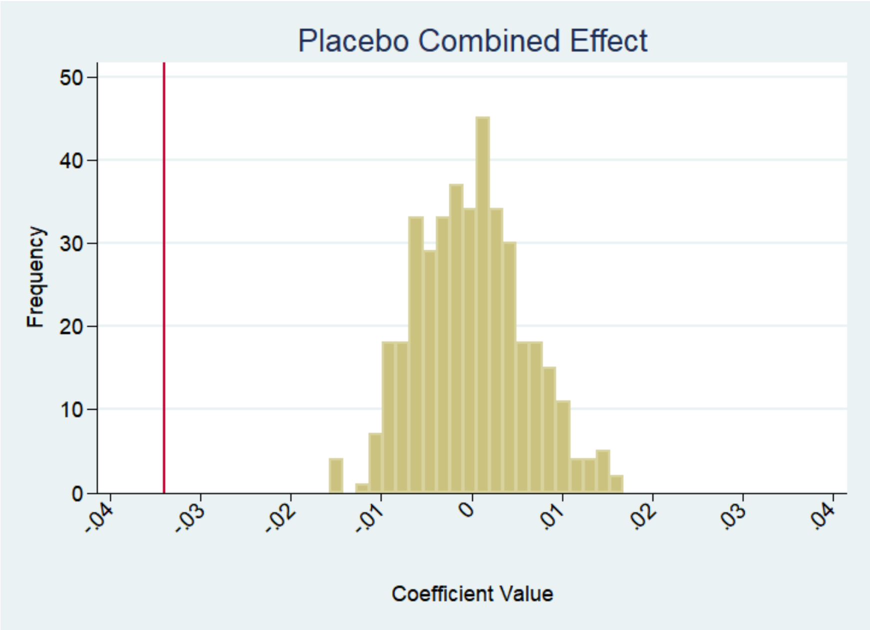
Probability density distributions of coefficients from estimating β^F in the DD-Di model using the OP measure in Table 2, but assigning 113 out of 338 cities randomly as the treatment regions in 500 iterations. The red, vertical lines represent the coefficients estimated in Table 2.

Policy effect – border firms (LP estimates)



Probability density distributions of coefficients from estimating β^B in the DD-Di model using the LP measure in Table 2, but assigning 113 out of 338 cities randomly as the treatment regions in 500 iterations. The red, vertical lines represent the coefficients estimated in Table 2.

Policy effect – non-border firms (LP estimates)



Probability density distributions of coefficients from estimating β^F in the DD-Di model using the LP measure in Table 2, but assigning 113 out of 338 cities randomly as the treatment regions in 500 iterations. The red, vertical lines represent the coefficients estimated in Table 2.

Appendix L: Effect of KCAPC policy on firm markups inferred using production function approach

	(1)	(2)
	All Firms	Border Firms
Policy*treatment	-0.0164	-0.0464
	(0.0477)	(0.0365)
Number of observations	404,119	116,855

Results from estimating Equation (11) in the main text with markups calculated following De Loecker and Warzinsky (2012) regressed on the policy dummy, treatment dummy, the policy dummy interacted with the treatment dummy, log labor, log capital, and log TFP. Markups are estimated industry-by-industry at the three-digit industry level. Number of observations differ from full sample because some industries with a small number of observations (fewer than 1,422 on average) resulted in negative markups. Entries are the coefficients and standard erorrs (in parentheses) of the policy-treatment interaction. Standard errors are clustered at the province-year level. *** p<0.01, ** p<0.05, * p<0.1.

Appendix M: Effect of KCAPC policy on firm "exit" and "entry" – DD estimation

	(1)	(2)	(3)	(4)
	Fraction Firms Leaving Sample	Fraction SOEs Exiting	Fraction Non-SOEs Leaving Sample	Fraction Firms Appearing
Policy*treatment	-0.002	-0.005	-0.003	-0.003
	(0.010)	(0.019)	(0.010)	(0.013)
Number of observations	2,936	2,834	2,888	2,976
City FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES

Results from estimating Equation (12) in the main text with different city-year outcome measures as specified at the top of each column along with sets of fixed effects identified at the bottom of the table. Coumns (1) through (3) use data from 1998 to 2006; Column (4) from 1999 to 2007. Number of observations in Columns (1) through (3) differ because some cities did not experience firm departures in a given year. The entries are the coefficients and standard erorrs (in parentheses) of the policy-treatment interaction. Standard errors are clustered at the province-year level. *** p<0.01, ** p<0.05, * p<0.1.

Appendix N: Effect of KCAPC policy on productivity – robustness to firm "exit"

	(1)	(2)	(3)	(4)	(5)	(6)
	OP - Cobb Douglas		LP - Cobb Douglas		LP-ACF - Translog	
	Baseline	Exit	Baseline	Exit	Baseline	Exit
A: Far*Policy*treatment	-0.027 **	-0.026 *	-0.034 **	-0.032 **	-0.036 ***	-0.038 ***
	(0.013)	(0.014)	(0.013)	(0.014)	(0.014)	(0.015)
B: Border*policy*treatment	-0.049 ***	-0.041 ***	-0.057 ***	-0.048 ***	-0.058 ***	-0.051 ***
	(0.013)	(0.015)	(0.012)	(0.015)	(0.012)	(0.014)
C: Exit*far*policy*treatment		-0.018		-0.024		0.012
		(0.018)		(0.019)		(0.019)
D: Exit*border*policy*treatment		-0.038 ***		-0.045 **		-0.039 *
		(0.015)		(0.022)		(0.020)
Number of observations	541,845					
Number of exit observations	180,556					
Firm FE	YES	YES	YES	YES	YES	YES
Province-by-year FE	YES	YES	YES	YES	YES	YES
4-digit-sector-by-year FE	YES	YES	YES	YES	YES	YES

This table reports results from augmenting Equation (10) in the main text by further interacting the border-policy-treatment and non-border-policy-treatment dummies with an indicator set to one if the firm exits or, if a private firm, falls below the CNY 5 million threshold before the end of the sample period along with sets of fixed effects identified at the bottom of the table. Regressions use data from 1998 to 2007 and apply a maximum distance of five kilometers to determine the border firms. The entries are the coefficients and standard erorrs (in parentheses) of the various policy-treatment interactions. Standard errors are clustered at the province-year level. *** p<0.01, ** p<0.05, * p<0.1.

Appendix O: Effect of KCAPC policy on productivity (DD-Di estimation) - robustness checks

	(1)	(2)	(3)	(4)
	Firm TFP (LP ACF Method - Translog)			
A: Far*policy*treatment	-0.036 ***	-0.037 ***	-0.036 ***	-0.033 ***
	(0.014)	(0.013)	(0.013)	(0.013)
B: Border*policy*treatment	-0.058 ***	-0.058 ***	-0.058 ***	-0.056 ***
	(0.012)	(0.012)	(0.012)	(0.012)
Ambient effect (A - B)	0.022 *	0.021 *	0.022 *	0.023 *
	(0.012)	(0.012)	(0.012)	(0.012)
Number of observations	541,845	541,845	541,845	541,845
Firm FE	YES	YES	YES	YES
Province-by-year FE	YES	YES	YES	YES
4-digit-sector-by-year FE	YES	YES	YES	YES
Weighted by firm value added		YES		
Weighted by firm employment			YES	
Weather controls				YES

This table reports results from estimating Equation (10) in the main text, regressing firm productivity (ACF method using a translog function) on the policy dummy interacted with a treatment dummy and then further interacted with the border indicator (within 5 kilometers), and a non-border indicator (above 5 kilometers). All regressions use data from 1998 to 2007 along with alternative sets of fixed effects, control variables, and observation weightings identified at the bottom of the table. The entries are the coefficients and standard erorrs (in parentheses) of the policy-treatment interactions. *** p<0.01, ** p<0.05, * p<0.1.

Appendix P: Agglomeration effects in conceptual framework

To test for agglomeration effects, we follow Greenstone *et al.* (2010) and assume that firm i's TFP is affected by the number of proximate firms. As that paper notes, agglomeration spillovers can be either positive, due to innovations or production efficiencies that span firms, or negative, due to increases in factor prices that occur when firms become more productive. The total number of proximate firms for firm i (N_i) is comprised of treatment (N_{Ti}) and control firms (N_{Ci}) so that $N_i = N_{Ti} + N_{Ci}$. We also assume that ambient pollution changes in response to the policy may increase in the number of proximate treatment but not proximate control firms given the policy regulates the former but not the latter. We modify Equation (4) in the main text as:

$$TFP_i = A_i \lambda_L(r, \Omega(N_{Ti}), N_i)^{\alpha}. \text{ (A8)}$$

We assume ambient effects weakly increase in the number of local treatment firms: $\partial\Omega(N_{Ti})/\partial N_{Ti} \geq 0$ and a local control firm has the same agglomeration effects as a local treatment firm: $\partial\lambda_L/\partial N_{Ci} = \partial\lambda_L/\partial N_{Ti} = \partial\lambda_L/\partial N_i$. Agglomeration effects can be positive, negative, or nil ($\partial\lambda_L/\partial N_i \leq\geq 0$) and can be either convex, concave, or neither in the number of firms ($\partial^2\lambda_L/\partial N_i^2 \leq\geq 0$).

To generate triple differences with respect to the number of treatment and control firms, we begin with Equation (5a) in the main text:

$$\frac{\partial ln(TFP_i)}{\partial r} = \alpha\frac{\partial ln(\lambda_L)}{\partial r} + \alpha\frac{\partial ln(\lambda_L)}{\partial \Omega}\frac{\partial\Omega}{\partial \text{r}}. \text{ (A9)}$$

Taking the derivatives with respect to the number of control and treatment firms, respectively, provides the triple differences:

$$\frac{\partial ln(TFP_i)}{\partial r\partial N_{Ci}} = \underset{(1)}{\alpha\frac{\partial ln(\lambda_L)}{\partial r}\frac{\partial ln(\lambda_L)}{\partial N_i}} + \underset{(2)}{\alpha\frac{\partial ln(\lambda_L)}{\partial \Omega}\frac{\partial\Omega}{\partial \text{r}}\frac{\partial ln(\lambda_L)}{\partial N_i}}, \text{ (A10a)}$$

and

$$\frac{\partial ln(TFP_{it})}{\partial r\partial N_{Ti}} = \underset{(1)}{\alpha\frac{\partial ln(\lambda_L)}{\partial r}\frac{\partial ln(\lambda_L)}{\partial N_i}} + \underset{(2)}{\alpha\frac{\partial ln(\lambda_L)}{\partial \Omega}\frac{\partial\Omega}{\partial \text{r}}\frac{\partial ln(\lambda_L)}{\partial N_i}} + \underset{(3)}{\alpha\frac{\partial ln(\lambda_L)}{\partial r}\frac{\partial ln(\lambda_L)}{\partial \Omega}\frac{\partial\Omega}{\partial N_{Ti}}} + \underset{(4)}{\alpha\frac{\partial^2 ln(\lambda_L)}{\partial \Omega^2}\frac{\partial\Omega}{\partial \text{r}}\frac{\partial\Omega}{\partial N_{Ti}}}. \text{ (A10b)}$$

Term (1) in Equations (A10a) and (A10b) are the amplification of the competitiveness effect by any agglomeration effects while term (2) is the amplification of the ambient effect by any agglomeration effects. Term (3) captures how the competitiveness effect is amplified by the ambient effect as a function of the number of treatment firms. Term (4) is the scaling of the ambient effect due to any nonlinearities with respect to the number of treatment firms.

Given that control and treatment firms paired together in the DD-Di estimation face the same proximate firms and the ambient effects ($\partial\Omega/\partial\text{r}$) are held constant in the border sub-sample, term (2) in Equations (A10a) and (A10b) is differenced out in estimation leaving:

$$\frac{\partial ln(TFP_{it})}{\partial r\partial N_{Ci}} = \alpha\frac{\partial ln(\lambda_L)}{\partial r}\frac{\partial ln(\lambda_L)}{\partial N_i}, \text{ (A11a)}$$

and

$$\frac{\partial ln(TFP_{it})}{\partial r \partial N_{Ti}} = \underset{(1)}{\alpha \frac{\partial ln(\lambda_L)}{\partial r} \frac{\partial ln(\lambda_L)}{\partial N_i}} + \underset{(3)}{\alpha \frac{\partial ln(\lambda_L)}{\partial r} \frac{\partial ln(\lambda_L)}{\partial \Omega} \frac{\partial \Omega}{\partial N_{Ti}}} + \underset{(4)}{\alpha \frac{\partial^2 ln(\lambda_L)}{\partial \Omega^2} \frac{\partial \Omega}{\partial \mathrm{r}} \frac{\partial \Omega}{\partial N_{Ti}}}. \quad \text{(A11b)}$$

The triple difference with respect to the number of proximate control firms (Equation (A11a)) reflects changes in agglomeration effects in response to the policy. Therefore, the interaction of the treatment dummy, the policy dummy, and the proximate number of control firms in a DD-Di estimation captures the agglomeration effect. If this is significantly different than zero then the baseline estimates of the competitiveness effects includes agglomeration spillovers.

In estimating this it is necessary to control for the triple difference with respect to the number of proximate treatment firms (Equation (A11b)) because it includes both agglomeration and amplification of ambient effects. This can be captured by including the interaction of the treatment dummy, the policy dummy, and the proximate number of treatment firms. In the absence of an agglomeration effect (term (1) is zero), the sign of the coefficient on this term could be either positive or negative. Term (3) is weakly positive (the negative competitiveness effect estimated in the baseline results is mitigated more by the ambient effect in areas with more treatment firms); but term (4) could be either positive or negative depending on whether the ambient effects is concave or convex.

Appendix Q. Effect of KCAPC policy on productivity using D-DD-Di estimation – agglomeration effects

	(1) OP - Cobb Douglas	(2) LP - Cobb Douglas	(3) LP - ACF Translog
Border*policy*treatment	0.0708 *	0.0733 *	0.0805 *
	(0.0420)	(0.0425)	(0.0411)
Border*policy*treatment*ln(# proximate treatment firms)	-0.0368 ***	-0.0410 ***	-0.0381 ***
	(0.0102)	(0.0110)	(0.0110)
Border*policy*treatment*ln(# proximate control firms)	-0.0063	-0.0024	-0.0030
	(0.0105)	(0.0097)	(0.0099)
Competitiveness effect at mean densities	-8.2%	-9.7%	-7.8%
Number of observations		409,156	
Number of observations (border sub-sample)		146,804	
Firm FE	YES	YES	YES
Province-by-year FE	YES	YES	YES
4-digit-sector-by-year FE	YES	YES	YES

This table reports results from estimating Equation (13) in the main text, regressing firm productivity on the border-policy-treatment dummy further interacted with number of proximate treatment and number of proximate control firms, along with the non-border-policy-treatment dummy interacted with number of proximate treatment firms and sets of fixed effects identified at the bottom of the table. 5-kilometer distance used to define border and proximate firms. Regressions use data from 1998 to 2007. Observations with zero treated firms within the maximum radius are excluded. The entries are the coefficients and standard erorrs (in parentheses) of the policy-treatment interactions. Standard errors are clustered at the province-year level. *** p<0.01, ** p<0.05, * p<0.1.