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

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Abstract

Measuring environmental regulation's effect on firm competitiveness is central to designing optimal policies. Existing studies document significant negative effects of air pollution regulations on manufacturing competitiveness as measured by total factor productivity (TFP). A separate literature finds that air pollution lowers TFP through its ambient effect on workers' physical and mental health and cognition. Extant empirical measures reflect some combination of the competitiveness and ambient effects and only under specific circumstances will they reflect the average policy effect across all firms. We develop a boundary-discontinuity-difference-in-differences approach to isolate the competitiveness effect: both regulated and unregulated firms adjacent to each other enjoy the ambient effect via spillovers but only regulated firms suffer the competitiveness effect. We apply the approach to a major air pollution regulation in China. The traditional approach to estimating the regulation's effect yields a 3.75% TFP decline and net policy cost of USD 21.2 billion. The true competitiveness effect is 6.40% implying an ambient effect of 2.65% among regulated firms. The true cost of the policy is lower (about USD 10.4 billion) because proximate non-regulated firms also enjoy the ambient effect.

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1. Introduction

Theoretically, binding environmental regulations can raise or lower firms' costs. Regulations impose compliance costs on firms including capital costs, such as pollution abatement equipment, and labor costs, such as 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. Regulations involve implementation costs but also impose costs on firms if firm competitiveness is lowered. 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 firms will resist or encourage the enactment of environmental regulations and how strongly.

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 (Berman and Bui (2001) and Gowrisankaran *et al.* (2020) estimate effects in particular industries).[2] The Act imposed regulations on plants not in compliance with pollution standards across multiple pollutants. 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.[3] The other notable estimate of a competitiveness effect is He *et al.* (2020) for water pollution in China using an increase in regulatory stringency in 2003. Using data from 2000 to 2007, the paper finds a 24% reduction in TFP for firms subject to monitoring versus those not (Wang *et al.* (2018) estimate effects for water pollution in a region of China).

The typical approach to quantifying a competitiveness effect in the case of air pollution is a difference-in-differences (DD) estimate comparing treatment firms subject to the regulation to control firms that are not. A separate literature (Graff Zivin and Neidell, 2012; Chang *et al.*, 2019; He *et al.*, 2019; Fu *et al.*, 2021) estimates how air pollution reduces output due to effects on the physical and mental stamina of workers or work absences. This implies that regulations that reduce air pollution

[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] Other studies examine regulatory effects on innovation (e.g., Jaffe and Palmer (1997)).

[3] We do not find evidence effects on firm survival, so such an adjustment is unnecessary in our setting.

will result in 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 will estimate some combination of the competitiveness and ambient effects (which we term the “combined effect”) and understate the competitiveness effect (in absolute value) if interpreted as such.

Figure 1 illustrates these different effects. The competitiveness effect is displayed along the top path. This path reflects the net effect of the two forces – compliance costs and process improvements. The bottom path of the diagram shows the ambient effect – the extent to which pollution reductions increase output via lower morbidity, lower mortality, greater stamina, and improved cognition. The sum of these two effects equals the combined effect (shown on the right-hand side) and is what is measured in previous papers.

[Insert Figure 1 here]

Decomposing the combined effect into its two components is necessary in achieving socially-optimal air pollution reductions. Consider an environmental regulation aimed at reducing manufacturing emissions and suppose it imposes a net regulatory cost on regulated firms. The regulation creates 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 all proximately-located firms).

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. Interpreting the combined effect as a competitiveness effect 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 CO in response to the CAAA. This could be consistent with the Porter Hypothesis, but if the ambient effect embedded in their estimate exceeds 2.2% this would be inconsistent. From a political economy perspective, prospective regulated firms will base their support of a regulation on the combined effect while nearby unregulated firms will favor based on the ambient effect. This also means that air pollution restrictions are less costly in regions with high firm densities.

If all firms are included, then DD estimates will provide the average policy (competitiveness and ambient) effect across all firms. Generally, not all data will be included. In some cases, data may not be available for all firms. Even if all data is available, only firms present before and after the policy change will identify the combined effects. In this case, the DD estimates will only be representative of all firms if the proportion of treatment and control firms (and geographic locations of control firms vis-à-vis treatment regions) in the estimated subsample are representative of the full sample. Otherwise, unbiased average policy effects can only be obtained by isolating the competitiveness and ambient effects and then grossing up the effects given the number and geographic placement of all firms. We illustrate this later in our setting and find that net policy effects based on the decomposition of competitiveness and ambient effects is quite different than that based on naïve DD estimates.

To disentangle the competitiveness and ambient effects we develop a boundary-discontinuity-difference-in-differences (BD-DD) approach and apply it to a major air pollution regulation in China. We identify pairs of firms that are geographically close to each other (ten kilometers or less), some of which are subject to the regulation (the treatment group) and others of which are not (the control group). We then compare the response of the two groups to the advent of the regulation (the treatment). Since the control and treatment groups are in close proximity, they experience the same air pollution concentrations both before and after the policy implementation and differ only in the application of the regulation after its advent. This differs from the typical DD estimates which use treatment and control firms regardless of distance from each other. In this case, the two groups experience different ambient pollution levels with the advent of the regulation, in addition to the difference in regulatory compliance. Figure 2 illustrates our approach to identifying the competitiveness effects using a heat map of $PM_{2.5}$ pollution concentrations. It shows two example borders (one with low and one with moderate pollution). In both examples, restricting the maximum distance from the border (and therefore between firms in a pair) to 10 kilometers or less ensures that the pollution is similar on both sides of the border while distances of 50 kilometers or more does not.

[Insert Figure 2 here]

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.[4] We apply the BD-DD approach to estimate the policy's competitiveness effect on TFP in China's manufacturing sector. We also apply the traditional DD approach to estimate the policy's combined effect. The difference between these two equals the productivity improvements due to ambient pollution reductions from the policy. The standard DD approach estimates a combined effect of -3.75%. The BD-DD approach estimates a competitiveness effect of -6.40% implying that the ambient effect is a 2.65% productivity increase for firms in treatment cities.[5] Thus, the direct regulatory costs on firms would be understated by 2.65 percentage points or 41.4% using the pre-existing DD approach. These estimates apply to firms in continued operation; however, we find similar combined and competitiveness effects for firms that exit post-policy as we do for the whole sample. The estimates also assume no price effects (we must use a measure of revenue rather than physical output and therefore price changes in response to the policy would confound the estimates). We provide some evidence that price effects are not significant. We also 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.

This paper is most closely related to Greenstone *et al.* (2012). It differs in that the focus is on developing a method to decompose the combined effect into the competitiveness and ambient effects. Also closely related to our work is He *et al.* (2020). It employs a regression discontinuity (RD) approach comparing the productivity of firms immediately upstream of a water quality monitoring station to those immediately downstream. Upstream firms are affected by the regulation while downstream firms are not because upstream pollution is measurable while downstream is not. 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.[6] Second, but related, the purpose of He *et al.* (2020) is not to disentangle the competitiveness and ambient effects.

More broadly, our paper relates to three areas of literature. The first is the literature estimating the effects of air quality regulations on competitiveness, in particular Greenstone *et al.* (2012). As that paper notes, there is little other empirical evidence concerning the competitiveness effect of air pollution regulations on productivity

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[4] In Chinese, the regulation is named "大气污染防治重点城市划定方案."
[5] Control firms in close proximity to treatment-region borders also experience ambient effects as we quantify later.
[6] Firms immediately upstream of a monitoring station might or might not experience an "ambient" effect. 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.

except for specific industries (Gollop and Roberts, 1983; Ryan, 2012). We contribute to this area of literature by providing a method to isolate the competitiveness effect from the ambient effect in an air pollution context.

The second area is the literature 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 framework for decomposing the combined effect into 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.

Third, 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 as the combined effect masks variation because there are two underlying contributions that are being averaged. The competitiveness effect applies to firms subject to a regulation while the ambient effect will be experienced by other firms depending on their density and proximity to regulated regions.

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

2. Conceptual framework

Our 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)[7] i produces a product according to a constant-returns-to-scale Cobb-Douglas production function employing $\widetilde{L}$ units of labor and $\widetilde{K}$ units of capital:

$$Q_i = A_i \widetilde{L}_i^{\alpha} \widetilde{K}_i^{1-\alpha}, \ (1)$$

where Q is the firm's output and A is a Hicks-neutral technology shifter. $\widetilde{L}$ and $\widetilde{K}$ are production-effective labor and capital – the quantity actually used in production.

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

Observed units of the two inputs (L and K) may differ because regulation may require firms to employ ineffective inputs in the production process such as compliance officers or pollution-reduction equipment. Observed units are related to effective units by:

$$\tilde{L}_i = \lambda_L(r,\Omega)L_i \text{ (2a)}$$
$$\tilde{K}_i = \lambda_K(r,\Omega)K_i, \text{ (2b)}$$

where λ_L and λ_K are proportionality factors that reflect 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 and λ_K is the competitiveness effect which could be positive or negative: $\partial\lambda_L/\partial r \leq\geq 0$ and $\partial\lambda_K/\partial r \leq\geq 0$. 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: $\partial\lambda_L/\partial\Omega \leq 0$ and $\partial\lambda_K/\partial\Omega \leq 0$. To determine the effects on productivity, substitute into the production function:

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

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

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

Taking the derivative of logged TFP with respect to r gives the combined effect of regulation on TFP:

$$\frac{\partial ln(TFP_i)}{\partial r} = \left[\alpha\frac{\partial ln(\lambda_L)}{\partial r} + (1-\alpha)\frac{\partial ln(\lambda_K)}{\partial r}\right] + \left[\alpha\frac{\partial ln(\lambda_L)}{\partial\Omega} + (1-\alpha)\frac{\partial ln(\lambda_K)}{\partial\Omega}\right]\frac{\partial\Omega}{\partial \mathrm{r}}. \text{ (5)}$$

The combined effect equals the effect on firm competitiveness (the first bracketed term) plus the regulation's effect on productivity via ambient pollution (the second bracketed term). To the extent that $\partial\Omega/\partial r < 0$, interpreting the overall estimate as the competitiveness effect will understate it (in absolute value). Our BD-DD approach eliminates the second bracketed term because it is differenced out by comparing firms that are exposed to the same pollution concentration levels (Ω).

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:

$$MC_i = \frac{1}{A_i\lambda_L(r,\Omega)^\alpha\lambda_K(r,\Omega)^{1-\alpha}}\phi w_i^\alpha r_i^{1-\alpha}, \text{ (6)}$$

where ϕ is a constant that depends on α. Marginal cost is decreasing in both λ_L and λ_K 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 robustness to these price effects 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.[8] 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.[9] 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}).[10] 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

[8] See http://www.gov.cn/gongbao/content/2000/content_60224.htm (in Chinese).
[9] A detailed description is at http://www.mee.gov.cn/gkml/zj/wj/200910/t20091022_172141.htm (in Chinese).
[10] 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.[11] 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.[12] 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.[13] 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.

4. Estimation approach

4.1 Overall approach

We first use the DD approach to estimate the combined effect for comparison to the previous literature. We then isolate the competitiveness effect using our BD-DD approach (see Figure 1 for the correspondence between policy effects and estimation). The difference between these estimates equals the ambient effect. To illustrate our approach, consider four firms in two cities A and B (Figure 3). City A is subject to the KCAPC policy while city B is not. A DD estimate comparing firms 2 and 4 quantifies the combined effect (competitiveness plus ambient effect). Firm 2

[11] 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.
[12] 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).
[13] 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.

suffers from the competitiveness effect but also enjoys the ambient effect, while Firm 4 experiences neither given its far distance from the treatment city.[14]

[Insert Figure 3 here]

A BD-DD estimate comparing firms 1 and 3 isolates the competitiveness effect. Firm 3 enjoys the ambient effect because it is close to the boundary of 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 DD and BD-DD estimates equals the ambient effect.

We next describe the econometric model corresponding to the extant DD approach which estimates the combined effect. We then describe the econometric model for the BD-DD approach for isolating the competitiveness effect.

4.2 Combined effect (DD estimation)

Previous estimates of the effects of air pollution regulations on productivity utilize a DD approach with regulated firms as the treatment group and unregulated as the control group. We use this same approach to estimate the combined effect of KCAPC on productivity. For this estimation we include all firms in the sample that have data in at least one year before the policy and at least one year after:[15]

$$log(Productivity_{it}) = \beta^{CO} Post2003_t * KCAPC_{ct} + \eta_i^{CO} + \theta^{CO} X_{it} + \varepsilon_{it}^{CO}, \quad (7)$$

where i indicates firm, t indicates year, and c indicates city and we index the parameters by CO to indicate combined effect. $Productivity_{it}$ is firm i's productivity in year t. The firm fixed effects (η_i^{CO}) capture time-persistent firm characteristics that affect productivity so that the combined effect is identified from inter-temporal variation within firms.[16] X_{it} includes fixed effects which vary by specification (region-by-year or province-by-year and industry-by-year) and in some specifications weather controls. The region- or province-by-year fixed effects control for geographic-specific unobservables within a year and the industry-by-year fixed

[14] Previous DD estimates, and ours, include firms 1 and 3 in this estimation even though they are not separated far enough geographically to experience the difference in pollution concentrations brought about by regulation. They likely affect the estimates modestly because samples usually involve large numbers of firms so that their influence is small in relative terms. Footnote 41 provides a back-of-the-envelope quantification of this in our setting.

[15] We do so because firms that appear only before or after do not contribute to identifying the policy effects (firm fixed effects absorb their effects) and we want the summary statistics to reflect only data that aids in identification. As we discuss later this is crucial in interpreting the average policy effect across all firms.

[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 since they would be nearly collinear with the firm fixed effects.

effects control for industry-specific unobservables within a year that affect productivity.[17]

ε_{it}^{CO} captures firm-year specific shocks to productivity. In our baseline estimates we follow Greenstone *et al.* (2012) in clustering the standard errors by city-year to allow for spatial correlation across firms within a city-year, but examine the robustness to clustering at the city level which allows for correlations across firms and over time within a city.

The key variables are the two indicators. $Post2003_t$ is set equal to zero prior to the imposition of the KCAPC and one after. It captures the pre- versus post-policy periods. $KCAPC_{ct}$ is set to one if the city in which firm i is located is regulated under KCAPC and zero otherwise. β^{CO} captures the combined effect of the KCAPC policy on productivity – the differential effect of the policy on firms subject to its provisions versus those not.

4.3 Competitiveness effect (BD-DD estimation)

To isolate the competitiveness effect, we embed this DD approach within a boundary discontinuity (BD) design that matches firms of opposite types (regulated versus unregulated) that are geographically close to each other. In sufficiently close proximity, the two types of firms are exposed to the same ambient pollution concentrations but only those in regulated areas must incur costs to comply with the KCAPC. This estimation exploits the spatial discontinuity in regulations between treatment and control cities to estimate the causal effect of regulation on firm competitiveness. The BD-DD subsample includes all firms of opposite types that are sufficiently close that they experience the same pollution concentrations. Specifically, we estimate Equation (7) but restrict the sample to treatment and control firm pairs that are in close proximity ($i \in \{BD\}$):

$$log(Productivity_{it}) = \beta^C Post2003_t * KCAPC_{ct} + \eta_i^C + \theta^C X_{it} + \varepsilon_{it}^C, i \in \{BD\}. \quad (8)$$

β^C captures the competitiveness effect of the KCAPC policy on productivity: the differential effect of the policy on firms subject to its provisions versus those not but facing the same ambient pollution reduction due to the policy.

The BD aspect of our BD-DD estimation differs slightly from the typical approach, which would compare outcomes for all firms within a certain distance on either side of a physical boundary between treatment and control areas. Doing so would include many firms that do not have a corresponding firm of the opposite type (control versus treatment) in close enough proximity that they face similar ambient

[17] We divide China into seven geographic regions (North, Northeast, Northwest, East, Central, South, and Southwest based on the first digit of the Administrative Division Code as in Zhang *et al.* (2018).

pollution levels. While including these firms would not bias our estimates it would add noise and reduce efficiency. To increase the power of our estimates, we include only firms that have another firm of the opposite type within a maximum distance. Since firms rarely move in the sample (fewer than 0.7% of observations) and both firms in a pair must appear before and after the policy, we base the pairs on the closest firm over all sample years.

To illustrate, suppose a treatment firm (A) has a control firm (B) located twelve kilometers away. If we impose a maximum distance cutoff of ten kilometers we would not include data for the A-B pair in estimation, while if we impose a maximum distance cutoff of twenty kilometers we would. However, firm A might be four kilometers from the border and firm B eight kilometers. This is also why we do not apply an RD approach. For each pair, we must include both the treatment and control firm; however, they are not necessarily equidistant from the border making it impossible to define a unique distance. An advantage of our approach is that it can be applied in settings in which regulations are applied to some but not all firms within the same geographic jurisdiction even if there is no defined boundary.

4.4 Illustrative example

To illustrate our estimation approach, consider a simple example. Suppose that a policy reduces pollution concentration by 5.0% and imposes a competitiveness effect of -6.0%. Further, assume that the pollution gradient is 0.05% per kilometer (i.e., pollution drifts such that the pollution reduction diminishes by 0.05% per kilometer as you move away from a treatment area) and an elasticity of productivity with respect to pollution of -0.5. Figure 4a illustrates the policy's effect on pollution concentrations as a function of the distance from the boundary between a treatment and control region (with negative distances representing moving further into the treatment and positive further into the control region) assuming a dense population of firms on both sides. The blue-dashed line shows the pollution reduction due to the policy. Treatment firms reduce their emissions such that pollution concentrations decline uniformly by 5.0% in response to the policy while those in the control region do not reduce their emissions. In the control region, the further from the border (more positive distances) the lower the pollution reduction because the spillover declines with distance according to a gradient of 0.05% per kilometer. The ambient pollution effect reaches zero at 100 kilometers into the control region.

[Insert Figure 4a here]

The green solid line in Figure 4a shows the productivity change due to the pollution reduction (the ambient effect). Applying the elasticity, productivity improves uniformly by 2.5% in the treatment region. In the control region, the ambient productivity effect lessens as you move further from the border (at a rate of 0.025% per kilometer) as the strength of the pollution spillover declines, hitting zero at 100 kilometers.

Figure 4b shows the combined effect. The solid green line replicates the ambient effect as a function of distance from Figure 4a (but rescaled). The small-dashed red line shows the competitiveness effect of -6.0%. Only firms in the treatment region suffer from the competitiveness effect so it jumps discontinuously from -6.0% to 0% at the boundary.

[Insert Figure 4b here]

The long-dashed black line shows the combined effect (it coincides with the ambient effect in the control region) which is what is observed in the data. The discontinuous jump at the border equals the competitiveness effect (-6.0%). The combined effect hits zero at 100 kilometers since both the competitiveness and ambient effects are zero beyond this. BD-DD estimates based on firms in close proximity to the border will yield an unbiased estimate of the competitiveness effect. DD estimates using data outside 100 kilometers from the border will equal the average combined effect in the treatment region (-3.5%) minus the average combined effect in the control region (zero) or -3.5%. This estimate differs from the BD-DD estimates by 2.5% which is the average ambient effect. If the DD estimates include all firms then the estimate will yield the average policy effect including the ambient effect for control firms. If not all firms are included or not all identify the DD estimates (for example, some firms are not present both before and after the policy), this will not equal the average effect unless the included data is proportional to the number of treatment and control firms (including their position relative to the border) in the full data.

5. Data

Our estimation combines data on firm productivity, 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)[18] 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

[18] A 2022 exchange rate of 6.7 is used throughout the paper.

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 provide evidence that our estimates are unlikely to be greatly affected by this threshold when we present our results. We also 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).[19] 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; however, we check robustness to including these. Each firm is classified in an industry using the Chinese Industry Classification (CIC) code.[20]

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. For the BD-DD analysis, 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 BD-DD 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 productivity measures. Our primary measures are TFP estimated using the OP (Olley and Pakes, 1996) and LP (Levinsohn and Petrin, 2003) methods and intermediate inputs as an instrument. We also check the robustness to labor productivity (output per worker) since this is commonly used in the environmental literature. 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

[19] 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.

[20] 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.

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 suggestive evidence when we discuss our results that margins are unaffected 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.

Appendix D provides summary statistics for the DD sample which includes 87,933 firms and 541,887 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). As we discuss later, this distinction is crucial in calculating the average policy effect across all firms. The three productivity measures reveal significant variation and are highly correlated with each other.[21] Appendix E provides summary statistics for the data used in our BD-DD estimation applying a maximum distance of ten kilometers between treatment and control firms (our preferred distance threshold). Again, this reflects only firms that appear before and after the policy change. This sample includes 35,398 unique firms and 224,334 firm-year observations or 6.3 years of data per firm on average.

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 and to check whether pollution is similar on the treatment versus control sides of the borders in our BD-DD estimation. The first is firm-specific SO_2 emissions from the Annual Environmental Survey of Polluting Firms (AESPF) of China.[22] The second is $PM_{2.5}$ concentrations. Although the KCAPC policy did not directly regulate $PM_{2.5}$, it is the only pollution concentration measure which is nationwide in coverage and of sufficient geographic specificity. Different air pollution concentrations are highly correlated so that results for it provide indirect evidence for other pollutants. $PM_{2.5}$ annual concentrations are derived from satellite-based Aerosol Optical Depth (AOD) retrieval techniques maintained by the National Aeronautics and Space

[21] The Pearson correlation coefficients for the TFP OP and LP measures is 0.79 and for labor productivity with respect to TFP OP and LP measures is 0.67 and 0.69, all significant at better than the 1% level.
[22] The AESPF includes 85% of total emissions volume. The survey covers many different environmental measures but we focus on SO_2 because the KCAPC directly regulates it.

Administration (NASA).[23] 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 which we aggregate to the city-level by averaging across grids.

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 of China.[24] 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 confirm that the KCAPC policy had a significant effect on pollution before estimating its combined effect. We then estimate the competitiveness effect and back out the ambient effect. We discuss identification of each as we proceed.

6.1 Pollution effect

A necessary condition for the KCAPC to exert an ambient effect is that it significantly reduced pollution. To see if this is the case, we estimate Equation (7) replacing productivity with two different pollution measures. Returning to our earlier illustrative example in Figure 4a, this estimates the difference of the average pollution reduction (blue-dashed curve) to the left of the boundary relative to the right. Columns (1) and (2) of Table 1 shows the DD estimates using log ambient $PM_{2.5}$ concentration as the dependent variable. The unit of observation is a city-year.[25] Both columns include city fixed effects which capture time-invariant city characteristics that affect pollution and year fixed effects that capture annual factors

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[23] 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).

[24] Available at http://data.cma.cn (in Chinese).

[25] This analysis includes 113 treatment and 148 control cities with ten years of data each. $PM_{2.5}$ data is not available for all cities because the geographic definition of cities changed over time and we are only able to access pollution data that defines cities as of the year 2000.

affecting pollution in all cities. Standard errors are clustered at the city level to allow for arbitrary correlations among unobservables affecting pollution over time within a city. Column 2 includes the weather controls while Column 1 does not. The KCAPC reduced $PM_{2.5}$ concentrations by 3.8 to 4.1% in treatment cities relative to the control cities before versus after the policy.

[Insert Table 1 here]

Columns (3) and (4) estimate the same equation but use firm-year data on SO_2 emissions. Both columns include firm fixed effects to capture time-persistent firm factors affecting emissions and province-by-year and four-digit industry sector-by-year fixed effects to capture time-varying geographic and industry factors affecting emissions. Standard errors are clustered at the city-year level to allow for correlations across firms within a city-year. Column (4) includes weather controls while Column (3) does not. The KCAPC policy leads to a 17.8 to 17.9% decline in emissions for treatment relative to control firms. Appendix F tests the parallel trends assumptions necessary for identifying these effects: event studies estimated by substituting year dummies (normalized to zero in 2002) for $Post2003$ in Equation (7). The figures confirm that both pollution measures follow similar trends for control and treatment groups in the three years prior to the KCAPC implementation, but afterward pollution drops discontinuously for the treatment group.

6.2 Combined effect

To estimate the combined effect we employ DD estimation using the three productivity measures as dependent variables and including different combinations of fixed effects. It is useful to compare our specification to that in Greenstone *et al.* (2012) as it relates to the sources of variation in the two settings. The CAAA and the KCAPC both 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 CAAA generated additional variation which we do not have available. Under the CAAA, only plants that were major emitters of pollution were subject to regulation, allowing controls for time-specific shocks to productivity within counties. Since we do not have intra-city variation, we must rely on province-by-year or region-by-year fixed effects. The CAAA also offers additional time-series variation as counties could move in and out of regulatory status, while in our setting cities retained the same status throughout the post-policy period. Nonetheless, the identification conditions for our DD estimates are met and the combined effect is precisely estimated.

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

5 shows coefficients and 95% confidence intervals for event studies (substituting year dummies for $Post2003$ in Equation (7)). The interaction terms (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 5 here]

Table 2 shows estimates of the combined effect (β^{CO} coefficient in Equation (7)). All specifications include firm fixed effects while Columns 1 through 3 use region-by-year fixed effects and Columns 4 through 6 province-by-year fixed effects. Industry-by-year fixed effects at the two-digit level are included in Columns 1 and 4, at the three-digit level in Columns 2 and 5, and at the four-digit level in Columns 3 and 6.[26] The results are very significant and fairly consistent across specifications. This stability implies that while these factors may determine productivity, they are uncorrelated with treatment status. In the most saturated model (Column 6), the KCAPC policy reduces TFP as measured by the OP method by 3.4%, TFP as measured by the LP method by 4.1%, and labor productivity by 3.9%. We use the midpoint of the TFP OP and LP measures (3.75%) as our headline result.

[Insert Table 2 here]

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 suggestive evidence of whether this is the case, we follow Greenstone *et al.* (2012) and focus on ready-mixed concrete (SIC code 3121). As that paper notes, ready-mixed concrete is homogeneous allowing for the computation of prices without performing quality adjustments and its ubiquity and high transport costs ensure many local markets to provide significant variation. We obtain annual physical units of ready-mixed concrete produced by each firm from the NBS' Production Survey and combine this with the annual revenue data per firm to compute a price per unit. Given this data, the effects of the KCAPC policy on revenue-based TFP can be decomposed into the effects on log price and log quantity-based TFP.[27]

The top panel of Appendix G shows the results of estimating the combined effect (Equation (7)) using the OP method. The KCAPC policy reduces revenue-based TFP

[26] There are 30 two-digit, 162 three-digit, and 425 four-digit industry codes.
[27] This follows because $\ln(revenue) = \ln(quantity) + \ln(price)$ and the inputs for both measures of TFP are the same.

by 7.2% for ready-mixed concrete – a higher effect than for manufacturing in general. Column (2) shows that the effect on log price per unit is small (1.7%) and not significantly different than zero. Column (3) shows that the policy reduces physical-quantity TFP by 8.9%, which differs from the revenue-based effects by the insignificant effect on price. Thus, we do not find significant price effects for ready-mixed concrete and the effects on revenue-based TFP are similar to those for quantity-based TFP.

Greenstone *et al.* (2012) find significant effects of the CAAA policy on prices in the US. Why might our results differ? One possibility is that China's denser population[28] might lead to more firms per market, vis-à-vis minimum efficient scale, so that market power is lower than in the US. Wang and Whalley (2017) find some evidence of this in comparing industry concentration measures between the two countries.

Survival selection bias

Firms that experience larger negative combined effects may be more likely to exit. If so, estimates of the combined effect will 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 first examine whether individual firm exit is affected by the KCAPC policy. Column (1) of Appendix H estimates Equation (7) with an indicator variable set to one if a firm leaves the sample (either because it exits or falls below the threshold) between years $t-1$ and t, and zero otherwise, as a dependent variable. The regression includes the same control variables as in the headline estimates. The KCAPC policy has an insignificant effect on the probability that a firm exits. Since this regression cannot utilize data on firms that leave pre-policy in identifying the effects of the control variables, we also estimate a DD regression at the city-year level:

$$Y_{ct} = \gamma^{CO} Post2003_t * KCAPC_{ct} + \rho_c^{CO} + \delta_t^{CO} + \epsilon_{it}^{CO}, \quad (9)$$

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

Column (2) of Appendix H estimates Equation (9) with firms leaving the sample as a fraction of all firms in the city-year. The point estimate is close to zero and

[28] According to "World Population Prospects 2019" (United Nations), China's population is, on average, four times denser than the US.

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 (3) 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 (4) 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 results but omits firms that exited after the KCAPC policy went into effect. Column (1) of Appendix I repeats the baseline results while Column (2) presents estimates excluding firms that either exited or fell below the CNY 5 million threshold between 2004 and 2007. The estimates are very similar to the baseline results consistent with the KCAPC policy not having an appreciable effect on firm survival.

Greenstone *et al.* (2012) find significant effects of the CAAA policy on firm survival. Why might our results differ? We cannot say for certain but one possibility is that China's economy is growing rapidly during our sample period. This growth may have been rapid enough that the KCAPC policy did not appreciably affect firm exit.

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 above this threshold. To see if our estimates might be affected by either, Column (5) of Appendix H estimates Equation (9) with firms appearing as a fraction of all firms in the city-year as the dependent variable.[29] The estimate is close to zero and insignificant consistent with the KCAPC policy not having an appreciable effect on firm entry or threshold crossings.

Robustness

We re-estimated clustering the standard errors at the city level to allow for arbitrary correlations across firms and over time within a city. The results are shown in Column (2) of Appendix J (top panel) compared to the baseline results in Column (1). As in Greenstone *et al.* (2012), this more general level of clustering results in less significant results. The significance levels are 14% for TFP OP, 9% for TFP LP, and 12% for labor productivity. Column (3) re-estimates weighting observations by firm value added in each year. The results are fairly similar to the baseline results except that the OP measure of TFP loses some significance. Column (4) weights instead by firm employment in each year. The results are somewhat greater in absolute value

[29] We cannot estimate the effect of the policy on entry at the firm level because a firm must appear both before and after the policy to identify the treatment effect.

consistent with larger firms experiencing larger combined effects. Column (5) adds the weather control variables which produces very similar results to the baseline.

6.3 Competitiveness effect

Since inferring the competitiveness effect combines BD and DD estimation, there are three separate identification conditions. First, the BD aspect of the estimation requires that the treatment and control firms are close enough to each other that they experience the same ambient pollution before and after the policy change. The relevant question for determining this is how far the regulated pollutants disperse so that firms in that proximity experience the same pollution levels. SO_2 pollution can travel hundreds of kilometers (Fisher, 1975), as does O_3,[30] NO_X (EPA, 1999: 5), and PM_{10} (EPA, 1996: IV-6 and IV-7). As another point of reference, Chen *et al.* (2013) and Ebenstein *et al.* (2017) both apply a BD analysis to the Huai River policy measuring pollution in one-degree buckets. This corresponds to about 100 kilometers distance.[31] Our preferred estimates use a ten-kilometer distance, which is well below these distances. As an additional check, Appendix K compares the $PM_{2.5}$ pollution concentrations in each year at maximum distances of one, ten, and fifty kilometers on each side of the borders between treatment and control regions. It shows that pollution is very similar on both sides of the border at maximum distances of 1 and 10 but not 50 kilometers.[32]

Second, the DD aspect of the estimation requires that the pre-existing trends in productivity for the control and treatment groups are parallel prior to the policy. Figure 6 shows coefficients and 95% confidence intervals for event studies (substituting year dummies for $Post2003$ in Equation (8)). The interaction terms (normalized to zero in 2002) 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 for all three productivity measures.

[Insert Figure 6 here]

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. 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;

[30] "What is Ozone?" (EPA) at https://www.epa.gov/ozone-pollution-and-your-patients-health/what-ozone, accessed on August 4, 2022.

[31] The Huai River is located from 111°55' to 121°25' East longitude and from 30°55' to 36°36' North latitude. Calculating the distance in moving one degree from roughly the middle of these coordinates (115° East longitude and 33° North latitude) yields a distance of about 100 kilometers.

[32] The differences at 10 kilometers are statistically significant given the large amount of data but the magnitude of the differences is small – less than 2.5% in all years.

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 our estimates. To check for this, we test whether SO_2 emissions of firms near the borders of treatment regions respond differently to the KCAPC policy than those inland of a treatment region (the same comparison within control regions is not possible because the ambient effect of the policy changes output, and therefore pollution, in border vis-à-vis inland areas of the control regions).

Appendix L shows the results from estimating Equation (7) 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 border and zero otherwise.[33] The KCAPC policy significantly reduces emissions by 15% to 18%. However, the policy has no differential effect on firms near the border relative to those inland in treatment regions, consistent with no manipulation in response to the policy. This is in contrast with results for water pollution; perhaps because air pollutants travel quite far and make such manipulation more difficult.

Table 3 shows estimates of the competitiveness effect from the KCAPC policy. For this estimation, we use the most demanding fixed-effects specification including firm, province-by-year, and 4-digit industry code-by-year (corresponding to Column 6 in Table 2 for the DD estimates). The table shows different maximum distances between treatment and control firm pairs. There is a tradeoff as the distance increases. On the one hand, there is more data available to provide precision. On the other hand, the identification requirement that the treatment and control firms face the same ambient pollution is less likely to be met. The estimates are very significant except for the labor productivity measure at one kilometer.

[Insert Table 3 here]

We use the ten-kilometer estimates for our baseline since it is the shortest maximum distance that yields enough data to generate results significant at the 1% level for all three productivity measures. Again, using the average of the OP and LP TFP estimates as our headline result, the competitiveness effect of the KCAPC is a 6.40% decline in TFP. The annual competitiveness effect is equivalent to stalling TFP growth by 3.0 years.[34]

[33] Appendix F shows the parallel trends tests for the SO_2 emissions measure.

[34] Annual TFP growth over the sample period is 2.1% using the average of the annual growth rates for TFP calculated using the OP and LP methods.

Price effects

The bottom panel of Appendix G shows the results of estimating the competitiveness effect (Equation (8)) on ready-mix concrete for the three different components of revenue productivity using the OP method. The price effect (Column (2)) is small and insignificant. The competitiveness effect for ready-mixed concrete is 23.4% measured in physical units (Column (3)), which differs from the revenue-based effects by the insignificant effect on price.

Survival selection bias

Appendix I shows robustness of the competitiveness effect estimates to firm survival. Column (3) replicates the preferred baseline estimates while Column (4) re-estimates excluding firms that either exited or fell below the CNY 5 million threshold post policy. The results are similar to the baseline estimates consistent with the KCAPC regulation not having an appreciable effect on firm survival.

Agglomeration effects

Competitiveness effects may be amplified by agglomeration spillovers. 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 BD-DD estimates but the estimates would include both effects.[35] 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 driven by more general labor and technological linkages between firms. To test for agglomeration effects, we follow that paper and assume that the TFP of a firm is affected by the number of proximate firms.[36] We estimate a triple-DD that compares policy effects in border areas with high relative to low densities of control firms controlling for the triple-DD 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 equally, 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 triple-DD with respect to the number of

[35] 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 BD-DD 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 BD-DD sample naturally includes higher density areas. This provides another reason for the robustness check.

[36] We are unable to follow the empirical approach of the paper as we do not have the same input-output, labor sharing, or technology linkage data.

control firms (N_{Ci}) isolates the agglomeration effect if we simultaneously control for the triple-DD with respect to the number of treatment firms (N_{Ti}). Appendix M provides the conceptual model underlying this estimation.

We modify Equation (7) to add these triple interaction terms to the BD-DD estimation:

$$log(Productivity_{it}) = Post2003_t * KCAPC_{ct}[\beta^{CO} + \beta^{NT} log(N_{Ti}) + \beta^{NC} log(N_{Ci})] + \gamma^T Post2003_t * log(N_{Ti}) + \gamma^C Post2003_t * log(N_{Ci}) + \eta_i^{CO} + \theta^{CO} X_{it} + \varepsilon_{it}^N. \quad (10)$$

β^{CO} captures the competitiveness effect for a firm with no proximate firms near it (i.e., $N_i = N_{Ti} + N_{Ci} = 0$).[37] β^{NT} controls for the amplification of ambient effects in response to the policy due to the number of treatment firms. β^{NC} is the main coefficient of interest and captures any agglomeration effect changes in response to the policy, which could be positive or negative. γ^T and γ^C control for any differential effects due to the density of treatment and control forms respectively before versus after the policy.[38]

Appendix N shows the results for each of the productivity measures using ten kilometers as the maximum distance between paired treatment and control firms, and setting N_{Ci} and N_{Ti} equal to the number of firms of each type within a ten-kilometer radius of the firm. There is a lot of variation in the number of proximate firms (mean of 570.7 with a standard deviation of 438.3). For all three measures, the treatment effect does not vary 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 consistent with agglomeration effects amplifying the competitiveness effect convexly (see Appendix M).

Robustness

We re-estimated clustering the standard errors at the city level to allow for arbitrary correlations across firms and over time within cities. The results are shown in Column 2 of Appendix J (lower panel) compared to the baseline results in Column 1. This more general level of clustering reduces the significance of the coefficients although the TFP OP and TFP LP results remain significant at the 10% and 5% levels. Column 3 weights observations by firm value added. The results are fairly similar and remain significant. Weighting by employment in the firm-year (Column 4) increases the coefficients somewhat in absolute value consistent with somewhat

[37] This is an out-of-sample prediction since the BD-DD estimation requires at least one nearby firm.
[38] Since we choose the closest firm of the opposite type over the whole sample period, an interaction of firm densities with the treatment dummy is absorbed by the firm fixed effects.

greater effects for large firms. Column 5 adds the weather controls. The results are fairly similar to the baseline results.

Falsification tests

Table 4 extends the maximum distance between treatment and control firm pairs in 20-kilometer increments from 20 to 100 kilometers and shows that the ambient effect confounds the competitiveness effect at far distances. As the maximum distance increases, the point estimates become monotonically less negative. This is because the competitiveness effect remains the same as the distance increases (treatment and control firms are still being compared) but the firms no longer face the same ambient pollution. As the distance increases, included firms in the control areas benefit less and less from the positive ambient spillovers as in the illustrative example in Figure 4b (large-dashed line). At a great enough distance, the estimate is equal to the combined effect since the control firms are far enough from the border to enjoy none of the ambient effect. This appears to occur above around 80 kilometers.

[Insert Table 4 here]

Appendix O performs a placebo test by randomly choosing 113 of the 338 cities and estimating the baseline model (Column (3) of Table 3) assuming these are the treatment cities and all other cities are the controls. Panel (a) plots the coefficients from repeating this 500 times using the TFP OP measure. The distribution is centered on zero and only 6.8% of the coefficients exceed (in absolute value) the baseline estimate. Panels (b) and (c) show similar results for the TFP LP and labor productivity measures with 2.4% and 13.6% of the distribution exceeding the baseline estimates, respectively.

6.4 Ambient effect

KCAPC's ambient effect on productivity in the treatment cities is the difference between the combined and competitiveness effects. To quantify the uncertainty in this estimate we perform a block bootstrap of 500 iterations that allows for clustering at the city-year level and accounts for estimation error across the estimates for both the combined and competitiveness effects.[39] Our headline estimate of the competitiveness effect is -6.40% and of the combined effect is -3.75%. This implies an ambient pollution effect in the treatment cities of 2.65% and the bootstrap standard error is 0.014 (significant at the 6.3% level).[40]

[39] At each iteration we draw (with replacement) a block bootstrap by city-year and estimate the combined and competitiveness effects. The standard deviation of the difference of these estimates across all iterations is the standard error.

[40] The estimates with bootstrap standard errors are 0.027 (0.014) for TFP using the LP method (significant at the 5.4% level) and 0.017 (0.015) using the labor productivity measure (insignificant).

The reasonableness of this calculation depends on treatment firms in the BD-DD subsample being similar to treatment firms in the overall DD sample. Appendix P makes this comparison. Column 1 provides the mean characteristics for firms in treatment cities used in the DD estimation and Column 2 for those used in the BD-DD estimation (applying a maximum distance of ten kilometers for treatment-control pairs). Column 3 tests for the difference between the two means. Although many of the characteristics are statistically significantly different from each other in the two samples, the magnitude of the differences is not large (no more than 8.7%).

We can combine the ambient effect estimate with our DD estimate of KCAPC's effect on $PM_{2.5}$ to obtain an elasticity of productivity with respect to ambient pollution. KCAPC reduced $PM_{2.5}$ by 4.1% (using the estimates with weather controls) implying an elasticity of -0.65 for our headline estimates. This is higher than the -0.28 estimate obtained in Fu *et al.* (2021). A possible reason is that the current estimate applies to 2003 while that in Fu *et al.* (2021) is an average across all years from 1998 to 2007.

6.5 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. If combined effects are identified from data on all firms then DD estimates provide a consistent estimate of the regulation's average policy effect on all firms. In our setting, as in many others, not all firms are included. A key constraint is that only firms present before and after the policy identify the DD effects. Therefore, the combined effect identified from this subset of firms cannot be directly applied to arrive at the average policy effect for all firms unless the mix of regulated and unregulated firms (and the geographic placement of the latter relative to the treatment regions) is the same in the full sample as in the sub-sample. We illustrate how the decomposition into competitiveness and ambient effects allows the average policy effects to be calculated in these circumstances using our setting as an example.

There are three components: First, the cost of the competitiveness effect to the 194,228 regulated firms in the full sample. Applying the estimated competitiveness effect (-6.40%) and average value added per regulated firm (CNY 12.667 million), yields a cost of CNY 157.5 (USD 23.5) billion annually. Second, is the benefit of the ambient effect (2.65%) applied to these regulated firms: CNY 65.2 (USD 9.7) billion annually. The third component is the ambient effect on proximate control firms. To roughly approximate this, we use the fact that our falsification test (Table 4) indicates that the ambient effects subside at about 80 kilometers from the nearest treatment firm. We identify the number of control firms in buckets of five kilometers up to a distance of 80 kilometers from the nearest treatment firm and assume that the ambient effects decline linearly from 2.65% in the five-kilometer bucket to zero in the

80-kilometer bucket.[41] There are a total of 105,062 control firms in all buckets with an ambient benefit of CNY 22.3 (USD 3.3) billion annually.[42] The three components imply a net policy cost of CNY 69.9 billion (USD 10.4) billion annually. This is quite different from naïvely applying the estimated combined effect (3.75%) to the CNY 12.177 million average value added of all firms in the sample (CNY 142.2, USD 21.2 billion annually).

6.6 Alternative specification based on distance

An alternative approach to identifying the competitiveness effect is to include data further from the boundary and include a measure of distance to the nearest firm of the opposite type (control versus treatment). The ambient effect declines with distance into a control city while the competitiveness effect is invariant to distance. Therefore, allowing the productivity to vary with distance forms a triple-differences estimator. For example, refer to the illustrative example in Figure 4b. If a control firm is located 20 kilometers from the boundary, the combined effect it experiences would be 2.0% compared to -3.5% for the treatment firms (a difference of -5.5%). On the other hand, if the control firm is located 40 kilometers from the boundary the combined effect it experiences would be 1.5% compared to -3.5% for the treatment firms (a difference of -5.0%). At a distance close to zero, a control firm experiences a combined effect of 2.5% compared to -3.5% for a treatment firm – a difference equal to the competitiveness effect of -6.0%.

Given the competitiveness effect is invariant to distance while the ambient effect is not, the two can be separated by including a policy-treatment interaction (to capture the competitiveness effect) along with a policy-treatment-distance interaction (to capture the changes in ambient effect with distance). The sample is the same as that used in the DD estimation. In order to assign a unique distance for each firm, we use the distance to the nearest firm of the opposite type. Since this is an approximation of the true geospatial relationships we regard these estimates as supporting evidence only. We estimate the following equation:

$$log(Productivity_{it}) = \beta^{D1} * Post2003_t * KCAPC_{ct} + \beta^{D2} * Post2003_t * KCAPC_{ct} * Distance_i$$
$$+\eta_i^D + \theta^D X_{it} + \varepsilon_{it}^D, \ (11)$$

[41] This assumption implies some contamination of the DD estimates as the control group is not completely free of ambient effects. The contamination depends on the number of control firms within each bucket b (N_{Cb}) according to $\frac{1}{F}\sum_{b\in\{5,10,\cdots,80\}}\left[\frac{0.0265(80-b)}{75}N_{Cb}\right]$ where F is the total number of firms in the sample. This follows from our assumption that the ambient effects (0.0265) decline linearly with distance to hit zero at 80 kilometers. This calculation yields 0.0063 (16.8% of the DD estimates).

[42] We use the average value added across all 117,225 control firms in the full sample (CNY 11.293) to calculate this.

where $Distance_i$ is the distance between firm i and its nearest neighbor of the opposite type (control versus treatment). β^{D1} captures the competitiveness effect – the policy effect at a distance of zero. β^{D2} captures the decay of the ambient effect as the firms are further apart. We expect β^{D2} to be positive – the difference between the treatment and control firm productivity becomes less negative as the firms are further from the border because the control firms benefit less and less from the ambient effect while treatment firms continue to enjoy the full ambient effect.[43]

Table 5 shows the results of estimating Equation (11) for the different productivity measures. For ease of reporting, distances are rescaled to hundreds of kilometers. The coefficient on the policy-treatment interaction term is very significant and estimates a somewhat smaller competitiveness effect than that estimated by the BD-DD approach for the OP measure (-5.2% versus -6.0%) as well as for the LP measure (-5.9% versus -6.8%). The coefficient on the policy-treatment-distance interaction term is positive as expected. Since we impose a linear function for the decay of the ambient function and Table 4 indicates that the ambient effect dissipates at about 80 kilometers, Columns (3) and (4) re-estimate restricting the sample to firm pairs within 80 kilometers of each other. The point estimates are somewhat closer to the baseline (-5.3% for the OP and -6.3% for the LP measure).

[Insert Table 5 here]

7. Conclusion

Choosing optimal environmental regulations requires an accurate cost-benefit analysis of their impact. This paper isolates 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 subject to the regulation. Failing to separate these effects understates the private costs to regulated firms and ignores the public benefits to other firms. The 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.

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

[43] A standalone distance term is not included as it would be collinear with the firm fixed effects.

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.

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