CESifo
AREA CONFERENCES 2020

Energy & Climate Economics
Munich, 1–2 October 2020

Intrafirm Leakage

Felix Samy Soliman

CESifo

Intrafirm Leakage

Felix Samy Soliman*

August 2020

Abstract

The environmental regulations US firms are exposed to are often place-based, incentivizing firms to move to less regulated counties or states. Consistent with this argument, multiplant firms partially regulated under the ozone regulations of the US Clean Air Act offset regulation-induced reductions among regulated plants with spillovers to unregulated plants and by moving plants out of regulated areas. Taken together, these leakage effects fully offset emissions reductions at regulated plants. Effects are strongest among highly productive firms and those operating in tradable industries.

1 Introduction

In the context of piecemeal environmental regulation, intrafirm leakage arises if multiplant firms shift emissions from regulated to unregulated plants, or move entire facilities to unregulated jurisdictions. A policy that creates incentives for firms to behave in this way is the US Clean Air Act (CAA). The CAA regulations are only applicable to industrial plants in the most polluted US counties, creating sharp differences in environmental regulation across the US. In light of the strongly negative effects on employment and productivity outcomes at plants under ozone regulations (Greenstone, 2002; Greenstone, List, and Syverson, 2012; Walker, 2011), there are potentially large payoffs for firms to relocate to unregulated areas.

Such relocations have the potential to challenge our understanding of the aggregate welfare effects of place-based environmental regulations like the CAA. On the one hand, emissions leakage can attenuate the overall benefit of the policy by shifting emissions to otherwise less polluted regions. However, if not only emissions, but also manufacturing jobs or entire plants move to unregulated counties, leakage may also lower the nationwide economic costs of the regulation. I will

*Department of Economics, University of Zurich. Many thanks to Alexander Teytelboym. I'd also like to thank Tilman Graff, Apoorv Gupta, Jacquelyn Pless, Fabio Schmidt-Fischbach and seminar participants at INET (Oxford), the EMCON conference at the University of Chicago and EAERE2020 for helpful feedback. E-Mail: felix.samysoliman@uzh.ch.

explore these hypotheses in the context of the largest expansion of the CAA in 2004, when about 200 additional counties became newly regulated for violation of a tightened ozone standard.

I guide the empirical analysis by taking a firm-level perspective on plant production choices. On the intensive margin - defined as emissions leakage between existing plants - leakage may arise if regulation induced cost increases make expansions at unregulated plants profitable. Provided that outputs across plants are substitutes for the consumer, regulation leads to decreases in activity at regulated plants, (partially) offset by increases at other, unregulated plants. Theoretically, this requires firm-level market shares to be partially inelastic with respect to the regulation induced cost increase, for example because consumer preferences give firms some market power. If so, resources shed at regulated plants are not reallocated to *any* firm in the economy, but remain within the firm. Further, I consider the incentive to relocate plants to an unregulated area - the extensive margin. As in the offshoring literature (Antràs and Helpman, 2004), firms trade off lower variable production cost of producing in an unregulated area and the fixed cost of moving there. This trade off crucially depends on firm productivity. The least productive firms exit at regulated plants, while high productivity firms relocate to take advantage of regulation-induced differences in variable cost. More productive firms are larger in size and therefore benefit the most from the variable cost saving potential of an unregulated plant.

To investigate the theoretical predictions, I construct a unique dataset of plant-level emissions from the Toxic Release Inventory (TRI), matched to balance sheet information for listed companies from Compustat. The focus on large, publicly owned companies is motivated by previous research showing that enforcement mostly ignores smaller plants (Becker and Henderson, 2000). Consistent with this hypothesis, I find that privately owned plants are unaffected by the regulation in terms of their emissions behavior. Using a triple difference research design similar to Greenstone (2002), I find that public firms either decrease emissions of volatile organic compounds (VOC), a precursor to ozone, at regulated plants or shut them down altogether.

To test whether these emissions decreases were offset by intrafirm leakage, I first ensure that firms' internal plant network is accurately represented in my dataset. The issue is addressed in the process of matching the plant-level data to Compustat using a state of the art search engine algorithm (Autor, Dorn, Hanson, Pisano, and Shu, forthcoming). Based on the overlap in search engine results across names, the matching algorithm serves to create a systematic and accurate firm boundary within the TRI database. The TRI includes only unreliable parent company identifiers, such that plants may be listed as part of subsidiaries when they in fact belong to larger conglomerates. Thus, I circumvent downward bias in my leakage estimates. Leakage effects are then identified using a research design that compares outcomes at plants in unregulated areas between firms that are regulated elsewhere and firms entirely unaffected by the regulation. Results show that multiplant firms increase emissions at plants in unregulated areas and increase the number of plants they operate in these regions. Adding these effects up, I find that the combination of extensive margin relocations and intensive margin increases at existing, unregulated plants amounts to a net increase in emissions at the firm-level. The balance sheet data allows me to test whether extensive margin responses vary along the dimensions emphasized in the theory. I confirm that only the most productive firms expand their operation in unregulated areas, while low productivity firms exit at regulated plants.

I further investigate to what extent the intensive margin effects are consistent with my conceptual framework. If leakage arises because firms can flexibly meet their demand using output from different plants, only tradable goods producing firms should be observed to shift emissions. For plants in some industries regulated under the CAA, such as cement, transportation costs mean that firms are not flexible in this way (Syverson, 2008). Consistent with this intuition, emissions spillovers to unregulated plants are entirely driven by firms in tradable industries. Additionally, I find that leakage effects are only apparent when regulated plant and leakage candidate operate in the same industry. Both findings support my hypothesis that interrelated demand is driving the leakage estimates.

The results have important, but complex implications for cost-benefit analyses of environmental regulation. Prior literature suggests that my findings on plant relocations, in particular, can have sizable negative effects on housing values and infant health outcomes (Currie, Davis, Greenstone, and Walker, 2015). The redistribution of emissions among plants may similarly erode the policy's health benefits. These costs of intrafirm leakage need to be compared to the benefit of allowing economic activity ro relocate within the US. Going forward, these findings are important to assess the likely effects of recently implemented, further expansions of ozone regulations (Friedman, 2017). By decomposing leakage into its two margins and substantiating the theoretical mechanisms, my findings become useful for policymakers concerned about intrafirm leakage in other contexts, such as state-level initiatives to decrease carbon emissions that are becoming increasingly prevalent in the US.

In many previous studies, the movement of economic activity towards unregulated jurisdictions is estimated using cross-country or regional industry-level aggregates.[1] I emphasize that correctly evaluating the effects of piecemeal environmental regulation necessitates a focus on plant's position within the internal firm network of multiplant firms. In related work, Hanna (2010) provides evidence that firms partially regulated under the CAA increase their FDI activity. Relative to her study, and perhaps unsurprisingly, the shift into unregulated areas documented in this paper is even larger at a national level. The welfare implications of my results may differ from her study in light of the perception of environmental regulations as "job killers" domestically. Colmer, Martin, Muûls, and Wagner (2018) consider within-firm leakage of carbon emissions under the European Trading Scheme (ETS). In this context, firm-level emissions decrease significantly only for firms that are regulated at each of their plants. They argue that this may reflect leakage, but effects lack statistical significance. Finally, I build on important work by Gibson (forthcoming), who shows that multiplant firms targeted under CAA regulation of particulate matter have directed

[1] See Brunnermeier and Levinson (2004) for a survey and Fowlie (2009) for a more recent study of between-firm leakage in the US.

their emissions towards unaffected plants. The current study differs in three aspects from Gibsons's (forthcoming) prior work. First, I find significantly larger leakage rates. Both the focus on particularly disruptive ozone regulations as well as a more accurately drawn firm boundary may account for the difference in effect size. Second, I also consider plant relocations. Thus, I emphasize that intrafirm leakage is not only associated with welfare lowering redistribution of pollution within the US. Rather, I argue that the reallocation of economic activity may lower the aggregate economic costs of this regulation. Third, I also study the mechanisms through which leakage operates.

More broadly, this study is related to recent studies of intrafirm spillovers in response to local economic shocks (Giroud and Mueller, 2015, forthcoming). I add to this literature by documenting a novel channel for within-firm spillovers, that is based on demand, and not financial constraints. Overall, my findings show that a full cost-benefit accounting of environmental regulation is faced with issues recently highlighted in studies on business reallocation caused by cross-state differences in tax policy (Fajgelbaum, Morales, Suárez Serrato, and Zidar, 2018; Giroud and Rauh, forthcoming).

This paper proceeds as follows. In section 2, I discuss whether intrafirm leakage is a plausible outcome of piecemeal regulation. Section 3 gives a brief overview over aspects of ozone regulation relevant to this study. In Section 4, I discuss sources and methods to construct the dataset as well as the research designs to identify direct and leakage effects of ozone regulation. In Section 5, I apply this empirical framework to test the theoretical predictions. In Section 6, I discuss the welfare implications of intrafirm leakage. Section 7 concludes.

2 Conceptual Framework

A substantial body of evidence suggests that ozone regulation under the original and amended CAA decreases plant-level employment, productivity and output (Greenstone, 2002; Greenstone et al., 2012; Walker, 2011). Under what circumstances should we expect firms to offset these regulation induced-downsizings by expanding production at unregulated plants?[2] To a first order, such incentives are small if the optimization problem the firm faces at each plant is independent of its operating decisions elsewhere. This is for example the case in a setting where the firm sells its output in a perfectly competitive market such that the optimal production decisions at each plant are pinned down by each plant's marginal cost. Regulation may increase this cost such that firms decrease their production at regulated plants, but this leaves the optimality condition of unregulated plants within the same firm unchanged.[3] In general equilibrium, the resources set free at the regulated plant may be reallocated to any of the unregulated plants in the economy.

[2] Emissions are thus thought of as a standard input in the firm production function. To offset output losses at regulated plants, firms must increase emissions at unregulated plants.

[3] See Appendix A for discussion why CAA regulation is best thought of as increasing the variable, as opposed to fixed, cost of production in the context of ozone regulation.

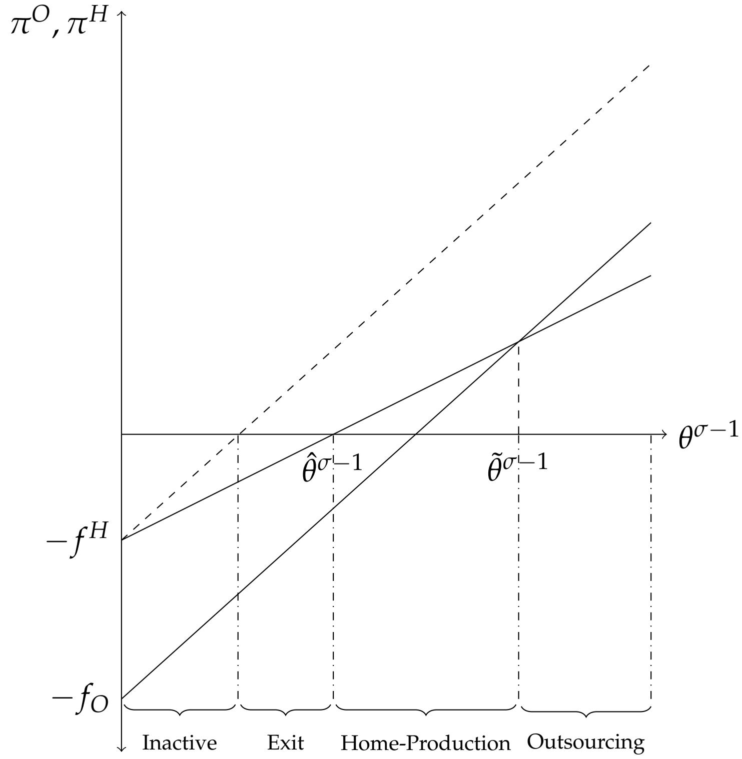
Such reallocation effects would, however, be equally likely to apply to plants within and across firms. Therefore, the perfectly competitive benchmark does not provide a distinct rationale for intrafirm leakage as a likely outcome of piecemeal regulation.

To introduce a partial equilibrium mechanism for why firms may choose to shift production among their plants, Appendix A.1 presents a stylized model of multiplant production. Multiplant firms optimize at each of their plants under imperfect competition in the output market. Optimal output choices are interdependent across plants within the same firm because a decrease in output at a regulated plant will increase marginal revenue at unregulated plants. Higher marginal revenue at unregulated plants implies a profit incentive to expand at unregulated plants. Intuitively, demand that was previously met with production from the regulated plant is now satisfied by unregulated plants. The model described in the appendix shows that intrafirm leakage arises whenever marginal revenue curves are sufficiently steep to offset potentially negative leakage effects arising from complementarities across plants operated by the same firm. Depending on the relative slope of marginal revenue at the regulated and unregulated plants, partial regulation may even increase emissions at the firm-level.

In principle, this partial equilibrium mechanism for intrafirm leakage may also apply to between firm leakage if plant-level output choices are strategic substitutes across firms (Bulow, Geanakoplos, and Klemperer, 1985). Since goods produced by plants within the same firm and industry are likely to be more substitutable than good produced by two competitors, intrafirm leakage may nevertheless be the economically more plausible outcome. As a result of limited substitution by consumers across firms, the market share of the large firms in my sample may be fixed in the short-run. Therefore, a cost shock to one plant primarily leads to within firm adjustments. This framework fits the focus of CAA regulation on manufacturing industries, each producing differentiated varieties. Fowlie (2009), in contrast, considers between firm leakage in the context of electricity, a homogeneous good, such that leakage could equally arise within and between firms. Overall then, this simple model of multiplant production provides an intuitive account as to why unregulated plants within the same firm may increase their emissions under partial regulation.

Regulation induced increases in the variable cost of producing at one plant can also serve as an incentive to open up a plant in a unregulated area of the country. I analyze this possibility in Appendix A.2 by assuming that firms can move plants to unregulated areas by paying an additional fixed cost. I embed this tradeoff in a parameterized version of the general set-up previously outlined, with imperfect competition once again providing a profit incentive for relocation. The relocation is modeled as a trade off between higher variable profits the firm can achieve by escaping the costs of regulation and the fixed cost of moving to an unregulated area. Following Antràs and Helpman (2004), I assume that firms are heterogenous in their productivity. Figure 1 depicts this situation graphically by plotting profits before and after regulation and under different operating modes against a transformation of firm productivity, θ.

Figure 1: FIRM PROFITS: OUTSOURCING VERSUS HOME-PRODUCTION



The dotted line depicts the profit under home-production and in the absence of environmental regulation. The straight line running parallel to it shows the profits under outsourcing. The difference between them reflects the larger fixed cost under outsourcing ($f_O > f_H$). After regulation, profits under home-production shift down. The most productive firms ($\theta > \tilde{\theta}$) engage in large-scale production such that the variable cost savings potential of an unregulated plant easily outweighs the fixed cost. The contrary is true for firms with productivity below $\hat{\theta}$. Unable to relocate, regulation forces these firms to shut-down their operations entirely. Firms at an intermediate level of productivity remain active in a regulated area. The interested reader is referred to Appendix A.2 for further discussion of the model and a precise statement of which firms prefer to move to an unregulated area.

3 Regulatory Background

3.1 Ozone Regulation under the Clean Air Act

The variation in the stringency of environmental regulation I exploit in this paper is provided by the structure of the Clean Air Act (CAA). Title I of the landmark legislation stipulates rules for the regulation of six criteria air pollutants (particulate matter (PM), ozone, sulfur dioxide, lead, carbon monoxide and nitrogen dioxide (NO_x)). According to the regularly updated National Ambient Air Quality Standards (NAAQS), an area is regulated under the CAA for a pollutant if monitor readings in that area exceed the NAAQS thresholds. Counties where such violations are detected are referred to as nonattainment counties and regulated accordingly. Counties can be in nonattainment for several pollutants; the focus in this paper lies on the regulation of ground level ozone and its effects on firm emissions of the ozone precursor volatile organic compounds (VOC).

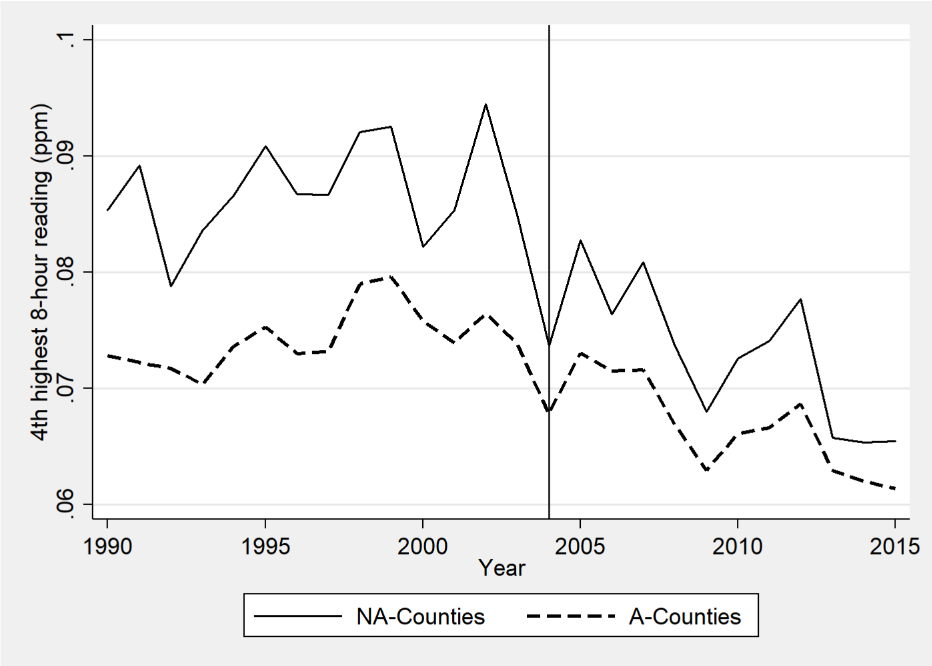
Regulation of ozone has become significantly more stringent since the original passing of the CAA in 1970. Initially, counties were designated as nonattainment areas if the yearly maximum 1-hour reading exceeded 0.12 parts per million (ppm) (EPA, 1997). Henderson (1996) demonstrates that this strong focus on ozone peaks has mainly led to a narrowing of the empirical distribution of ozone readings, without consistently lowering the mean or median of ozone pollution over longer stretches. Since 2004, ozone regulation is determined based on rolling 3-year averages of the fourth highest 8-hour county-level monitor reading. The switch from an 1-hour to an 8-hour rule represents a significant tightening of regulation since, under the old rule, factories merely needed to monitor emissions peaks, while still being able to emit relatively high levels on a consistent basis. The EPA also tightened standards along a second margin by lowering the threshold from 0.12 ppm to 0.08 ppm. The update to ozone standards was decided on in 1997, but implementation could only start in 2001 following the Supreme Court's ruling in *Whitman v. American Trucking Associations* ruling in favor of the EPA having the legal authority to follow up on the proposed change (see Mills (2002) for legal background).

Violation of federal ozone standards remained widespread after the Amendments to the CAA passed in 1990. In total, 520 counties were subject to CAA regulations between 1992 and 2014. To illustrate how counties are designated as nonattainment areas, Figure 2 plots the average monitor reading for the 207 counties that were newly regulated in 2004 over time. This large expansion of the CAA follows the previously described tightening of ozone standards. The straight line ("NA-Counties") shows that in the years before 2004, ozone levels always exceed this threshold. As a consequence of sustained violations of the 8-hour rule based standard, 207 counties were designated nonattainment areas in 2004. For comparison, the dotted line depicts yearly average monitor readings for counties that were unregulated throughout the period from 1992 to 2015. While pollution levels are significantly higher in about to be regulated counties prior to 2004, this gap largely disappears by 2015. For two thirds of the newly regulated counties, the achieved reductions in ozone levels lead them to regain attainment status by 2015. Even for the counties that remain regulated, average monitor readings in 2015 have dropped below 0.07 ppm.[4]

Compared to market based initiatives, such as cap-and-trade programs, the implementation of the regulation more closely resembles a command-and-control regime. Together with the EPA, state authorities develop State Implementations Plans (SIPs) that spell out how the nonattainment areas within the state can effectively reduce pollution levels to bring the violating areas back into attainment. The process leaves some discretion to local regulators over the types of abatement

[4]Based on independent calculation. Counties remain in regulation due to subsequent tightenings implemented under President Obama.

Figure 2: OZONE LEVELS IN REGULATED AND UNREGULATED COUNTIES



Notes: The figure plots the average of the 4th highest 8-hour reading of ozone across two sets of counties. NA-Counties refers to the group of 207 counties that were in attainment in 2003 and are reclassified to nonattainment in 2004. A-Counties are in attainment throughout the period.

procedures plants in their area have to adopt. While details of implementation are developed with regard to its cost to industry (Gibson, forthcoming), the NAAQS determining nonattainment in the first place are not to be revised with reference to the economic cost of regulation (Mills, 2002).

Ozone, not directly emitted by industrial facilities, is the result of ozone precursors VOC and NO_x reacting in the presence of sunlight and heat (EPA, 2014). To understand the effect of ozone regulation on environmental outcomes at the plant-level, I take advantage of detailed panel data on VOC emissions. Cost constraints often lead to regulatory strategies intricately linked to patterns of concentrations of a given pollutant within nonattainment counties (Auffhammer, Bento, and Lowe, 2009; Gibson, forthcoming). As in previous research on ozone regulation, I assume that cost-effective regulation is focused on major emitters within a county (Becker and Henderson, 2000; Greenstone, 2002). Likely reasons for why regulation takes this pattern are discussed in Appendix B.1.

4 Empirical Framework

4.1 Data Construction

Plant-Level Data.— To measure emissions at the plant-level, I obtain publicly available Toxic Release Inventory (TRI) datafiles from 1987-2014. These include detailed information on the chemicals emitted by large industrial plants. According to the EPA, reporting is mandatory for plants i) in any of the 6-digit NAICS industries targeted by the EPA under the TRI programme ii) employing more than ten full time employees and iii) manufacture/process 25,000 pounds or otherwise use more than 10,000 pounds of any of the 600 chemicals covered by the TRI.[5] I use a classification by EPA (2013) that details which of these chemicals is classifiable as VOC and regulated under the CAA.[6] This gives me a panel dataset on plant-level VOC emissions. Plants enter and exit the TRI database over the course of the sample frame. To study the impact of regulation on the extensive margin, i.e. on plant entry and exit behavior, I balance the panel by creating the following dummy variable: In plant-years where the plant is listed in the TRI the dummy takes the value one and zero otherwise.

Both the emissions variable and the dummy variable indicating a plant's operating status could suffer from measurement issues.[7] TRI emissions have been shown to contain measurement error (de Marchi and Hamilton, 2006). Gibson (forthcoming) finds that reported emissions correlate strongly with those listed in the National Emissions Inventory (NEI) dataset. Checks on accuracy are more stringent in the construction of the NEI, implying that TRI reported emissions are substantially related to an accurate measure of emissions. Self-reporting can also bias the estimated effects of regulations if plants aim to please regulators by reporting lower emissions. Emissions decreases following regulation may thus be spurious. While difficult to disprove conclusively, I will closely examine the dynamics of direct treatment and spillover effects. If they go in opposite direction and coincide in timing, this should be interpreted as evidence against strategic reporting at regulated plants. Leakage is unlikely to arise from reporting lower emissions in an attempt to please regulators.

Inferring entry and exit behavior using the TRI data creates related measurement issues. Plants may have been in operation before they are listed in the TRI because they only satisfy the size requirements above which reporting becomes mandatory after several years in operation. Exits

[5] Due to reporting changes in 1990-91, leading to extreme spikes in reported emissions levels, I exclude observations from years prior to 1992. I further limit my analysis to the set of chemicals listed in the TRI data in the first year of my sample. The list of covered chemicals has expanded considerably since 1992. Excluding these chemicals ensures not confounding any estimated effects with changes in reporting requirements. In addition, TRI reporting became mandatory for a number of additional industries in 1998. All results are robust to only focusing on the industries already covered in 1992.

[6] Only some VOC chemicals contribute to ozone levels and are regulated accordingly. An alternative classification is developed by Greenstone (2003). All results reported in this paper are robust to using this alternative concordance. I thank Michael Greenstone for sharing his concordance.

[7] Both criticisms are for instance articulated in Currie et al. (2015). For applications using TRI derived measures of plant openings and closings see Banzhaf and Walsh (2008) and Levine, Lin, and Wang (2018a).

from the TRI dataset can similarly reflect reductions in plant size. The plant opening may also precede entry into the TRI dataset if plants do not immediately comply with reporting requirements. The latter is certainly possible, especially in less closely monitored attainment counties. The empirical analysis nevertheless proceeds under the assumption that entry and exit into the TRI dataset over the course of the sample frame is informative about either plant openings/closings or plants crossing a size threshold. Within each year, I winsorize non-binary dependent variables (emissions and counts of operating plants) at the 2.5th and 97.5th percentile of their empirical distribution to account for outliers.[8]

Match to Compustat and Construction of Internal Firm Network.— Previous research suggests that regulation of ozone is mostly focused on larger plants part of multiplant firms (Becker and Henderson, 2000). Thus, I hypothesize that the impact of regulation is more significant for publicly traded firms than for smaller, privately owned ones. Appendix B.2 contains a discussion to what extent this limits the external validity of my leakage estimates. To establish which firms are listed, I use a matching algorithm to link the plant-level data to Compustat. A main payoff of the particular algorithm I employ is that it not only reveals which firms are listed, but also helps establish a consistent and accurately drawn firm boundary. Otherwise, estimates may be attenuated due to misclassification of plants as stand alone, when they are in fact leakage candidates within a partially regulated conglomerate. The starting point is using the parent company identifiers (DUNS codes) included in the TRI data to identify plants' parent company. There are several inconsistencies in this variable, essentially boiling down to plants within the same company (according to the company's name included in the TRI data) listing what is likely to be an establishment, and not firm-level DUNS code. Plants are recorded as belonging to the same firm if either the identifier from the raw data links them together or they share the same parent company name.

The link to Compustat is constructed in two steps. First, I use simple matching on parent company names. Companies frequently change names, which is problematic given the limited reliability of the firm-level DUNS codes. To address this issue, I use historical name information for publicly listed companies from the `COMPHIST` file from CRSP/Compustat. The file contains current and historical name information for the universe of publicly listed firms. Second, I aim to limit the number of false negatives, i.e. firms that have a true match, but cannot be linked because of spelling differences or because they are in fact subsidiaries of larger, listed companies. To this end, I implement a web-based string matching algorithm pioneered by Autor et al. (forthcoming). Their idea is to leverage the machine-learning capabilities of the search engine Bing.com in producing similar search results for the same firm regardless of the use of acronyms or other name changes. Using scraped URLs as well as those provided for a subset of Compustat firms, I use the exact matching algorithm outlined in Autor et al. (forthcoming). Beyond increasing the sample of matched firms, matching on URLs leads to a considerable number of plants being assigned to the same company that were previously recorded as being part of different parent companies.

[8]Results are robust to using other thresholds or not winsorizing the data.

Inspection of these duplicate matches revealed that the algorithm accurately detects which firms are in fact subsidiaries of larger corporations. Web-based matching also proves to be reliable in that it detects essentially no outright false positives.[9]

The link to Compustat further allows me to add in firm observables, such as productivity and financial constraints. These allow me to test through which channels leakage operates. Table 1 presents correlations between the firm-level variables used in the analysis below. Consistent with prior research, pollution intensive firms, i.e. those with higher (log) levels of emissions over sales, are less productive and more financially constrained (Levine, Lin, Wang, and Xie, 2018b; Shapiro and Walker, 2018). These correlations are relatively small, however.

The data on county-level nonattainment status comes from the EPA Greenbook. Further variables, their sources and methods used in their construction are discussed as they are brought into the analysis.

4.2 Research Design

4.2.1 Estimating Equations

Effects of Air Regulation on Regulated Plants.— To set the stage for the estimation of within-firm leakage, I study whether nonattainment designation for ozone leads plants to decrease their air emissions of VOC. Since ozone regulation varies across counties and, importantly, within counties across years, a natural way of identifying these effects is to exploit these within-county changes using a difference-in-differences research design.

Consistent with prior literature, I assume that regulators only target plants in heavily polluting industries. I follow Greenstone et al. (2012) and classify plants in the industries "Petroleum refining", "Pulp and Paper", "Organic chemicals", "Rubber and miscellaneous plastic products" and "Stone, clay, glass, and concrete" as those likely to be targeted by ozone regulations.[10] About one fifth of the plants in my sample are classified as dirty using this classification. Combining within county changes in nonattainment designations with across industry variation in emissions intensity to determine the effects of environmental regulation leads to the triple difference estimator popularized by Greenstone (2002):

$$D_{ijct} = \delta_i + \delta_t + \beta_1 NAA_{ct-1} + \beta_2 NAA_{ct-1} \times Dirty_j + \epsilon_{ijct}. \tag{1}$$

[9] One complication that did arise among the subset of non-unique matches was the issue of spin-offs and mergers. For each non-unique match, i.e. for each non-unique link from DUNS code to Compustat identifier `gvkey`, I checked the Wikipedia page of the companies involved to look for potential M&A activity. The final matches account for mergers and spin-offs in that these firms carry time-varying parent company identifiers reflecting the timing of acquisitions.

[10] See Table 1 in Greenstone et al. (2012). In robustness checks, I use the more expansive set of industries used in Greenstone (2002). Greenstone et al. (2012) find particularly disruptive effects of ozone nonattainment using this classification. It is therefore adopted to maximize the potential to detect regulatory effects.

The dependent variable, D_{ijct}, is the log of total onsite air emissions of VOC chemicals of plant i in industry j, located in county c, in year t. NAA_{ct} takes the value one in years a county is part of a nonattainment area and zero otherwise, while $Dirty_j$ is an indicator for whether the plant belongs to one of the aforementioned dirty industries. Since nonattainment designations are published in July of year t, I lag this variable by one period. δ_i and δ_t are plant and year fixed effects. In this simple fixed effects set-up, β_1 is identified from within plant changes in air emissions taking place when the county the plant is located in transitions in and out of attainment. β_2 adds a third difference, by further considering differences in this quantity across plants in industries either classified as dirty or clean according to the criterion by Greenstone et al. (2012). Standard errors are clustered at the county-level to adjust for correlation in the residuals within plants in the same county and for autocorrelation between periods.[11]

Intrafirm Leakage Effects.— To test the intensive margin of within-firm leakage I estimate the following difference-in-differences specification for (log) air emissions.[12]

$$D_{ifjt} = \delta_t + \delta_i + \beta_1 other\_treated_{fjt} + \epsilon_{ifjt}. \tag{2}$$

The dependent variable and fixed effects are defined as in equation (1). $other\_treated_{fjt}$ is a dummy equal to one for plants part of a firm f that is exposed to environmental regulation at one of its other plants in year t in the same 5-digit NAICS industry j. Leakage candidates are thus required to be within the same firm-segment fj as the regulated plant. A positive coefficient β_1 would reflect higher emissions among plants whose parent company operates a regulated plant in the same industry.

I test for plant relocation effects by studying whether firms subject to environmental regulations via the CAA at one of their plants are more likely to start up plants in unregulated counties. I organize the panel as a plant-year dataset, listing all VOC-emitting plants that eventually locate in attainment counties. The birth year of a plant is determined by its entry into the Toxic Release Inventory (TRI). For years prior to its birth the plant is recorded as not in operation. Plant-year observations where there are no active plants within the firm-segment are excluded. This is done to avoid confounding the relocations of existing firm-segments with the segment's entry decision. Organizing the data in this way, I regress the plant's operating status on the independent variable used in equation (2).

$$Active_{ifjt} = \delta_t + \delta_i + \beta_1 other\_treated_{fjt} + \epsilon_{ifjt}. \tag{3}$$

The dependent variable $Active_{ifjt}$ is a dummy variable taking the value one in all years a plant is listed in the TRI as an emitter of VOC and zero otherwise. A linear probability framework is

[11] Results are robust to alternative clustering schemes, such as two-way clustering at county and four-digit NAICS-level, or clustering at the county-polluting industry-level (i.e. including two clusters per county).

[12] This specification for intrafirm spillovers closely matches those used by Giroud and Mueller (2015) and Gibson (forthcoming).

chosen since the inclusion of a large number of fixed effects leads to well known biases in non-linear probability models such as the probit. The coefficient β_1 indicates whether plants part of firms regulated elsewhere have a higher probability to start operating/remain active. Results thus not only reflect plant openings, but also the higher value partially regulated firms may place on continued operation of unregulated plants. This is sensible here because firms operating a regulated plant, on average, already own an unregulated plant. The firm can use such plants to outsource particularly pollution intensive production steps only feasible under a lenient regulatory regime.

I impose the restriction on the estimation sample that leakage candidates are never subject to regulation.[13] Plants that are currently regulated or were regulated in the past are likely to present poor leakage candidates given the monitoring they face. The criterion I use is stricter in that it excludes plants that are eventually regulated. On the one hand, the pre-treatment years are particular given possible expansions of the polluting sector in those years. Anticipation of future regulation, a possibility given the legal back and forth that preceded the 2004 expansion of ozone regulations, may produce a countervailing effect on plant emissions in pre-treatment years. To circumvent these potential biases, I include these plants in my main specifications and test the robustness of the results to using broader samples.

In essence, the models in equations (2) and (3) compare plants exposed to environmental regulations through the internal firm network of their parent company to plants whose parent company is entirely unaffected by the regulation. The effect is identified from within-firm-segment variation in the exposure to regulation. To correct for the correlation of standard errors across plants in the same firm and industry, standard errors are double clustered at the firm and five digit NAICS-level in both specifications (2) and (3). This mimics the recommendation of Abadie, Athey, Imbens, and Wooldridge (2017) to cluster at the level of the treatment variable.[14]

Going beyond the plant-level, it is of additional interest estimate leakage of emissions at the segment-level to for emissions increases in the form of extensive margin expansions. In contrast to plant-level regressions, these can account for the fact that firms with many leakage candidates may only target a subset of plants in their leakage activities. Aggregating yearly emissions across unregulated and regulated plants to the segment-level, I can easily determine whether leakage effects outweigh the direct effects of regulation for partially regulated firms, taking into account both plant opening and closings.[15] Analogously to the plant-level regressions, I use segment-level emissions across all never regulated plants as the dependent variable in a specification otherwise equivalent to (2).

Motivated by the fact that there is little variation in a dummy for presence in attainment counties

[13] Using the definition of regulation as i) belonging to one the Greenstone et al. (2012) dirty industries and ii) being located in a nonattainment county according to the standard for ozone.

[14] Plausible alternatives are to two-way cluster at the county and firm-level or to cluster separately at the level of the firm-segment. Results are robust alternative choices.

[15] The dependent variable is $D_{fjt} = \sum_i D_{ifjt}$.

given the large firms in my the sample, the extensive margin analogue of (3) asks: Do partially regulated firms increase the number of plants they operate in unregulated areas in response to being regulated? This is done via the following specification:

$$\#Plants_{fjt} = \delta_{fj} + \delta_t + \beta_1 other\_treated_{fjt} + \epsilon_{fjt}. \tag{4}$$

$\#Plants_{fjt}$ is a count of the number of plants a firm-segment operates that are not affected by CAA regulation. I estimate (4) using both OLS as well as a fixed effects Poisson framework, adopted to account for the use of a count variable as the dependent variable (Hausman, Hall, and Griliches, 1984). These segment-level specifications complement the plant-level in that the estimates allow us draw conclusions about whether partial regulation reduces emissions at the firm level. The more granular specifications (2) and (3) have the benefit of allowing me to control for a wider range of confounders.

4.3 Internal Validity

Differences-in-Differences.— By including a varying set of fixed effects, I can account for a range of omitted variables that could otherwise bias the estimates. I discuss the underlying rationale and outstanding threats to identificiation. The plant fixed effects control for time-constant differences across plants, such as persistent differences in input use or other level differences in emissions, productivity or plant size. Greenstone et al. (2012) find that estimates of the impact of regulation on plant-level outcomes are often of opposite sign without plant fixed effects, so I include them throughout. The year dummies purge the estimates of common shocks to air emissions from the dependent variable. These partly account for the secular clean-up of US manufacturing documented in Shapiro and Walker (2018). Industry-year effects are used to derive estimates from comparison to an equally pollution intensive set of control plants. At baseline, I include separate year effects for each of the seven dirty industries identified by Greenstone et al. (2012) as well as a set of year effects for the remaining clean industries.

Importantly, the plant-level specifications allow me to tightly control for the disparate effects of various shocks on US counties. Over the sample period from 1992 to 2014, manufacturing in the US has been affected by a series of disruptive shocks such as the phase-in of NAFTA, the rapid increase of import competition from China or the shale-gas boom and a variety of environmental regulations beyond those considered in this paper.[16] The impact of these shocks displays enormous spatial variation and determines how suitable a plant is as a leakage candidate as well as its exposure to regulation. This is because regulation is a function of local pollution which in

[16] Cherniwchan (2017) (also focusing on a sample of TRI plants) and Autor, Dorn, and Hanson (2013) are examples of the heterogenous effects of trade shocks across US regions, Feyrer, Mansur, and Sacerdote (2017) estimate the highly localized and large effects of the shale-gas boom. Shapiro and Walker (2018) quantify the aggregate effects of these many, locally overlapping environmental regulations on pollution levels.

turn depends on the size of the manufacturing sector. By including county-year fixed effects I can account for these confounders.

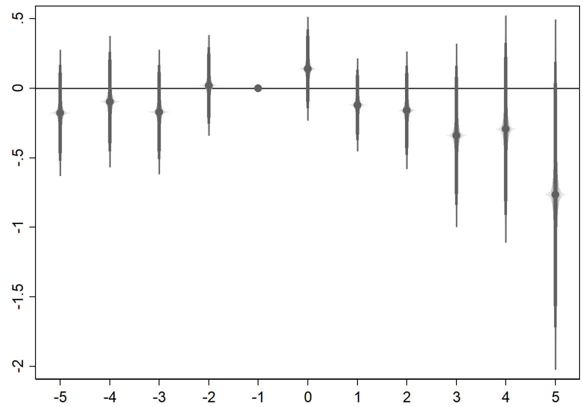
Idiosyncracies within the enforcement of the CAA across counties are a further potential source of bias. Zou (2018) and Grainger, Schreiber, and Chang (2018) both show that, at a local level, regulators often try to circumvent enforcement via strategic monitoring efforts. Such meddling efforts are particularly problematic, if they are conducted with plants' potential outcomes under regulation in mind. Thus, county-year effects also control for the fact that there is substantial variation in the stringency of environmental laws across the US, far exceeding the variation created by the CAA.

Common Trends.— While fixed effects credibly control for a number of omitted variables, other confounding factors remain. Nonattainment is strongly influenced by past emissions in a county, creating the possibility of selection into treatment. To evade treatment, regulated firms may have also preemptively shifted production into counties unlikely to be affected by the CAA expansion, biasing my leakage estimates. To examine possible selection and anticipation effects, I estimate the dynamics of plant emissions in the years prior to treatment. Differences in emissions trends between treatment and control group prior to treatment further provide a useful diagnostic whether these between group differences would have remained constant in the absence of treatment, i.e. the common trends assumption. Panels (a), (b) and (c) in Figure 3 show that differences in trends are small and statistically insignificant prior to the regulatory/leakage treatment. Overall, I interpret these results as providing support for the common trends assumption. Common trends, however, remain an assumption of the research design since confounders, such as some of the ones discussed, may still bias results if they closely covary with the onset of treatment. The post-treatment dynamics also provide some preliminary support for the proposed hypothesis: Leakage and emissions reductions occur simultaneously, indicating that firms aim to actively shift emissions between plants as local factors change.[17]

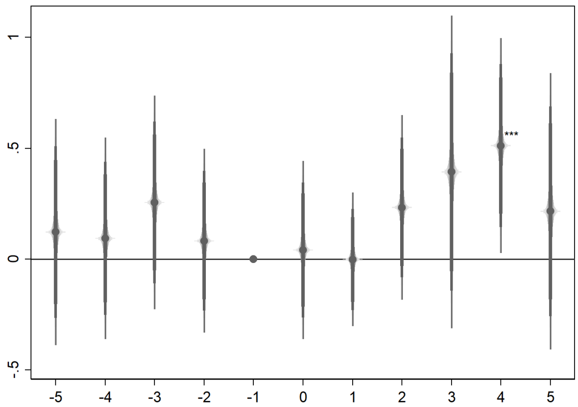
Stable Unit Treatment Value.— A final set of concerns revolve around potential violations of the stable unit treatment value assumption (SUTVA) (Rubin, 1980). SUTVA would be violated if treatment indirectly affects the control group via within-firm spillovers as in this paper, or in the form of reallocations to other firms in the economy. Along these lines, Hafstead and Williams (2018) argue that difference-in-differences estimators of environmental regulation like mine are substantially biased if resources are absorbed by unaffected firms in the economy. A relative decrease in emissions among regulated plants may hence not reflect an absolute decrease but rather a shifting of emissions between control and treatment group. Leakage estimates, on the other hand, may be biased downwards if plants without regulated affiliates also increase emissions. In robustness checks, I therefore estimate the direct effects of regulation on sub-samples excluding parts

[17] Borusyak and Jaravel (2017) point out that if the dynamic effects are heterogenous, as appears to be the case here, the coefficient β_1 in equations (1), (2) and (3) need not identify a convex combination of the individual effects β_k for $k = 0, 1, ..., 5$. In the presence of a large control group of never treated units, such as in the present setting, the bias they describe plays a quantitatively small role, however.

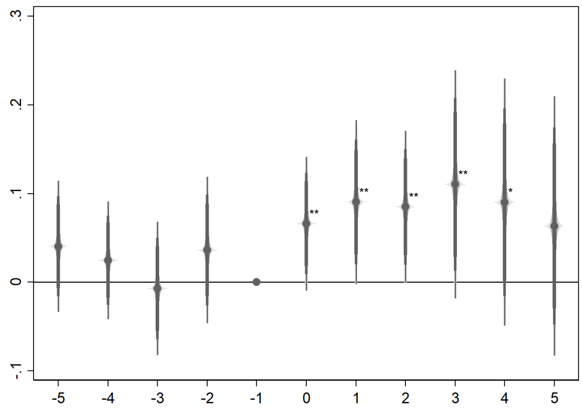
Figure 3: COMMON TRENDS



(a) Direct Effects



(b) Leakage: Intensive Margin



(c) Leakage: Extensive Margin

Notes: The figure plots coefficients on indicators β_k of years relative to the regulatory/leakage treatment obtained from estimating $Y_{ict} = \delta_i + \delta_{ct} + \sum_{k=-10}^{-1} \beta_k \mathbb{1}\{K_{it} = k\} + \sum_{k=0}^{5} \beta_k \mathbb{1}\{K_{it} = k\} + \alpha \mathbb{1}\{k > 5\} + \epsilon_{ict}$, where δ_i and δ_{ct} are plant and county-year effects, respectively and α absorbs the long-run effects. Only coefficients within 5 years of the the treatment dates are plotted. The dependent variable is the log of VOC air emissions ((a) and (b))/ the dummy variable $Active_{ifjt}$ as defined in the text. Standard errors are clustered on the county (a)/ double-clustered on firm and five-digit NAICS-level ((b) and (c)). * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$.

of the control group that may be most likely to benefit from the negative effects regulation has on directly affected plants, such as plants within the same firm or geographically proximate competitors of regulated firms. These results are briefly summarized in the subsequent section.

Summary Statistics.— Table B.1 presents descriptive statistics of the sample, by regulatory status of the plant (firm). While treatment is more likely to affect larger and pollution intensive firms, this is intuitive given that operating more plants increases the likelihood of being exposed to CAA regulation. The size differences are not apparent when considering sales per plant. The higher pollution intensity similarly follows from the industry based definition of treatment. The sample appears balanced on covariates such as productivity or financial constraints, which is reassuring as the incidence of the regulation should not disproportionately fall on firms that stand out in these dimensions.

5 Results

This section presents the main results of applying the estimation framework outlined in the previous section to the dataset. First, the main results for the triple difference or difference-in-difference estimators of the regulatory effects and associated within-firm leakage effects are shown. Robustness checks are presented at the end of this section

5.1 Triple Difference Estimates of Regulatory Effects

Table 2 shows the results of estimating the model in equation (1), with each column differing in the set of included fixed effects. The coefficients for the effect of nonattainment indicate that clean industries are unaffected by regulation. The negative and significant interaction effect shows that dirty industries, on the other hand, react strongly to changes in regulation. Across columns, I add industry-year, state-year and county-year effects to account for geographically isolated as well as industry-level shocks. In column (4), for example, the effects of regulation are identified from comparing outcomes for plants that experience a change in regulation, holding constant their exposure to other county and industry-level shocks. The point estimates remain stable and precisely estimated regardless of which combination of fixed effects is employed. Specification (4), estimated using county and industry-year fixed effects, may in particular be taken as a sign of robustness. This specification may, however, also be particularly vulnerable to spillovers to the control group if resources are reallocated within the county. Effects are essentially identified by comparing plants in dirty and clean industries within the same county without any cross county comparisons. Such specifications also considerably restrict the information used to identify the coefficient since the level of policy variation comes at the county industry-level. With these caveats in mind, the robustness to the inclusion of county-year effects is encouraging in the sense that results do not appear to be driven by time varying differences in enforcement or local shocks which influence the probability of treatment.

In terms of magnitude, Table 2 suggests that ozone regulation reduces emissions of VOC by between 19% (3) and 24% (1).[18] The effect is half as large as the effect of PM regulation estimated by Gibson (forthcoming). The smaller effect of ozone regulation is perhaps to be expected given the focus on a small set of plants in nonattainment counties under PM regulation.

I briefly discuss the results of some further sensitivity checks. First, I have checked whether results are robust to the inclusion of lags of the dependent variable. While there are no significant pre-trends, these tests may be underpowered. If there were some undetected increases prior to treatment, the negative triple difference estimate may reflect mean reverting dynamics (Greenstone, 2002). All specifications are robust to the inclusion of one lag, and columns (1) and (2) continue to be significantly different from zero in the presence of two lags. These results are

[18] Estimates derived using exponentiated coefficients.

available upon request. It is also possible that the results are driven by leakage between and within-firms, instead of (only) reflecting reductions at treated plants. Table B.2 shows that the effects of regulation are essentially equivalent within sub-samples more likely to satisfy the SUTVA. Specifically, Panel A in Table B.2 presents estimates of the effects of ozone regulation on the sample of the 660 plants that are subject to regulation at some point between 1992-2014.[19] Using only plants eventually regulated, or regulated in the past, as the control group may limit the scope of between-firm leakage given firms ability to anticipate regulation under the CAA. In Panel B, I exclude all plants identified as within-firm leakage candidates, i.e. plants that are themselves not treated, but within the same firm and industry as a treated plant. Effects in either sub-sample are similar in terms of statistical precision and size as those reported in Table 2. It may seem surprising that results are insensitive to excluding leakage candidates from the estimation sample. This robustness likely stems from the fact that they are just one subset of a relatively large control group. Thus, the leakage effects are averaged out within the large control group of unregulated plants.

5.2 Leakage Effects: Intensive Margin

In Table 3, I test whether partially regulated firms shift emissions towards unregulated plants. Plants are subject to the spillover treatment if the firm is regulated at a different plant in the same five digit NAICS industry (firm-segment). The sample consists of plants that never receive the regulatory treatment (defined as $NAA_{ct} \times Dirty_j = 1$). I separately estimate the effect of having at least one, exactly one or more than one plants regulated within the firm-segment. Applying the model in equation (2) to this sample shows that within-firm leakage effects are small and insignificant in specifications with only plant and year fixed effects (columns (1) and (4)). Once county-year effects are accounted for (columns (2)-(3) and (5)-(6)), effects increase by factor of six and are statistically significant (the standard error remains roughly constant). The coefficient becomes slightly larger, but remains similar in magnitude once I additionally control for industry-year effects. These much larger effects, emerging from specifications that tightly control for changes in local economic conditions, may indicate that shocks to US manufacturing co-determine plants' attractiveness as leakage candidates. Another explanation is that many other changes to local environmental regulations are uncontrolled for in column (1) and (4), biasing the effect downward.[20] Conceptually, models including county-year effects have an attractive interpretation. Effects in those specifications are derived from comparing plants within the same county, but who differ in their exposure to environmental regulation via the internal firm network of their

[19]Since all eventually regulated plants ($NAA_{ct} \times Dirty = 1$ for some t) are in nonattainment counties, the effect of nonattainment as well as well the county-year effects are not separately identified here. I have also estimated the effects of the regulation within a sample of both dirty and clean plants located in eventual nonattainment counties. In that case, both *are* identified and their inclusion makes essentially no difference.

[20]A prominent example of a policy change occurring simultaneously to the expansion of ozone regulation under the CAA in 2004, is the NO_x budget trading program, which had sizable impacts on the industrial sector, directly or via increased energy prices (Curtis, 2018).

parent company. This identification strategy is sensible here as the spillover treatment is firm-segment specific, making such comparisons quite informative. In terms of magnitude, emissions increase between and 28-30%, which is around three times the size of leakage effects found by Gibson (forthcoming). Columns (5)-(6) show that leakage effects are larger, but similar, for firms regulated at more than one plant.

In robustness checks, I have estimated models including firm-year effects, essentially comparing plants within the same firm, but in segments that are differentially exposed to the regulation. Similar to the inclusion of county-year effects in (1), this is highly restrictive, relying on within multisegment firm comparisons. Including these fixed effects produces results similar in terms of precision and size compared to the ones reported in Table 3, with the exception that large and marginally significant effects obtain even in the absence of county-year effects. These results are available upon request. In Table B.3, I present estimates derived from a slightly larger sample, only excluding plants regulated in the past (Panel A) or currently regulated plants (Panel B). Coefficients in Panel B imply around five percentage points smaller leakage effects, but estimates in Table B.3 are otherwise similar to those in Table 3.

5.2.1 Leakage Effects: Intensive Margin - Mechanisms

I have proposed that leakage arises because output at different plants is equally suitable to satisfy the demand facing the firm. To probe the extent to which my results are consistent with this mechanism, I consider two simple extensions to validate this hypothesis and one alternative explanation based on financial constraints.

Tradability.— The conceptual framework describes firms as using different plants interchangeably to meet their demand. Firms in non-tradable industries can only do so at high transportation cost, making them less likely to engage in intrafirm leakage. To measure tradability, I employ two indices, constructed following work by Giroud and Rauh (forthcoming) and Mian and Sufi (2014). Both are based on the geographic distribution of four-digit NAICS industries within the US. Roughly, they both capture the intuition that non-tradable industries will be geographically dispersed to be able to reach consumers everywhere despite high transportation cost. Appendix B.3 provides more details.

To investigate, how effects vary across industries, I sort the 110 four-digit NAICS industries into four quartiles according to the respective tradability measure. Effects are estimated separately for each quartile using the following model:

$$D_{ijt} = \delta_{tj} + \delta_i + \beta_k \sum_{k=1}^{4} Q_k \times other\_treated_{ijt} + \epsilon_{ijt}. \quad (5)$$

where δ_{tj} are previously defined industry-year effects.[21] Since only plants in dirty industries are subject to spillovers in the models previously estimated, the spillover candidates are not equally distributed across quartiles. There is only a low number of observations in the highest quartile of tradability (according to either measure). The few leakage candidates in the fourth quartile are assigned to the third quartile. I additionally estimate models similar to (2), where I add an interaction of the dummy variable $other\_treated_{fjt}$ with the continuous tradability indices.[22]

Table 4 presents the results of estimating both sets of models using the respective indices. Some of the coefficients are only marginally significant or generally imprecisely estimated, especially in specifications where the continuous tradability measures are used. Granting this uncertainty, leakage effects for non-tradable (bottom quartile) industries are either negative or close to zero. Columns (1) and (5) indicate that previous null results (e.g. in column (1) of Table 3) are driven by offsetting effects across quartiles. Zero effects resulted from negative effects for bottom quartile plants and positively estimated leakage effects for plants in tradable industries. The negative effects in non-tradable industries, however, are neither significant nor robust to the inclusion of my preferred set of fixed effects. I therefore interpret these results as providing evidence of zero leakage in non-tradable industries.

The differences in effect size across quartiles are sensible given the ranking of industries. Plants in the lowest quartile belong to industries such as concrete or petroleum, whereas the third quartile is comprised of industries like pharmaceutical manufacturing or synthetic rubber. High transport costs in the former set of industries makes it unprofitable for firms to substitute across plants in order to meet their demand. A simple F-test of the null hypothesis of equal effects for the bottom and top quartile does not, however, consistently reject the null hypothesis of equal effects (the p-values are reported in the Table). Table 4 nevertheless paints a relatively consistent picture. There is no leakage in non-tradable industries, while plants in tradable industries increase emissions if their parent company is regulated.

Different Industries.— Second, I estimate the leakage effects by defining the spillover treatment at the firm and not the firm-segment-level. If interrelated demand is in fact the driving mechanism, leakage effects should be small or zero if plants produce different goods. I test this hypothesis in Table B.4. The specifications used are otherwise equivalent to those presented Table 3. The estimated effects are close to zero across columns (1)-(6). Together, results in Tables 4 and B.4, corroborate the idea that demand interdependencies are a prerequisite for leakage effects to arise. These appear to be absent across plants in different industries and for plants in tradable industries. That leakage effects vary along these dimensions also highlights the importance of

[21] Replacing these with four-digit NAICS year dummies to identify the triple difference effect within industries that are equally tradable has little to no effect on the results. I therefore report estimates using the coarser industry classification, as specifications using county and industry-year effects become very demanding otherwise.

[22] The model in equation (5) has two advantages over those using more standard continuous interaction terms (Hainmueller, Mummolo, and Xu, forthcoming): For one, it does not restrict the effects to be linear across industries. Second, it is less susceptible to measurement error as it only exploits a limited amount of ordinal information to rank industries.

estimating leakage affects within firm-segments.

Financial Constraints.— Financial constraints are an alternative explanation for these results. In a recent set of papers, Giroud and Mueller (2015, forthcoming) show that financial constraints can act as a powerful propagator of shocks within internal firm networks. In their theory, only resource constrained firms, not able to finance the desired, first best allocation at each of their plants, distribute the effects of local shocks across establishments. Plants part of financially unconstrained firms do not absorb the resources set free by the shock since they are already at the first best allocation. This is a different explanation for within-firm spillovers.

To consider the role of financial constraints, Table B.5 shows estimates conceptually similar to the ones in Table 4. Columns (1)-(4) display estimates using the Hoberg and Maksimovic (2014) measure, while columns (5)-(8) re-estimate those same specifications using the Kaplan and Zingales (1997) index. They suggest that - if anything - effects are larger and more precisely estimated for firms in the lower quartiles of the text-based index. Results derived from specifications using the KZ index are slightly different, in that financial constraints do not appear to affect the intensity of leakage effects in either direction. Taken together, the estimates in Tables 4, B.4 and B.5 support the demand based mechanism for within-firm leakage.

5.3 Leakage Effects: Extensive Margin

Table 5 reports the results from estimating (3). The explanatory variable is not lagged in this case to account for the immediate response on the extensive margin observed in Figure 3. Recall that the dependent variable is one in years a plant is in operation and zero otherwise, with years where a firm-segment is not active anywhere excluded from the estimation. Estimated coefficients across columns (1)-(3) are small and statistically indistinguishable from zero. This contrast somewhat with the event study specification shown in Panel c) of Figure 3, which showed some significant post treatment dynamics. The identifying variation is, however, somewhat different here in that i) both moves in and out of treatment are used to estimate the effects ii) effects are identified relative to all periods where the plant is not treated and not to a particular pre-treatment year. Results for using the binned treatment variables in columns (4)-(6) show larger and significant effects on the probability of operating a unregulated plant for firms regulated at more than one plant. This result is broadly consistent with the idea that the fixed cost of relocation is worth paying only for firms particularly affected by the regulation. This is shown analytically in Appendix A.2. Estimates indicate that exposure to more than one regulated plant through the internal firm network increases the probability of a plant going into or remaining in operation by 8-10 percentage points. In contrast to the results on the intensive margin, results are generally insensitive to the inclusion of county or industry-year effects.

5.3.1 Leakage Effects: Extensive Margin - Mechanism

Productivity.— Figure 1 shows that high productivity firms benefit most from the cost saving potential of an unregulated plant. To test this hypothesis, I use the Compustat data to estimate total factor productivity (TFP) by regressing (log) real sales on firm and year fixed effects, (log) real capital and the (log) number of employees:[23]

$$ln(y_{it}) = \delta_t + \mu_i + \beta_1 ln(emp_{it}) + \beta_2 ln(k_{it}) + \epsilon_{it}. \tag{6}$$

Since the focus is on estimating time constant differences between-firms, the objects of interests are the firm fixed effects μ_i. To estimate equation (6), I use data from 1980-2016 since μ_i is asymptotically consistent only for large T. The estimated firm fixed effects are then demeaned by subtracting the three-digit SIC industry average. This removes variation in the fixed effect due to differences in factor use across industries. These demeaned estimates of μ_i are sorted into four quartiles.

In Table 6, I present estimates where I either separately estimate effects for each TFP quartile (columns (1)-(3)) or use the TFP measure as a continuous interacting variable (columns (4)-(6)). Across columns, I include a successively more stringent set of fixed effects. The effects for firms in the highest TFP quartile are indeed positive, large and precisely estimated. Results consistently reject the hypothesis that results are equal for the least and most productive firms. Specifications using the continuous TFP variable confirm this, with the coefficient on the interaction being positive and significantly estimated across columns (4)-(6). For firms low in productivity, effects are either insignificant or estimated to be significantly negative.

To probe the robustness of these results, I include firm-year effects in columns (3) and (6). Effects are thus identified by comparing segments within the same firm that are differentially exposed to environmental regulation. By restricting the estimates to be derived within the firm, I mitigate concerns that high productivity firms may generally exhibit differential entry and exit dynamics. Effects continue to be positive and precisely estimated for firms in the highest productivity quartile.

5.4 Leakage Effects: Firm-Level

To test for leakage at the firm-level, I aggregate the emissions across plants within the same firm-segment and year. Columns (1) and (2) of Table 7 show estimates of regulatory effects on emissions stemming from never regulated plants. Both are derived from models including firm-

[23]The method of estimating productivity is taken from Fromenteau, Schymik, and Tscheke (forthcoming). I follow their approach to construct the capital stock and use the same deflators.

segment and industry-year fixed effects.[24] In column (1), the explanatory variable is a dummy for whether the segment is exposed the environmental regulation at one of its plants. Effects are small and not significantly different from zero. This is in line with the results on the intensive margin, which were derived from specifications accounting for county-year effects. Once a binned count of plants is used as the explanatory variable, as in column (2), the effects of having more than one plant regulated is positive and significant. Leakage to unregulated plants thus also obtains on the segment-level, even without controlling for local shocks.

The results so far have shown that there are reductions in emissions at regulated plants and leakage to unregulated plants. What do these countervailing effects amount to? In columns (3) and (4), I approach this question by aggregating emissions across all plants, regulated and unregulated, and re-estimate the models in columns (1) and (2). Estimates in column (3) indicate that on average, firms exposed to CAA regulations of ozone have not decreased their emissions. Column (4) shows that firm-segments that were regulated at more than one of their plants have in fact increased their total emissions. Sensibly, the estimates in column (4) are lower than in column (2) as positive leakage effects are partially offset by the direct, negative effects of regulation. The implication is that firms that are particularly strongly affected by the regulation increase their emissions at unregulated plants even more.

Two factors might be responsible for these disproportionate leakage effects for firm-segments with two or more regulated plants. First, segments with many regulated plants also tend to operate many plants that are suitable leakage candidates because they are large in general. The regulation-induced decreases at the treated plants are thus associated with increases in emissions across a large number of plants. More importantly, however, strong exposure to regulation is associated with extensive margin expansions in unregulated areas, as shown in Table 5 (see below for further evidence). These shifts are factored in when estimating leakage at the segment level, but are not considered when estimation is conducted at the plant-level. A back-of-the-envelope calculation based on the plant-level estimates of direct and leakage efects also implies an increase in emissions at the segment-level.[25]

In columns (5)-(8), I estimate the extensive margin of leakage at the firm-segment-level. To do so, I use the count of plants never subject to regulation as the dependent variable. The count takes the value zero in years where the segment does not yet operate an unregulated plant. Analogously to

[24]The industry with the highest number of plant-year observations within a firm-segment is used as that segment's industry for the purposes of constructing industry-year effects.

[25] Consider the following back-of-the-envelope calculation (Gibson, forthcoming): The average, partially regulated firm-segment operates about 1.4 regulated plants and 1.6 unregulated plants. Pre (spillover)-treatment emissions are similar across either set of plants (171,060 pounds (leakage plant) vs. 158,681 pounds (regulated) plants. (Exponentiated) coefficients in column (4) of Table 2 and column (3) of Table 3 imply spillover and treatment effects of 30% and -21% respectively. On net, leakage effects are:

$$\underbrace{1.6 \times 171,060 \times 0.3}_{Spillover} - \underbrace{1.4 \times 158,681 \times 0.21}_{Treatment} \approx 35,000$$

the estimation of extensive margin effects at the plant-level, I exclude firm-segment-years where the segment has no active TRI plants anywhere in the country. To estimate the model specified in equation (4), I employ either Poisson or standard OLS regression frameworks. For the Poisson regressions, the segment's industry is either dirty or clean, to limit the number of industry-year fixed effects to be estimated. Standard errors for the Poisson regressions are adjusted for overdispersion.

Estimates using both the binary spillover dummy as well as the dummies for the binned counts suggest that partial regulation leads segments to expand into unregulated areas. The effects are sizable and statistically significant. The coefficient in column (5), for example, implies that the number of never regulated plants increases by 8%. For firm-segments that are observed before they are regulated at one of their plants, the pre-treatment mean of the dependent variable is 1.96. The coefficient thus implies an increase of 0.15 establishments. The coefficient in column (7), where I estimate the same specification using OLS, implies roughly the same magnitude. The extensive margin effects are about twice as large for segments that are regulated at more than one of their establishments, potentially explaining the large leakage rates among this set of firms.

5.5 Extensions and Robustness

In this section, I briefly discuss additional results as well robustness checks reported in the Appendix.

Plant Exit.— In Table B.6, I test whether plants exposed to environmental regulation are more likely to exit. To do so, I define a dummy variable that takes the value one in the last year the plant is observed in the TRI and zero otherwise. The last year of the sample is excluded as measuring exits requires knowledge of plant's operating status in the future. Years before the plant enters are similarly excluded. In columns (1)-(3), this dependent variable is employed in a specification otherwise equivalent to (1). Column (1) implies about a 2 percentage point increase in the likelihood of exiting. Effects are not robust to controlling for more granular local and industry-year shocks (columns (2)-(3)). In columns (4)-(6), I test whether the least productive firms are most likely to exit in response to the cost-shock - as implied by the heterogenous firm model presented in the appendix. Consistent with the theory, column (4) shows that regulation increases the likelihood of exit by around 6 percentage points for plants whose parent company is below the median in productivity, with no effect on more productive firms. Column (5), controlling controlling for state and industry shocks, confirms this pattern, with negative, insignificant estimates for the most productive firms. The exit effects are no longer apparent and even negative (marginally significant) for the latter set of firms if I control for county-year effects in column (6). Given that these results only obtain in a specification that severely restricts the information in the independent variable, I refrain from interpreting the negative effects on more productive firms and take columns (4) and (5) as providing a reasonable empirical case that plants within the least productive firms exit. A prediction that is clearly not borne out by the data is that the most productive firms open new plants *and* shut down regulated plants. Rather, low productivity firms exit at regulated plants, and high productivity firms expand.

Broader Treatment Definition.— To test the robustness of my results to using a more expansive definition of polluting industries, I expand this set so that it includes other less polluting sectors as in Greenstone (2002). Many more plants are potentially the target of regulations using this classification. In Table B.7, I re-estimate the direct effects of regulation as in equation (1) using this alternative definition of *Dirty Industry* to determine a plant's regulatory status. Results in columns (1)-(2), show that the regulatory effects are attenuated compared to those found in Table 2, but remain statistically significant. In columns (3)-(4), I drop all plants at the intersection of the classifications by Greenstone (2002) and Greenstone et al. (2012). Thus, I separately estimate treatment effects for marginal industries. The effect of county nonattainment status is essentially zero for the plants in these relatively less polluting industries.

In Table B.8, I re-estimate the intensive and extensive margin of leakage at the plant-level using the models in equation (2) and (3), but with spillover dummy variables adjusted to reflect the wider definition of treatment. On the intensive margin (columns (1)-(3)), this produces similar but slightly larger leakage rates than in Table 3. Coefficients still double in size once county-year effects are included, but are marginally significant without. While these plants do not appear to be affected in terms of their emissions patterns, they appear to have nevertheless shifted emissions out of regulated counties. Results on the extensive margin are very similar to those in Table 5 and merit no further discussion.

Public and Private Firms.— In the main analysis, I have focused on plants belonging to large publicly traded companies. Public firms are larger than private firms, and should therefore correspond more closely to the set of "corporate" plants most affected by ozone regulation (Becker and Henderson, 2000).[26] To test this hypothesis, I re-estimate the regulatory effects first on the sample of publicly and privately owned plants as well as on the subset of plants owned by private companies. Table B.9 displays the results. Regulatory effects are attenuated and vary in significance for the whole sample. Columns (3)-(4) show that ozone regulation had no effects within the sample of privately owned plants, consistent with the argument in Becker and Henderson (2000).

Regarding leakage effects for the whole sample, intensive margin estimates, reported in Table B.10, are attenuated in size and statistically insignificant. Including the smaller set of private plants unaffected by the regulation attenuates intensive margin leakage estimates since these plants face little incentive to engage in pollution shifting activities in the first place. Extensive margin results are similar to the public firm sample.

General Equilibrium Effects.— A salient concern for the identification of treatment and spillover

[26]Consistent with these hypothesized differences in size, plants part of publicly listed firms emit roughly 45 % more VOC emissions than their privately listed counterparts. Emissions likely understate the differences in size since smaller facilities are often particularly pollution intensive (Becker and Henderson, 2000).

effects are emissions increases within the control group. If between-firm spillovers are large, as argued by Hafstead and Williams (2018), regulatory effects are biased upwards and spillover effects downwards. Unregulated plants located in close vicinity to regulated plants may be particularly likely to benefit from regulation of their competitors. This idea is related to Gibson (forthcoming)'s test for between firm spillovers. To test this hypothesis, I use a sample of plants in attainment counties and regress (log) emissions of plant i on the number of regulated plants within 100, 200 or 500 km of plant i as well as plant and year fixed effects. Results are shown in columns (1)-(3). While there are some statistically significant spillover effects at greater distances, the effect size is economically small. In column (2), for example, a one standard deviation increase in the explanatory variable means 9.3 more regulated plants within 200 km. The coefficient thus implies a modest increase in emissions of about 0.9% per 10 additional regulated plants within 200 km. Effects are small and indistinguishable from zero if I employ counts of regulated plants in the same five-digit NAICS industry as the explanatory variable (columns (4)-(6)). I conclude that between-firm leakage effects, at a local level, are quantitatively small or insignificant, providing some tentative support for the identifying assumption of no direct effects on the control group. Since general equilibrium effects are difficult to pin down in reduced form analysis, results do not imply *no* reallocation of activity to the control group.

Heterogeneity in Regulatory Effects.— A point not considered so far is heterogeneity in the strength of direct regulatory effects. Larger spillover effects along any of the dimensions considered in this study may simply be the flip side to higher intensity of treatment at the regulated plants. If so, interpreting this heterogeneity through the lens of my theoretical framework would be misleading. In Table B.12, I estimate models similar to specification (5) for the four main interacting variables used for the intensive and extensive margin estimates. The models include plant and county-year fixed effects. Effects are essentially equivalent using other fixed effect schemes. While effects vary between quartiles in some cases, I cannot reject the hypothesis that effects are equal for plants in the top and bottom quartile.

6 Discussion

These results call for a potential reassessment of both the benefit *and* cost to industry of ozone regulation as well as its contribution to emissions reductions achieved by US manufacturing (Levinson, 2009). Taking a quantitative approach, Shapiro and Walker (2018) identify stringent environmental regulation as the main driver of this clean-up. My reduced form estimates, by contrast, suggest that ozone regulations under the CAA may not have facilitated these reductions.

The benefits of the CAA are thought to arise from improved health outcomes by way of decreased population exposure to air pollution. Ozone pollution has significant mortality cost in the short and long-run (Bell et al., 2004; Di et al., 2017) as well as detrimental effects on worker productivity (Graff Zivin and Neidell, 2012). VOC chemicals, the ozone precursors studied in this paper, are known carcinogens (Villeneuve et al., 2013; Zhao et al., 2004) and adversely affect infant health outcomes (Agarwal, Banternghansa, and Bui, 2010; Currie and Schmieder, 2009).[27] As a result, decreasing VOC emissions became an independent priority under the 1990 Amendments to the CAA (Portney, 1990).[28] Whether the CAA, by shifting the incidence of VOC pollution from more to less polluted counties rather than reducing its level in the aggregate, has improved health outcomes depends on whether reductions in polluted areas are more significant than increases in less affected ones. The dose-response relationship between pollution and health outcomes in the aforementioned studies is found to be linear, implying that merely shifting emissions does not improve health outcomes.

An important caveat is that one cannot easily determine how regulation-induced changes in VOC emissions affect ozone levels. While I have focused on VOC chemicals which the EPA considers to be major contributors to ozone levels, other environmental factors co-determine how much VOC emissions contribute to ozone levels. VOC emissions are, however, direct threats to human health, such that leakage of VOC offsets at least some of the benefits of ozone regulation. The welfare losses associated with extensive margin relocations are more straightforward: Currie et al. (2015) show that the presence of a TRI plant increases the probability of low birth weight by 3 percent within 1 mile of the plant and lowers housing values by 11% within 0.5 miles. The expansions into unregulated areas documented here thus have significant negative impacts on health and wealth outcomes.

On the cost side, an evaluation of the CAA that ignores intrafirm leakage may overstate the national loss caused by the policy. If regulation-induced changes in emissions are symmetrically accompanied by corresponding changes in employment, these negative economic effects of the CAA may be smaller than the direct effects documented in previous literature (e.g. Becker and Henderson (2000); Greenstone et al. (2012)). The results on firm-level expansions into unregulated areas provide some direct evidence that the relocations are not just in terms of emissions, but also come in the form of tangible economic activity. It is significant that this reallocation takes place at the firm-level where adjustment processes are plausibly less frictional than between firms or industries. As a counterpoint, Becker and Henderson (2000) argue that reallocation of economic activity within the US may lead to "spatial distortions". If these distortions are large, the aggregate cost of ozone regulation may not be substantially lower when factoring in intensive and extensive margin leakage effects.

[27] Compared to the long literature on the health effects of criteria air pollutants, the health effects of toxic pollutants like VOC are less well understood (Currie et al., 2015).

[28] The increased focus was reflected in the creation of Title III, specifying a list 189 particularly toxic chemicals, many of which can be classified as VOCs.

7 Conclusion

I have analyzed whether firms' internal network is an important margin of adjustment to piecemeal environmental regulation. The empirical results, based on a newly assembled dataset, show that firms both rely on existing unregulated plants as well as the creation of new ones to offset the direct effects of regulation. Strikingly, these leakage effects fully offset the direct effects on regulated plants. By themselves, these results imply that expanded ozone regulation under the CAA has not contributed to the clean-up of US manufacturing, in contrast with results by Shapiro and Walker (2018). By testing for heterogeneous effects, these results are revealed to be consistent with a simple framework of multiplant production.

Notably, these effects were identified by differencing out possible general equilibrium adjustments across firms, sectors and regions. At a local level, these effects appear insignificant in that unregulated plants in proximity to regulated regions do not take up market share. A fuller investigation of these adjustments across labor markets and industries may well reveal even larger reallocative effects of piecemeal regulation.

Future research could further quantify the implications of (intrafirm) leakage of economic activity (plants) as well as emissions for cost-benefit evaluations of ozone regulation. First, it is important to investigate whether emissions spillover are accompanied by increases in employment at unregulated plants. Given the large leakage rates documented in this study, it would be important to better understand how much this leakage of VOC emissions erodes the health benefits of ozone regulation. Progress could be made by disaggregating the VOC chemicals by their contribution to ozone levels, as in Auffhammer and Kellogg (2011). Finally, it would be a valuable exercise to exploit the CAA-induced reductions of VOC emissions to determine the direct health effects of toxic pollution. Such quasi-experimental evidence is so far missing from the literature.

References

Abadie, Alberto, Susan Athey, Guido W. Imbens, and Jeffrey Wooldridge (2017). When Should You Adjust Standard Errors for Clustering? NBER Working Papers 24003, National Bureau of Economic Research, Inc

Agarwal, Nikhil, Chanont Banternghansa, and Linda T.M. Bui (2010). Toxic exposure in America: Estimating fetal and infant health outcomes from 14 years of TRI reporting. *Journal of Health Economics* 29(4), pp. 557–574

Antràs, Pol and Elhanan Helpman (2004). Global Sourcing. *Journal of Political Economy* 112(3), pp. 552–580

Auffhammer, Maximilian, Antonio M. Bento, and Scott E Lowe (2009). Measuring the effects of the Clean Air Act Amendments on ambient PM10 concentrations: The critical importance of a spatially disaggregated analysis. *Journal of Environmental Economics and Management* 58(1), pp. 15–26

Auffhammer, Maximilian and Ryan Kellogg (2011). Clearing the Air? The Effects of Gasoline Content Regulation on Air Quality. *American Economic Review* 101(6), pp. 2687–2722

Autor, David, David Dorn, and Gordon H. Hanson (2013). The China syndrome: Local labor market effects of import competition in the United States. *American Economic Review* 103(6), pp. 2121–68

Autor, David, David Dorn, Gordon H. Hanson, Gary Pisano, and Pian Shu (forthcoming). Foreign Competition and Domestic Innovation: Evidence from U.S. Patents. *American Economic Review: Insights*

Banzhaf, H. Spencer and Randall P. Walsh (2008). Do people vote with their feet? An empirical test of Tiebout. *American Economic Review* 98(3), pp. 843–63

Becker, Randy and Vernon Henderson (2000). Effects of Air Quality Regulations on Polluting Industries. *Journal of Political Economy* 108(2), pp. 379–421

Bell, Michelle L., Aidan McDermott, Scott L. Zeger, Jonathan M. Samet, and Francesca Dominici (2004). Ozone and short-term mortality in 95 US urban communities, 1987-2000. *Journal of the American Medical Association* 292(19), pp. 2372–2378

Borusyak, Kirill and Xavier Jaravel (2017). Revisiting event study designs. *Working Paper*

Brunnermeier, Smita B. and Arik Levinson (2004). Examining the Evidence on Environmental Regulations and Industry Location. *The Journal of Environment & Development* 13(1), pp. 6–41

Bulow, Jeremy I., John D. Geanakoplos, and Paul D. Klemperer (1985). Multimarket Oligopoly: Strategic Substitutes and Complements. *Journal of Political Economy* 93(3), pp. 488–511

Cherniwchan, Jevan (2017). Trade liberalization and the environment: Evidence from NAFTA and US manufacturing. *Journal of International Economics* 105, pp. 130–149

Colmer, Jonathan, Ralf Martin, Mirabelle Muûls, and Ulrich J. Wagner (2018). Emissions Trading, Firm Behavior, and the Environment: Evidence from French Manufacturing Firms. *Working Paper*

Currie, Janet, Lucas Davis, Michael Greenstone, and Reed Walker (2015). Environmental health risks and housing values: evidence from 1,600 toxic plant openings and closings. *American Economic Review* 105(2), pp. 678–709

Currie, Janet and Johannes F. Schmieder (2009). Fetal Exposures to Toxic Releases and Infant health. *American Economic Review* 99(2), pp. 177–83

Curtis, E. Mark (2018). Who Loses under Cap-and-Trade Programs? The Labor Market Effects of the NOx Budget Trading Program. *The Review of Economics and Statistics* 100(1), pp. 151–166

de Marchi, Scott and James T. Hamilton (2006). Assessing the accuracy of self-reported data: an evaluation of the toxics release inventory. *Journal of Risk and Uncertainty* 32(1), pp. 57–76

Di, Qian, Yan Wang, Antonella Zanobetti, Yun Wang, Petros Koutrakis, Christine Choirat,

Francesca Dominici, and Joel D. Schwartz (**2017**). Air Pollution and Mortality in the Medicare Population. *New England Journal of Medicine* 376(26), pp. 2513–2522

EPA (**1997**). National Ambient Air Quality Standards for Ozone. *Federal Register* 62(138), pp. 38856–38896

——— (**2013**). 2008 National Emissions Inventory: Technical Support Document (v.3). Technical report, Environmental Protection Agency

——— (**2014**). Regulatory Impact Analysis of the Proposed Revisions to the National Ambient Air Quality Standards for Ground-Level Ozone. Technical report, Environmental Protection Agency

Fajgelbaum, Pablo D., Eduardo Morales, Juan Carlos Suárez Serrato, and Owen Zidar (**2018**). State taxes and spatial misallocation. *The Review of Economic Studies* 86(1), pp. 333–376

Feldman, Naomi E., Laura Kawano, Elena Patel, Nirupama Rao, Michael Stevens, and Jesse Edgerton (**2018**). The Long and the Short of It: Do Public and Private Firms Invest Differently? *Working Paper*

Feyrer, James, Erin T. Mansur, and Bruce Sacerdote (**2017**). Geographic dispersion of economic shocks: Evidence from the fracking revolution. *American Economic Review* 107(4), pp. 1313–34

Fowlie, Meredith L. (**2009**). Incomplete environmental regulation, imperfect competition, and emissions leakage. *American Economic Journal: Economic Policy* 1(2), pp. 72–112

Friedman, Lisa (**2017**). E.P.A. Reverses Course on Ozone Rule. *New York Times*

Fromenteau, Philippe, Jan Schymik, and Jan Tscheke (**forthcoming**). Foreign Competition and the Durability of U.S. Firm Investments. *RAND Journal of Economics*

Gamper-Rabindran, Shanti (**2009**). The Clean Air Act and volatile organic compounds: Did plants reduce their health-indexed air emissions or shift their emissions into other media? *Working Paper*

Gibson, Matthew (**forthcoming**). Regulation-Induced Pollution Substitution. *The Review of Economics and Statistics*

Giroud, Xavier and Holger M. Mueller (**2015**). Capital and Labor Reallocation within Firms. *Journal of Finance* 70(4), pp. 1767–1804

——— (**forthcoming**). Firms' Internal Networks and Local Economic Shocks. *American Economic Review*

Giroud, Xavier and Joshua Rauh (**forthcoming**). State taxation and the reallocation of business activity: Evidence from establishment-level data. *Journal of Political Economy*

Graff Zivin, Joshua and Matthew Neidell (**2012**). The Impact of Pollution on Worker Productivity. *American Economic Review* 102(7), pp. 3652–73

Grainger, Corbett, Andrew Schreiber, and Wonjun Chang (**2018**). How states comply with federal regulations: strategic ambient pollution monitoring. *Working Paper*

Greenstone, Michael (**2002**). The Impacts of Environmental Regulations on Industrial Activity: Evidence from the 1970 and 1977 Clean Air Act Amendments and the Census of Manufactures. *Journal of Political Economy* 110(6), pp. 1175–1219

——— (**2003**). Estimating Regulation-Induced Substitution: The Effect of the Clean Air Act on Water and Ground Pollution. *American Economic Review* 93(2), pp. 442–448

Greenstone, Michael, John A. List, and Chad Syverson (**2012**). The Effects of Environmental Regulation on the Competitiveness of U.S. Manufacturing. *Working Paper*

Hafstead, Marc A.C. and Roberton C. Williams (**2018**). Unemployment and environmental regulation in general equilibrium. *Journal of Public Economics* 160, pp. 50 – 65

Hainmueller, Jens, Jonathan Mummolo, and Yiqing Xu (**forthcoming**). How much should we trust estimates from multiplicative interaction models? Simple tools to improve empirical practice. *Political Analysis* pp. 1–30

Hanna, Rema (2010). US Environmental Regulation and FDI: Evidence from a Panel of US-Based Multinational Firms. *American Economic Journal: Applied Economics* 2(3), pp. 158–89

Hausman, Jerry, Bronwyn H. Hall, and Zvi Griliches (1984). Econometric Models for Count Data with an Application to the Patents-R & D Relationship. *Econometrica* 52(4), pp. 909–938

Henderson, J. Vernon (1996). Effects of Air Quality Regulation. *The American Economic Review* 86(4), pp. 789–813

Hoberg, Gerard and Vojislav Maksimovic (2014). Redefining Financial Constraints: A Text-Based Analysis. *The Review of Financial Studies* 28(5), pp. 1312–1352

Kaplan, Steven N. and Luigi Zingales (1997). Do Investment-Cash Flow Sensitivities Provide Useful Measures of Financing Constraints?*. *The Quarterly Journal of Economics* 112(1), pp. 169–215

Kleinman, L. I., P. H. Daum, D. Imre, Y.-N. Lee, L. J. Nunnermacker, S. R. Springston, J. Weinstein-Lloyd, and J. Rudolph (2002). Ozone production rate and hydrocarbon reactivity in 5 urban areas: A cause of high ozone concentration in Houston. *Geophysical Research Letters* 29(10), pp. 105–1–105–4

Levine, Ross, Chen Lin, and Zigan Wang (2018a). Toxic Emissions and Executive Migration. *Working Paper*

Levine, Ross, Chen Lin, Zigan Wang, and Wensi Xie (2018b). Bank Liquidity, Credit Supply, and the Environment. *Working Paper*

Levinson, Arik (2009). Technology, international trade, and pollution from US manufacturing. *American Economic Review* 99(5), pp. 2177–92

Lewis, Alastair C., Nicola Carslaw, Philip J. Marriott, Russel M. Kinghorn, Paul Morrison, Andrew L. Lee, Keith D. Bartle, and Michael J. Pilling (2000). A larger pool of ozone-forming carbon compounds in urban atmospheres. *Nature* 405, pp. 778 EP –

Mian, Atif and Amir Sufi (2014). What explains the 2007–2009 drop in employment? *Econometrica* 82(6), pp. 2197–2223

Mills, Elizabeth (2002). Whitman v. American Trucking Associations, Inc. *Ecology Law Quarterly* 29, p. 159

Mrázová, Monika and J. Peter Neary (forthcoming). Selection Effects with Heterogeneous Firms. *Journal of the European Economic Association*

Mukherjee, Arijit (2010). A Note on Firm-Productivity and Foreign Direct Investment. *Economics Bulletin* 30(3), p. 2107–2111

Portney, Paul R. (1990). Policy Watch: Economics and the Clean Air Act. *The Journal of Economic Perspectives* 4(4), pp. 173–181

Rader, Trout (1968). Normally, Factor Inputs Are Never Gross Substitutes. *Journal of Political Economy* 76(1), pp. 38–43

Rubin, Donald B. (1980). Randomization analysis of experimental data: The Fisher randomization test comment. *Journal of the American Statistical Association* 75(371), pp. 591–593

Shapiro, Joseph S. and Reed Walker (2018). Why Is Pollution from US Manufacturing Declining? The Roles of Environmental Regulation, Productivity, and Trade. *American Economic Review* 108(12), pp. 3814–54

Syverson, Chad (2008). Markets: Ready-mixed concrete. *Journal of Economic Perspectives* 22(1), pp. 217–234

Traina, James (2018). Is aggregate market power increasing? production trends using financial statements. *Working Paper*

Villeneuve, Paul J., Michael Jerrett, Jason Su, Richard T. Burnett, Hong Chen, Jeffrey Brook, Amanda J. Wheeler, Sabit Cakmak, and Mark S. Goldberg (2013). A Cohort Study of Intra-Urban Variations in Volatile Organic Compounds and Mortality, Toronto, Canada. *Environmental pollution* 183, pp. 30–39

Walker, W. Reed (**2011**). Environmental regulation and labor reallocation: Evidence from the Clean Air Act. *American Economic Review* 101(3), pp. 442–47

Zhao, Lirong, Xinming Wang, Qiusheng He, Hao Wang, Guoying Sheng, L.Y. Chan, Jiamo Fu, and D.R. Blake (**2004**). Exposure to Hazardous Volatile Organic Compounds, PM10 and CO While Walking along Streets in Urban Guangzhou, China. *Atmospheric Environment* 38(36), pp. 6177–6184

Zou, Eric (**2018**). Unwatched Pollution: The Effect of Intermittent Monitoring on Air Quality. *Working Paper*

Table 1: CORRELATION MATRIX

	Pollution Intensity	Financial Constraints	Productivity
Pollution Intensity	1		
Financial Constraints	0.0942***	1	
Productivity	-0.112***	0.0157	1

The table displays pairwise correlation coefficients. *Pollution Intensity* is the (log) ratio of VOC air emissions to real sales. *Financial constraints*, taken from Hoberg and Maksimovic (2014), are measured based on the analysis of 10k forms. For both variables, the median value across firm-years is used. *Productivity* is estimated by i) obtaining the firm fixed effects μ_i from the regression model $ln(y_{it}) = \delta_t + \mu_i + \beta_1 ln(emp_{it}) + \beta_2 ln(k_{it}) + \epsilon_{it}$ ii) regressing μ_i on industry dummies (3-digit SIC). The residuals from step ii) are used as the measure of *Productivity*. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2: EFFECT OF OZONE NONATTAINMENT

	(1)	(2)	(3)	(4)
NAA	0.0428	0.0233	-0.00854	
	(0.0465)	(0.0473)	(0.0513)	
NAA x Dirty Industry	-0.275***	-0.207**	-0.194**	-0.228**
	(0.0854)	(0.0873)	(0.0879)	(0.103)
Plant F.E.	yes	yes	yes	yes
Year F.E.	yes	no	no	no
State-Year F.E.	no	no	yes	no
Industry-Year F.E.	no	yes	yes	yes
County-Year F.E.	no	no	no	yes
Observations	66891	66835	66833	54997

An observation is a plant-year. Standard errors, clustered on the county-level, are reported in parentheses. The dependent variable is the natural logarithm of plant-level air emissions of VOC. *NAA* equals one in all years a county is classified as part of a nonattainment area for ozone according to the EPA Greenbook data and zero otherwise. *Dirty Industry* is a binary indicator equal to one for plants in industries classified as heavy emitters of ozone precursors by Greenstone et al. (2012). The independent variables are lagged by one period. *Industry-Year F.E* are separate year dummies for each of the seven dirty industries, in addition to one set of year dummies for the remaining clean industries. $p < 0.1$ *, $p < 0.05$ **, $p < 0.01$ ***.

Table 3: INTRAFIRM LEAKAGE: INTENSIVE MARGIN

	(1)	(2)	(3)	(4)	(5)	(6)
1+ other treated plants	0.0418	0.248***	0.273***			
	(0.112)	(0.0901)	(0.0917)			
1 other treated plant				0.0409	0.241**	0.261***
				(0.111)	(0.0970)	(0.0979)
2+ other treated plants				0.0518	0.340	0.502**
				(0.185)	(0.206)	(0.228)
Plant F.E.	yes	yes	yes	yes	yes	yes
Year F.E.	yes	no	no	yes	no	no
County-Year F.E.	no	yes	yes	no	yes	yes
Industry-Year F.E.	no	no	yes	no	no	yes
Observations	59034	47137	47078	59031	47137	47078

An observation is a plant-year. Standard errors, double clustered at the firm and five-digit NAICS-level, are reported in parentheses. The dependent variable is the natural logarithm of plant-level air emissions of VOC. In columns (1)-(3), the independent variable, lagged by one period, is a dummy for plants that are part of a firm and industry (5-digit NAICS) that is regulated elsewhere. Columns (4)-(6) display estimates for binned counts of treated plants. *Industry-Year F.E* are separate year dummies for each of the seven dirty industries, in addition to one set of year dummies for the remaining clean industries. $p < 0.1$ *, $p < 0.05$ **, $p < 0.01$ ***.

Table 4: MECHANISM FOR INTRAFIRM LEAKAGE: TRADABILITY

Tradbility Index:	Co-Location Index				Geographical Herfindahl Index			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Q_1 \times$ other treated	-0.126	0.122			-0.0930	-0.0149		
	(0.0831)	(0.255)			(0.0625)	(0.241)		
$Q_2 \times$ other treated	-0.169**	0.105			-0.0316	0.331**		
	(0.0766)	(0.156)			(0.121)	(0.164)		
$Q_3 \times$ other treated	0.300**	0.384***			0.266	0.318***		
	(0.141)	(0.0946)			(0.174)	(0.0783)		
1+ other treated plants			0.177	0.360***			2.214*	1.552
			(0.139)	(0.124)			(1.307)	(1.589)
Tradable Index $\times$ other treated			0.741	0.734			0.400*	0.244
			(0.478)	(0.735)			(0.233)	(0.304)
p-value: $Q_1 = Q_3$	0.0126	0.3738			0.0552	0.1927		
Plant F.E.	yes	yes	yes	yes	yes	yes	yes	yes
Year F.E	yes	no	yes	no	yes	no	yes	no
County-Year F.E.	no	yes	no	yes	no	yes	no	yes
Industry-Year F.E.	no	yes	no	yes	no	yes	no	yes
Observations	57982	46148	57982	46148	57982	46148	57890	46148

An observation is a plant-year. Standard errors, double clustered at the firm and NAICS-level, are reported in parentheses. The dependent variable is the natural logarithm of VOC air emissions. *other treated* is a dummy, lagged by one period, for plants that are part of a firm and industry (5-digit NAICS) that is regulated elsewhere. The industry-level tradability index in columns (1)-(4) is constructed similar to one proposed by Giroud and Rauh (forthcoming). An index close to the one suggested by Mian and Sufi (2014) is used in columns (5)-(8). Both are time-constant and constructed at the four-digit NAICS level. In columns (1)-(2) and (5)-(6), leakage effects are estimated separately for each quartile of the tradability index. Very few within-firm leakage candidates fall within Q_4 for both indices. These plants are therefore added to the third quartiles. The p-values are for an F-Test of equality of coefficients for the top and bottom quartile. $p < 0.1$ *, $p < 0.05$ **, $p < 0.01$ ***.

Table 5: INTRAFIRM LEAKAGE: EXTENSIVE MARGIN

	(1)	(2)	(3)	(4)	(5)	(6)
1+ other treated plants	0.00955	0.00927	0.00769			
	(0.0211)	(0.0219)	(0.0230)			
1 other treated plant				0.00151	0.00285	0.000785
				(0.0217)	(0.0218)	(0.0233)
2+ other treated plants				0.0781**	0.0775*	0.0917**
				(0.0336)	(0.0427)	(0.0449)
Plant F.E.	yes	yes	yes	yes	yes	yes
Year F.E.	yes	no	no	yes	no	no
County-Year F.E.	no	yes	yes	no	yes	yes
Industry-Year F.E.	no	no	yes	no	no	yes
Observations	109450	96569	96526	109450	96569	96526

An observation is a plant-year. Standard errors, double clustered at the firm and five-digit NAICS-level, are reported in parentheses. The dependent variable equals one in all years a plant operates and zero otherwise. Plant-year observations where the firm-segment is not listed in the Toxic Release Inventory are excluded. In columns (1)-(3), the independent variable is a dummy for plants that are part of a firm and industry (5-digit NAICS) that is regulated elsewhere. Columns (4)-(6) display estimates for binned counts of treated plants. $p < 0.1$ *, $p < 0.05$ **, $p < 0.01$ ***.

Table 6: EXTENSIVE MARGIN: BY PRODUCTIVITY

	(1)	(2)	(3)	(4)	(5)	(6)
$Q_1 \times$ other treated	-0.108**	-0.107	-0.149			
	(0.0475)	(0.0648)	(0.0935)			
$Q_2 \times$ other treated	-0.0750**	-0.0393	-0.0570			
	(0.0318)	(0.0437)	(0.0557)			
$Q_3 \times$ other treated	-0.0248	-0.0150	-0.0543			
	(0.0262)	(0.0373)	(0.0368)			
$Q_4 \times$ other treated	0.150***	0.155***	0.204***			
	(0.0450)	(0.0400)	(0.0503)			
1+ other treated plants				-0.0112	-0.00194	-0.0347
				(0.0223)	(0.0198)	(0.0329)
TFP Interaction				0.265***	0.298***	0.345***
				(0.0727)	(0.0550)	(0.124)
p-value: $Q_1 = Q_4$	0.00003	0.00010	0.00190			
Plant F.E.	yes	yes	yes	yes	yes	yes
Year F.E.	yes	no	no	yes	no	no
County-Year F.E.	no	yes	no	no	yes	no
Industry-Year F.E.	no	yes	no	no	yes	no
Firm-Year F.E.	no	no	yes	no	no	yes
Observations	99762	86740	94968	99762	86740	94968

An observation is a plant-year. Standard errors, double clustered at the firm and five-digit NAICS-level, are reported in parentheses. The dependent variable equals one in all years a plant operates and zero otherwise. Plant-year observations where the firm-segment is not listed in the Toxic Release Inventory are excluded. *other treated* is a dummy, lagged by one period, for plants that are part of a firm and industry (5-digit NAICS) that is regulated elsewhere. The p-values are for an F-Test of equality of coefficients for the top and bottom quartile. In columns (4)-(6), the treatment variable is interacted with the continuous TFP measure. $p < 0.1$ *, $p < 0.05$ **, $p < 0.01$ ***.

Table 7: INTRAFIRM LEAKAGE: FIRM-LEVEL

	Intensive Margin				Extensive Margin			
	Unregulated Emissions		All Emissions		# Unregulated Plants			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1+ other treated plants	0.0876		0.0646		0.0804**		0.189**	
	(0.106)		(0.0887)		(0.0403)		(0.0939)	
1 other treated plant		0.0616		0.0439		0.0608*		0.153*
		(0.104)		(0.0882)		(0.0352)		(0.0839)
2+ other treated plants		0.441**		0.372**		0.242***		0.584***
		(0.192)		(0.157)		(0.0887)		(0.218)
Firm-Industry F.E	yes	yes	yes	yes	yes	yes	yes	yes
Industry-Year F.E.	yes	yes	yes	yes	yes	yes	yes	yes
Observations	29725	29725	32695	32695	34069	34069	34069	34069

An observation is a firm-segment-year. Standard errors, clustered on the firm-level, are reported in parentheses (columns (1)-(4); (7)-(8)). In columns (1)-(2), the dependent variable is the natural logarithm of the (log) total of all VOC related air emissions that a firm-segment emits at never regulated plants. (Log) Total VOC emissions across all plants within a firm-segment serve as the dependent variable in columns (3)-(4). In columns (5)-(8), the dependent variable is a count of never regulated plants within the firm-segment. Firm-segment-year observations where the segment is not listed in the Toxic Release Inventory are dropped in columns (5)-(8). Estimation is carried out by means of a Poisson regression in columns (5)-(6), with standard errors robust to overdispersion. The independent variables are dummy variables indicating whether there are at least one, exactly one or more than one regulated plants within the firm-segment. $p < 0.1$ *, $p < 0.05$ **, $p < 0.01$ ***.

A Theoretical Framework

This appendix presents a simple framework, informally described in section 2 of this paper, of multiplant production. I first describe the general set-up I use to analyze firms' incentives to shift emissions between regulated and unregulated plants (intensive margin) and relocate plants from unregulated to regulated areas (extensive margin). On the extensive margin, I add a simple fixed cost and make additional functional form assumptions about demand and technology. This allows me to precisely describe the role of factors that are known to shape exit and relocation decisions of firms, such as firm productivity. I analyze the intensive margin in the general set-up to highlight the importance of market power and technological complementarity.

General Setup.— Each firm operates at most two plants, located in different areas, and employs a polluting input $d_i \in \mathbb{R}_+$ at plant $i = 1, 2$. Input d_i is supplied competitively at price p_{id}. The firm employs both inputs to produce output q using the production technology $f(d_1, d_2)$. The production function $f(d_1, d_2)$ is assumed to be twice differentiable and to exhibit weakly decreasing returns in the use of each input ($f'(d_i) > 0, f''(d_i) \leq 0$). Consistent with the empirical analysis of plant-level emissions, and not input use, I assume that d_i is expressed in terms of its contribution to emissions. By increasing its use of polluting inputs, the firm also increases its emissions. The firm sells the output q at price $p(q)$. The role of price taking behavior in the output market is discussed below, but I assume that inverse demand $p(q)$ is (weakly) downward sloping ($p'(q) \leq 0$), with $p'(q) = 0$ corresponding to perfect competition in the output market. If the firm has market power ($p'(q) < 0$), this demand function is assumed to be sufficiently elastic such that the firm's first-order conditions for optimal input choices identify a profit maximum.[29]

Environmental Regulation.— In the context of the regulation of VOC emissions in ozone polluted areas, EPA guidelines mandate substitutions towards cleaner inputs or the implementation of technical changes in the manufacturing process.[30] Since input d_i is expressed in terms of its contribution to emissions, this input switching amounts to a reduction in d_i. A reduced-form mechanism with the same effect as these input switching requirements is therefore to simply assume that the price of input d_i increases.

A.1 Intensive Margin

I first analyze how firms operating two plants change their use of dirty inputs in response to environmental regulation at one of those plants. Multiplant firms face the following optimization problem:

[29] Under perfect competition, an interior solution for profit maximizing input choices is only guaranteed if there are decreasing returns to scale in the use of each input ($f''(d_i) < 0$). Since I allow the market for q to be either perfectly or imperfectly competitive on the intensive margin, I additionally impose $f''(d_i) < 0$ for that part of the analysis. To analyze the extensive margin, I assume constant returns to scale - for tractability reasons - and imperfect competition.

[30] See Appendix 4A.4 in EPA (2014) for an overview.

$$\begin{aligned} \max_{d_1,d_2} \quad & \pi = p(q)q - d_1 p_{1d} - d_2 p_{2d} \\ \text{s.t.} \quad & f(d_1, d_2) \geq q. \end{aligned} \tag{7}$$

Modeling multiplant firms in this way can be interpreted in two ways: One is that each plant the firm owns carries out a distinct step in the production process of the final good q. According to this interpretation, multiplant firms feature an internal supply chain from plant 1 to plant 2 (or vice versa). For such firms, the production function satisfies $\frac{\partial^2 f(d_1,d_2)}{\partial d_1 \partial d_2} > 0$, a property referred to as *normal* (Rader, 1968). If firms' plants are not integrated in this way, we can write $q = f(d_1, d_2) = g(d_1) + h(d_2) = q_1 + q_2$. Both plants can thus also be thought of as producing output goods that are directly sold to consumers. If the production function takes this form, consumers treat outputs sold by the two plants within the firm as perfect substitutes, implying that they are sold at the same price $p(q)$. Allowing for a more general demand interdependency leads to similar qualitative implications as long as consumers substitute between outputs.[31]

With this clarification in place, we can proceed to solve for the optimal input choices by taking first-order conditions with respect to d_1 and d_2. The two first-order conditions are:

$$\frac{\partial \pi}{\partial d_i} = \underbrace{\frac{\partial f(d_1, d_2)}{\partial d_i}}_{MP(d_i)} \underbrace{[p(q) + \frac{\partial p(q)}{\partial q} q]}_{MR} - p_{id} = 0, \quad i = 1, 2. \tag{8}$$

Each input d_i is chosen such that its marginal revenue product is equal to its cost p_{id}. Since environmental regulation is akin to an increase in the price of an input, the object of interest is the cross-effect on the use of dirty inputs at the unregulated plant.

To fix ideas, suppose that environmental regulation of plant 1 leads to a rise in p_{1d}. To find the effect this has on the unregulated plant, we apply the implicit function theorem to the first-order conditions (8). We obtain that

$$\frac{\partial d_1^*}{\partial p_{1d}} = \frac{\frac{\partial^2 f(d_1,d_2)}{\partial d_2^2} MR + \frac{\partial f(d_1,d_2)}{\partial d_2} \frac{\partial MR}{\partial d_2}}{\det(H_d)} \tag{9}$$

$$\frac{\partial d_2^*}{\partial p_{1d}} = -\frac{\frac{\partial^2 f(d_1,d_2)}{\partial d_1 \partial d_2} MR + \frac{\partial f(d_1,d_2)}{\partial d_2} \frac{\partial MR}{\partial d_1}}{\det(H_d)}, \tag{10}$$

where $\det(H_d) > 0$ is the determinant of the Hessian associated with the optimization problem in (7). Since marginal revenue curves (weakly) slope downwards and the production function is

[31] Assuming that non-integrated firms ($\frac{\partial^2 f(d_1,d_2)}{\partial d_1 \partial d_2} = 0$) produce two outputs that are perfect substitutes is convenient because the behavior of these firms can then be analyzed within the same framework as that of vertically integrated firms ($\frac{\partial^2 f(d_1,d_2)}{\partial d_1 \partial d_2} > 0$).

assumed to satisfy $f''(d_i) < 0$, the own price effect, as given by (9), is negative. The sign of (10) is ambiguous because d_1 has a negative effect on the marginal product of d_2 (first term), but a positive effect on its marginal revenue product (second term). Based on these equations we can describe the effects of regulation in this set-up as follows:

Proposition 1. Assume the production function satisfies $f''(d_i) < 0$. After regulation, partially regulated firms

(a) decrease their use of polluting input d_1 at the regulated plant.

(b) increase their use of polluting input d_2 at the unregulated plant whenever $|\frac{\partial^2 f(d_1,d_2)}{\partial d_1 \partial d_2} MR| < |\frac{\partial f(d_1,d_2)}{\partial d_2} \frac{\partial MR}{\partial d_1}|$.

To provide further economic intuition for part (b), I discuss two special cases of the model, where no positive leakage arises, as well as the general case.

Special Case 1: Perfect Competition and Independent Marginal Products. In terms of the mechanics of the model, perfect competition and independent marginal products imply that the optimality conditions for d_1 and d_2 are entirely independent of one another. The two assumptions are akin to assuming that plants are operated by different firms. In the partial equilibrium context considered here, cost shocks to one plant leave the other plant the firm operates unaffected. Leakage effects are thus zero at the firm-level. I abstract from leakage based on general equilibrium reallocations for reasons outlined in the main text.

Special Case 2: Perfect Competition and Normal Production. Consider now the case where inputs are complementary ($\frac{\partial^2 f(d_1,d_2)}{\partial d_1 \partial d_2} > 0$), while maintaining the assumption of perfect competition. In that case, input use at the unregulated plant declines in response to regulation at the firm's other plant, making the inputs gross complements (Rader, 1968). Complementarity across plants implies that the regulation hurts the firm twice. It forces the firm to cut back at the regulated plant, which in turn negatively affects the profitability of employing d_2. The model thus predicts negative leakage, i.e. emissions decreases at not directly affected plants, for perfectly competitive firms whose production technology features a complementarity in input use across plants.

General Case: Imperfect Competition and Normal Production. In the present set-up, d_2 can only increase at unregulated plants if the firm has market power ($\frac{\partial MR}{\partial q} < 0$). The price increase of d_1 decreases the firm's ouput q and increases marginal revenue, providing a profit incentive to expand the use of d_2. Input use at the unregulated plant increases if this substitution effect is larger in absolute terms than the countervailing effect of technological complementarity.

Part (a) and (b) of Proposition (1) are explored in Tables 2 and 3 respectively.

We can combine (9) and (10) to get the effect of an increase in p_{1d} on total input use:

$$\frac{\partial(d_1^* + d_2^*)}{\partial p_{1d}} = \frac{\left(\frac{\partial^2 f(d_1,d_2)}{\partial d_2^2} - \frac{\partial^2 f(d_1,d_2)}{\partial d_1 \partial d_2}\right) MR + \left(\frac{\partial MR}{\partial d_2} - \frac{\partial MR}{\partial d_1}\right) \frac{\partial f(d_1,d_2)}{\partial d_2}}{\det(H_d)}. \tag{11}$$

The sign of (11) is ambiguous. If marginal revenue is more sensitive to d_1 than to d_2 (second term is positive), then any given regulation-induced reduction in d_1 requires a more pronounced offsetting increase in d_2. Total use of polluting inputs increases if this effect outweighs the negative effect of diminishing returns and potential complementarities in input use (first term). This is stated in Proposition 2.

Proposition 2. Assume the production function satisfies $f''(d_i) < 0$. After regulation, partially regulated firms increase their total use of polluting inputs whenever $|\left(\frac{\partial^2 f(d_1,d_2)}{\partial d_2^2} - \frac{\partial^2 f(d_1,d_2)}{\partial d_1 \partial d_2}\right) MR| < |\left(\frac{\partial MR}{\partial d_2} - \frac{\partial MR}{\partial d_1}\right) \frac{\partial f(d_1,d_2)}{\partial d_2}|$.

Recall that changes in the use of polluting inputs are assumed to vary directly with emissions. Proposition 2 thus implies that the offsetting effects of partial regulation may amount to an increase in emissions at the firm-level. This possibility is explored in Table 7.

A.2 Extensive Margin

Increases in the variable cost of producing at one plant can also serve as an incentive to open up a plant in a unregulated area of the country. To analyze this possibility theoretically, I assume firms can move plants to unregulated areas by paying an additional fixed cost. I embed this tradeoff in a parameterized version of the general set-up previously outlined. By assuming that firms are monopolists and face constant elasticity demand, the model becomes a simplified version of the sourcing model introduced by Antràs and Helpman (2004).

The model aims to describe the relocation choice from the perspective of a firm eventually exposed to regulation at one of its plants. I assume firms only operate one plant to produce output q using the technology $f(d_i) = \theta d_i$. θ is the firm's productivity level. Constant returns to scale allow me derive intuitive, parametric expressions for firm profits which I use to illustrate the role of productivity and cost differences (robustness to alternative functional form assumptions is discussed below). The firm can employ the input d_i at a potentially regulated plant ($i = 1$) under production mode H (home-production), or abandon this plant and move to an unregulated area under production mode O (outsourcing).

Either operating mode entails a fixed cost, with the fixed cost under outsourcing, f_O, being strictly greater than the one for continuing to produce at the same plant f_H. These higher fixed costs capture additional expenses related to moving and starting up a new plant. The cost functions for each operating mode are

$$C^H(q, p_{1d}) = f_H + \frac{q}{\theta} p_{1d} \tag{12}$$

and

$$C^O(q, p_{2d}) = f_O + \frac{q}{\theta} p_{2d}. \tag{13}$$

The pricing decision is made tractable by assuming that firms face a CES inverse demand function

$$p(q) = \beta q^{-\frac{1}{\sigma}} \tag{14}$$

with elasticity of substitution $\sigma > 1$. In models of monopolistic competition, β depends on an aggregate of other firms' prices as well as consumer spending. I maintain a partial equilibrium approach and treat β as a constant.

Unregulated Economy.— Assume first that there are no regulatory differences, so that the variable cost p_{id} of using polluting input d_i are the same everywhere. Since relocation entails a fixed cost $f_O > f_H$, a firm would only pay this fixed cost in the model to escape regulation. No firm therefore relocates in the absence of environmental regulation.

Having chosen H as its preferred production mode, firms maximize their profits subject to the consumer's demand function (14). As a function of the parameters of the model and prices, the profits are

$$\pi^H = B\theta^{\sigma-1} p_{1d}^{1-\sigma} - f_H, \tag{15}$$

where $B = \frac{1}{\sigma}(\frac{\sigma}{\sigma-1})^{1-\sigma}\beta^{\sigma}$.

Regulated economy.— CAA regulation is again assumed to increase the price of employing dirty inputs at plant 1, p_{1d}. Once a firm is regulated, $p_{1d} > p_{2d}$. If a firm continues to operate within a regulated area, its profits are as in equation (15). Alternatively, it can outsource its production to an unregulated area, where it pays the lower price p_{2d} on the input that is now regulated at plant 1. This requires paying a fixed cost $f_O > f_H$, but brings the benefit of evading the higher price on polluting inputs. The maximized profit function associated with this problem, conditional on choosing to outsource is:

$$\pi^O = B\theta^{\sigma-1} p_{2d}^{1-\sigma} - f_O. \tag{16}$$

Results.— Within this set-up, we can ask how environmental regulation affects firms' outsourcing behavior on the extensive margin. Thus, the extensive margin of sourcing in this model is defined as the decision to open a plant in an unregulated county. To analyze the economic forces shaping this decision, I take the difference in profit between an O firm and a H firm after p_{1d} has increased due to regulation.

$$\pi^O - \pi^H = B\theta^{\sigma-1}\left[p_{2d}^{1-\sigma} - p_{1d}^{1-\sigma}\right] - (f_O - f_H). \tag{17}$$

The necessary and sufficient conditions for firms to open a new plant are that this difference is positive and profits are weakly positive under outsourcing. Environmental regulation lowers profits overall, either through higher variable or fixed cost, such that it will lead some firms to exit (see below). The two terms in (17) are easily interpreted. The first term captures the difference in variable profits. A sufficient condition for it to be positive is that $\sigma > 1$ (as required for the firm's first-order condition) and that input prices are higher in regulated areas ($p_{1d} > p_{2d}$). The second term is related to the difference in fixed cost between the two sourcing modes and is also greater than zero. Deciding between home-production and outsourcing, firms trade off higher variable profits with higher fixed costs. Proposition 3 shows that such differences in variable profits can lead firms to relocate.

Proposition 3. After regulation, all firms that remain profitable relocate whenever $p_{1d} > \left(\frac{1}{p_{2d}^{\sigma-1}} - \frac{f_O - f_H}{B\theta^{\sigma-1}}\right)^{\frac{1}{1-\sigma}}$.

Proposition 3 follows from solving $\pi^O - \pi^H > 0$ for p_{1d}. If regulation results in a sufficiently large increase in p_{1d}, firms relocate. Firms may prefer outsourcing O over home-production H, even though neither operating mode is profitable. Firms only relocate, however, if they are able to operate profitably given the larger fixed cost under outsourcing. I test for such relocation effects in Table 5.

Further comparative statics can be derived by considering how productivity shapes the outsourcing decision. Graphically and intuitively this was illustrated in Figure 1.

Proposition 4 describes this more formally.

Proposition 4. After regulation and whenever $\left(\frac{p_{1d}}{p_{2d}}\right)^{\sigma-1} < \frac{f_O}{f_H}$,

(a) high productivity firms with $\theta > \tilde{\theta}$, where $\tilde{\theta} = \left(\frac{f_O - f_H}{B\left[p_{2d}^{1-\sigma} - p_{1d}^{1-\sigma}\right]}\right)^{\frac{1}{\sigma-1}}$, relocate.

(b) previously active, low productivity firms with $\theta < \hat{\theta}$, where $\hat{\theta} = \left(\frac{f_h}{B}\right)^{\frac{1}{\sigma-1}} p_{1d}$, exit.

(c) firms with an intermediate level of productivity, $\tilde{\theta} > \theta > \hat{\theta}$, neither exit nor relocate.

The predictions made in Proposition 4 are explored in Tables 6 and B.6.

While the nature of these selection effects appears intuitive, they do depend on the shape of the demand function. It is not necessarily the case that only the most productive firms select into outsourcing if utility is not CES (Mrázová and Neary, forthcoming; Mukherjee, 2010). Further analysis would also be required to consider whether more productive firms profit the most from lower input prices under decreasing returns to scale in the production function. Similar to the case of non-CES demands, decreasing returns imply that it may not generally be possible to write ex-post profits as a linear function of a transformation of productivity. The absence of such a convenient functional form complicates the analysis. Proposition 3, by contrast, also hold under decreasing returns to scale.

B Empirical Background

B.1 Regulatory Strategy

In principle, the CAA creates transparent variation in the stringency of environmental regulation across counties. This variation could, for instance, be exploited via difference-in-differences research designs, comparing VOC emissions of plants in attainment and nonattainment counties before and after regulation comes into place. Such an approach to evaluate the CAA is only valid if regulators target pollution sources in nonattainment counties uniformly. Many studies suggest that effects across monitor stations, plants or industries in nonattainment counties are very heterogeneous (Greenstone, 2002; Auffhammer et al., 2009; Gibson, forthcoming). Constrained by limited available resources, regulators aim for emissions reductions at the sources that were most likely to have pushed the county into nonattainment in the first place. This has clear implication for studying the within-firm leakage consequences of regulation. Leakage refers to the reallocation of activity from regulated to unregulated unit, which can only be studied if there are well defined differences in exposure to regulation. Accurately assessing the effects of CAA regulation thus becomes the task of distinguishing between targets and non-targets within nonattainment areas.

A further complication is that enforcement procedures differ across pollutants. In the case of regulation of PM, Gibson (forthcoming) shows that regulators only target the areas within in a county that are in close vicinity to the monitoring station that forced the county into nonattainment. They argue that this is partially explained by PM remaining concentrated around the source of emission. An intuitive metric for this concentration is the share of monitors within nonattainment counties that violate the NAAQS. If pollution is limited to certain spots within nonattainment counties, there should remain a significant fraction of monitors that does not record a violation of the pollution threshold. For PM, only about 31% of all monitors detect pollution levels above the threshold. In ozone nonattainment counties, 76% of all operating monitors detect violations of the NAAQS according to the 8-hour rule.[32] These differences in concentration have consequences for enforcement. For PM, regulators can bring a county into attainment by only targeting plants close to the violating monitor (Gibson, forthcoming). High levels of ozone, by contrast, appear to be pervasive throughout a county, so that a narrow geographical focus is unlikely to bring counties back into attainment.

Considering the density of ozone pollution within nonattainment counties, cost-effective regulation is more likely to focus on major emitters within a county.

[32] Both figures based on own calculation.

B.2 External Validity

The sampling criteria used by the TRI combined with the focus on publicly listed companies are clear threats to the external validity of the findings presented in this paper. Reporting to the TRI is only mandatory for relatively large and heavily polluting industrial plants. The focus on publicly listed firms furthers this emphasis. Compared to their private counterparts, listed firms are known to be significantly larger on average and display differential investment behavior.[33] I argue that there are empirical reasons to focus on this sample to estimate regulatory effects of ozone regulation as well as spillovers.

Becker and Henderson (2000) find that regulators in ozone nonattainment counties focus on a set on larger plants that are part of multiplant conglomerates. Plants underneath the size threshold for inclusion in the TRI are hence unlikely to face significant regulatory barriers. The effect of regulation on plants belonging to smaller, private firms can easily be investigated using the main sample. As I show below, within the sample of non-listed firms, county nonattainment status is not associated with emissions reductions. Choosing a sample of public firms is thus also appropriate for the estimation of within-firm leakage effects. This is because leakage only arises in the model if regulation negatively affects directly affected plants. I therefore take it to be a sensible point of departure to consider spillovers from treated to untreated units for a set firms measurably affected in their emissions behavior.

Beyond differences in the direct effect of regulation, the focus on large firms may have distinct consequences for within-firm leakage. These follow straightforwardly from the simple theory of multiplant input choice. In that framework, substitutability requires interdependent demand across plants. Shifting emissions is thus more feasible for public firms since they are less dependent on local demand than their smaller, private counterparts. Second, Traina (2018) shows that larger firms, within the Compustat universe, also charge significantly higher mark-ups. Under the assumption that this positive association holds across public and private firms, one would expect public firms to be more likely to shift dirty production between plants. This relation holds in the theory because high mark-up firms face a low elasticity of demand, which increases the degree of strategic substitutability between plants' input choices.[34]

In sum, I test whether regulation that curbs emissions as intended leads to within-firm leakage. The particular enforcement of the CAA, with regulators focusing on larger plants that are often part of firms with greater ability to shift production between plants should additionally be kept in mind. My findings may be externally valid in this limited sense.

[33] See the recent study by Feldman, Kawano, Patel, Rao, Stevens, and Edgerton (2018) and references therein.

[34] The potential for leakage may also shape the direct response to regulation. To the extent firms know they can shift emissions elsewhere, they may, more readily, reduce them to evade costly regulations. While plausible, this hypothesis is hard to disentangle empirically from the previously raised possibility of CAA enforcement focusing on large plants that are often part of public firms.

B.3 Details on Tradability Indices

Two tradability indicies are employed in Table 4. One is the geographical Herfindahl index by Mian and Sufi (2014). For industry j, this index is formally defined as $\sum_c \theta_{cj}^2$, where c indexes counties and θ_{cj} is the number of establishments in county c and industry j divided by the total number of industry j establishments in the US. Geographically concentrated industries depend on agglomeration economies, whereas dispersed industries, with a low value of this index, rely on local demand. In Mian and Sufi (2014), the authors mainly use this index to distinguish tradable manufacturing establishments from non-tradable services. There remains, however, substantial variation after excluding industries not listed int the TRI. This is evidenced by the similar mean and standard deviation the index has within the subsample of 110 four-digit NAICS industries in the TRI (Mean: 0.0084 vs. 0.0081; Standard Deviation is equal at 0.013) as across all 294 industries.

Giroud and Rauh (forthcoming) propose an index with a stronger focus on transportation cost and local demand. Their index is defined as $\sum_p |s_{pj} - s_p|$, where s_{pj} is the number of establishments in industry j and state p divided by the number of establishments in state p. s_p is the number of establishments in state p divided by the number of establishments in the US economy. If an industry has high transportation costs, it will be distributed proportionally to the demand in each market. Non-tradable industries should hence have a low value of this index. Descriptively, these measures have a rank correlation of 0.23 (p-value: 0.016).

Both indices are constructed using County Business Patterns (CBP) data on the number of establishments in each county (state) and industry. The establishment counts are disaggregated by industry. The median value of the indices across years 1998-2014 is taken. 1998 is the first year NAICS classifications are in active use in the CBP.

B.4 Additional Tables

Table B.1: DESCRIPTIVE STATISTICS AND BALANCE

	Unregulated			Regulated		
Variable	Observations	Mean	Std. Deviation	Observations	Mean	Std. Deviation
Plant-Year Level						
Air Emissions	74,784	84,836	277,202	6,667	101,676	266,786
Firm-Year Level						
(Real) Sales ($100 Mio.)	9954	77	204.8	2122	154	403
# Plants	15,872	3.2	5.4	3243	9.2	11.3
Firm-level						
Productivity	1037	-0.006	0.41	200	0.013	0.44
Emissions Intensity	913	6.95	3.06	182	8.0	2.3
Financial Constraints	771	0.02	0.04	151	0.02	0.048

Regulated plants are those active in a *dirty* industry and who are exposed to CAA ozone regulation in the county they operate in at some point over the sample frame. Firms are classified as regulated if they operate one such plant. See main text and Table 1 for definitions of the variables.

Table B.2: EFFECT OF OZONE NONATTAINMENT: SUB-SAMPLES

	(1)	(2)	(3)	(4)
Panel A: Exclude Never Regulated Plants				
NAA	-0.200**	-0.207***	-0.251***	
	(0.0798)	(0.0762)	(0.0841)	
Observations	7860	7804	7708	
Panel B: Exclude Spillover Plants				
NAA	0.0345	0.0205	-0.0223	
	(0.0487)	(0.0495)	(0.0537)	
NAA $\times$ Dirty Industry	-0.272***	-0.215**	-0.203**	-0.238**
	(0.0864)	(0.0873)	(0.0874)	(0.109)
Plant F.E.	yes	yes	yes	yes
Year F.E.	yes	no	no	no
State-Year F.E.	no	no	yes	no
Industry-Year F.E.	no	yes	yes	yes
County-Year F.E.	no	no	no	yes
Observations	60297	60250	60234	48813

An observation is a plant-year. Standard errors, clustered on the county-level, are reported in parentheses. The dependent variable is the natural logarithm of plant-level air emissions of VOC. *NAA* equals one in all years a county is classified as part of a nonattainment area for ozone according to the EPA Greenbook data and zero otherwise. *Dirty Industry* is a binary indicator equal to one for plants in industries classified as heavy emitters of ozone precursors by Greenstone et al. (2012). The independent variables are lagged by one period. *Industry-Year F.E* are separate year dummies for each of the seven dirty industries, in addition to one set of year dummies for the remaining clean industries. In Panel A, I exclude all plants never subject to regulation. In Panel B, I exclude all plants subject to the spillover treatment (leakage plants). $p < 0.1$ *, $p < 0.05$ **, $p < 0.01$ ***.

Table B.3: Intensive Margin Leakage: Larger Sample

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Unregulated In the Past						
1+ other treated plants	0.0365	0.229**	0.254***			
	(0.111)	(0.0916)	(0.0940)			
1 other treated plant				0.0362	0.224**	0.245**
				(0.110)	(0.0968)	(0.0988)
2+ other treated plants				0.0405	0.298	0.442**
				(0.173)	(0.182)	(0.200)
Observations	59644	47776	47713	59641	47776	47713
Panel B: Currently Unregulated						
1+ other treated plants	0.0199	0.204**	0.221**			
	(0.108)	(0.0851)	(0.0883)			
1 other treated plant				0.0185	0.198**	0.212**
				(0.104)	(0.0901)	(0.0931)
2+ other treated plants				0.0350	0.289*	0.415**
				(0.163)	(0.161)	(0.165)
Plant F.E.	yes	yes	yes	yes	yes	yes
Year F.E.	yes	no	no	yes	no	no
County-Year F.E.	no	yes	yes	no	yes	yes
Industry-Year F.E.	no	no	yes	no	no	yes
Observations	60924	49041	48975	60921	49041	48975

An observation is a plant-year. Standard errors, double clustered at the firm and five-digit NAICS-level, are reported in parentheses. The dependent variable is the natural logarithm of plant-level air emissions of VOC. In columns (1)-(3), the independent variable, lagged by one period, is a dummy for plants that are part of a firm and industry (5-digit NAICS) that is regulated elsewhere. Columns (4)-(6) display estimates for binned counts of treated plants. *Industry-Year F.E* are separate year dummies for each of the seven dirty industries, in addition to one set of year dummies for the remaining clean industries. $p < 0.1$ *, $p < 0.05$ **, $p < 0.01$ ***.

Table B.4: Mechanism for Intrafirm Leakage: Effects across all plants

	(1)	(2)	(3)	(4)	(5)	(6)
1+ other treated plants	-0.0722	-0.0297	-0.0256			
	(0.0660)	(0.0498)	(0.0501)			
1 other treated plant				-0.0663	-0.0311	-0.0312
				(0.0602)	(0.0500)	(0.0500)
2+ other treated plants				-0.0978	-0.0233	-0.00143
				(0.0726)	(0.0587)	(0.0588)
Plant F.E.	yes	yes	yes	yes	yes	yes
Year F.E.	yes	no	no	yes	no	no
County-Year F.E.	no	yes	yes	no	yes	yes
Industry-Year F.E.	no	no	yes	no	no	yes
Observations	59034	47137	47078	59031	47137	47078

An observation is a plant-year. Standard errors, double clustered at the firm and five-digit NAICS-level, are reported in parentheses. The dependent variable is the natural logarithm of plant-level air emissions of VOC. In columns (1)-(3), the independent variable, lagged by one period, is a dummy for plants that are part of a firm that is regulated elsewhere. Columns (4)-(6) display estimates for binned counts of treated plants. *Industry-Year F.E* are separate year dummies for each of the seven dirty industries, in addition to one set of year dummies for the remaining clean industries. $p < 0.1$ *, $p < 0.05$ **, $p < 0.01$ ***.

Table B.5: MECHANISM FOR INTRAFIRM LEAKAGE: FINANCIAL CONSTRAINTS

Measure of Financial Constraints:	Text-based Measure				KZ Index			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Q_1 \times$ other treated	0.349**	0.582**			0.198	0.416**		
	(0.138)	(0.229)			(0.354)	(0.175)		
$Q_2 \times$ other treated	0.209	0.304*			0.164	0.613***		
	(0.199)	(0.156)			(0.157)	(0.219)		
$Q_3 \times$ other treated	-0.206	-0.161			0.0892	0.147		
	(0.165)	(0.286)			(0.119)	(0.140)		
$Q_4 \times$ other treated	0.0267	0.117			-0.347**	-0.279		
	(0.150)	(0.215)			(0.148)	(0.207)		
1+ other treated plants			0.184	0.355***			0.138	0.491***
			(0.133)	(0.129)			(0.159)	(0.165)
FC (Text) $\times$ other treated			-2.775	-4.394**				
			(1.880)	(2.057)				
FC (KZ) $\times$ other treated							-0.126	-0.446***
							(0.150)	(0.171)
p-value: $Q_1 = Q_4$	0.1560	0.1527			0.1349	0.0307		
Plant F.E.	yes	yes	yes	yes	yes	yes	yes	yes
Year F.E.	yes	no	yes	no	yes	no	yes	no
County-Year F.E.	no	yes	no	yes	no	yes	no	yes
Industry-Year F.E.	no	yes	no	yes	no	yes	no	yes
Observations	42898	31487	42898	31487	48234	36504	48234	36504

An observation is a plant-year. Standard errors, double clustered at the firm and five-digit NAICS-level, are reported in parentheses. The dependent variable is the natural logarithm of VOC air emissions. *other treated* is a dummy, lagged by one period, for plants that are part of a firm and industry (5-digit NAICS) that is regulated elsewhere. Columns (1)-(4) use a measure of financial constraints constructed by Hoberg and Maksimovic (2014) based on text-analysis of 10k forms. Models in columns (5)-(8) use the Kaplan and Zingales (1997) measure of financial constraints. In columns (1)-(2) and (5)-(6), leakage effects are estimated separately for firms in each quartile of the financial constraints variable. The p-values are for an F-Test of equality of coefficients for the top and bottom quartile. The interaction term in column (3)-(4), FC (Text) $\times$ other treated, is defined as the product of the continuous, text-based measure of financial constraints and the treatment dummy. Columns (7)-(8) use an interaction of the treatment variable and a dummy variable equal to one for firms in the upper tercile of the empirical distribution of the Kaplan-Zingales measure (FC (KZ) $\times$ other treated). $p < 0.1$ *, $p < 0.05$ **, $p < 0.01$ ***.

Table B.6: Plant Exit

	Treatment Effects			By TPF Quartile		
	(1)	(2)	(3)	(4)	(5)	(6)
NAA	0.000126			-0.000951		
	(0.00604)			(0.00621)		
$NAA\times$ Dirty Industry	0.0226*	0.00805	-0.0111			
	(0.0116)	(0.0141)	(0.0132)			
$Q_1\times$ NAA $\times$ Dirty Industry				0.0685***	0.0427*	0.00544
				(0.0205)	(0.0229)	(0.0214)
$Q_2\times$ NAA $\times$ Dirty Industry				0.0619***	0.0467*	0.0279
				(0.0189)	(0.0250)	(0.0265)
$Q_3\times$ NAA $\times$ Dirty Industry				-0.00580	-0.0341	-0.0421*
				(0.0183)	(0.0238)	(0.0223)
$Q_4\times$ NAA $\times$ Dirty Industry				0.00827	-0.0163	-0.0304*
				(0.0155)	(0.0181)	(0.0177)
p-value: $Q_1 = Q_4$				0.0065	0.0183	0.1255
Plant F.E.	yes	yes	yes	yes	yes	yes
Year F.E.	yes	no	no	yes	no	no
County-Year F.E.	no	yes	yes	no	yes	yes
Industry-Year F.E.	no	no	yes	no	no	yes
Observations	77911	65912	65845	70779	58717	58631

An observation is a plant-year. Standard errors, clustered at county-level, are reported in parentheses. The dependent variable equals one the last year the plant is observed. The last year of the sample is excluded. *NAA* equals one in all years a county is classified as part of a nonattainment area for ozone according to the EPA Greenbook data and zero otherwise. *Dirty Industry* is a binary indicator equal to one for plants operating in industries classified as heavy emitters of ozone precursors by Greenstone et al. (2012). Columns (4)-(6) show estimates of the treatment effect across quartiles Q_i of the empirical distribution of TFP, as defined in the text. The p-values are for an F-Test of equality of coefficients for the top and bottom quartile. $p < 0.1$ *, $p < 0.05$ **, $p < 0.01$ ***.

Table B.7: EFFECT OF OZONE NONATTAINMENT: ALTERNATIVE INDUSTRY CLASSIFICATION

Dirty Industry Definition:	Wider Set		Only Marginal Industries	
	(1)	(2)	(3)	(4)
NAA	0.0492		0.0325	
	(0.0461)		(0.0477)	
NAA $\times$ Dirty Industry	-0.168**	-0.181**	-0.0378	-0.0421
	(0.0726)	(0.0880)	(0.108)	(0.107)
Plant F.E.	yes	yes	yes	yes
Year F.E.	yes	no	yes	no
Industry-Year F.E.	no	yes	no	yes
County-Year F.E.	no	yes	no	yes
Observations	66891	55069	50881	39782

An observation is a plant-year. Standard errors, clustered on the county-level, are reported in parentheses. The dependent variable is the natural logarithm of plant-level air emissions of VOC. *NAA* equals one in all years a county is classified as part of a nonattainment area for ozone according to the EPA Greenbook data and zero otherwise. In columns (1)-(2), *Dirty Industry* is a binary indicator equal to one for plants in industries classified as heavy emitters of ozone precursors by Greenstone (2002). In columns (3)-(4), all plants in industries at the intersection of the *Dirty Industry* definitions by Greenstone (2002) and Greenstone et al. (2012) are dropped. The independent variables are lagged by one period. *Industry-Year F.E* are separate year dummies for each of the 18 dirty industries, in addition to one set of year dummies for the remaining clean industries. $p < 0.1$ *, $p < 0.05$ **, $p < 0.01$ ***.

Table B.8: INTRAFIRM LEAKAGE: ALTERNATIVE INDUSTRY CLASSIFICATION

	Intensive Margin			Extensive Margin		
	(1)	(2)	(3)	(4)	(5)	(6)
Dummy Variable:						
1+ other treated	0.168*	0.325***	0.349***	0.00744	0.0140	0.0150
	(0.0884)	(0.0688)	(0.0725)	(0.0199)	(0.0200)	(0.0187)
Binned Counts:						
1 other treated	0.169*	0.324***	0.347***	0.000298	0.00776	0.00886
	(0.0903)	(0.0735)	(0.0781)	(0.0198)	(0.0197)	(0.0185)
2+ other treated	0.162	0.341*	0.397**	0.0818***	0.0835**	0.0901***
	(0.144)	(0.175)	(0.171)	(0.0302)	(0.0361)	(0.0340)
Plant F.E.	yes	yes	yes	yes	yes	yes
Year F.E.	yes	no	no	yes	no	no
County-Year F.E.	no	yes	yes	no	yes	yes
Industry-Year F.E.	no	no	yes	no	no	yes
Observations	54233	42248	42238	101135	88020	88000

An observation is a plant-year. Standard errors, double clustered at the firm and five-digit NAICS-level, are reported in parentheses. In columns (1)-(3), the dependent variable is the natural logarithm of plant-level air emissions of VOC. In column (4)-(6), the dependent variable equals one in all years a plant operates and zero otherwise. Plant-year observations where the firm-segment is not listed in the Toxic Release Inventory are excluded in columns (4)-(6). In the upper panel, the independent variable is a dummy for plants that are part of a firm and industry (5-digit NAICS) that is regulated elsewhere. The lower panel displays estimates for binned counts of treated plants. The difference to estimates in Tables 3 & 5 is that a wider set of industries is classified as *Dirty*. $p < 0.1$ *, $p < 0.05$ **, $p < 0.01$ ***.

Table B.9: EFFECT OF OZONE NONATTAINMENT: ALL FIRMS

Firm Sample:	Public and Private		Private	
	(1)	(2)	(3)	(4)
NAA	0.0534		0.0635	
	(0.0344)		(0.0474)	
NAA $\times$ Dirty Industry	-0.181***	-0.0730	-0.0597	0.0663
	(0.0665)	(0.0755)	(0.0804)	(0.0990)
Plant F.E.	yes	yes	yes	yes
Year F.E.	yes	no	no	no
County-Year F.E.	no	yes	no	yes
Industry-Year F.E.	no	yes	no	yes
Observations	119239	105552	51963	39187

An observation is a plant-year. Standard errors, clustered on the county-level, are reported in parentheses. The dependent variable is the natural logarithm of plant-level air emissions of VOC. *NAA* equals one in all years a county is classified as part of a nonattainment area for ozone according to the EPA Greenbook data and zero otherwise. *Dirty Industry* is a binary indicator equal to one for plants in industries classified as heavy emitters of ozone precursors by Greenstone et al. (2012). In columns (1)-(2), all VOC emitting plants are used to estimate regulatory effects. The sample in columns (3)-(4) is limited to plants whose parent company has never been publicly listed. The independent variables are lagged by one period. *Industry-Year F.E* are separate year dummies for each of the 7 dirty industries, in addition to one set of year dummies for the remaining clean industries. $p < 0.1$ *, $p < 0.05$ **, $p < 0.01$ ***.

Table B.10: INTRAFIRM LEAKAGE: ALL FIRMS

	Intensive Margin			Extensive Margin		
	(1)	(2)	(3)	(4)	(5)	(6)
Dummy Variable:						
1+ other treated	-0.0425	0.0501	0.0516	0.0327	0.0365	0.0319
	(0.0868)	(0.0845)	(0.0913)	(0.0256)	(0.0269)	(0.0250)
Binned Counts:						
1 other treated	-0.0437	0.0427	0.0418	0.0212	0.0282	0.0233
	(0.0875)	(0.0863)	(0.0930)	(0.0249)	(0.0266)	(0.0249)
2+ other treated	-0.0286	0.159	0.219	0.129***	0.113***	0.120***
	(0.162)	(0.152)	(0.161)	(0.0377)	(0.0363)	(0.0313)
Plant F.E.	yes	yes	yes	yes	yes	yes
Year F.E.	yes	no	no	yes	no	no
County-Year F.E.	no	yes	yes	no	yes	yes
Industry-Year F.E.	no	no	yes	no	no	yes
Observations	104733	90925	90850	183880	170218	170138

An observation is a plant-year. Standard errors, double clustered at the firm and five-digit NAICS-level, are reported in parentheses. In columns (1)-(3), the dependent variable is the natural logarithm of plant-level air emissions of VOC. In column (4)-(6), the dependent variable equals one in all years a plant operates and zero otherwise. Plant-year observations where the firm-segment is not listed in the Toxic Release Inventory are excluded in columns (4)-(6). In the upper panel, the independent variable is a dummy for plants that are part of a firm and industry (5-digit NAICS) that is regulated elsewhere. The lower panel displays estimates for binned counts of treated plants. The difference to estimates in Tables 3 & 5 is that private and public firms are included in the sample. $p < 0.1$ *, $p < 0.05$ **, $p < 0.01$ ***.

Table B.11: BETWEEN-FIRM LEAKAGE

	(1)	(2)	(3)	(4)	(5)	(6)
# Regulated Plants within:						
100 KM	0.00719					
	(0.00906)					
200 KM		0.00815**				
		(0.00319)				
500 KM			0.00518***			
			(0.00105)			
# Regulated Plants in same industry within:						
100 KM ¡ 100 KM				-0.0262		
				(0.0372)		
200 KM					0.0130	
					(0.0286)	
500 KM						-0.00947
						(0.00953)
Plant F.E	yes	yes	yes	yes	yes	yes
Year F.E.	yes	yes	yes	yes	yes	yes
Observations	46703	46703	46703	46703	46703	46703

An observation is a plant-year. Standard errors, clustered on the county-level, are reported in parentheses. The dependent variable is the natural logarithm of plant-level air emissions of VOC Sample consists of plants in attainment counties. Independent variables are counts of regulated plants within varying distance thresholds. In column (4)-(6), the counts are defined over regulated plants in the same five-digit NAICS industry. $p < 0.1$ *, $p < 0.05$ **, $p < 0.01$ ***.

Table B.12: EFFECT OF OZONE NONATTAINMENT: HETEROGENEITY

Firm Sample:	TFP	Fixed Cost	FC	Tradability
	(1)	(2)	(3)	(4)
$Q_1 \times$ NAA $\times$ Dirty	-0.330***	-0.365	-0.208	-0.753***
	(0.127)	(0.291)	(0.147)	(0.262)
$Q_2 \times$ NAA $\times$ Dirty	-0.202	-0.231	0.128	-0.0577
	(0.132)	(0.253)	(0.224)	(0.0818)
$Q_3 \times$ NAA $\times$ Dirty	0.0107	-0.173	-0.273	-0.332**
	(0.155)	(0.128)	(0.169)	(0.140)
$Q_4 \times$ NAA $\times$ Dirty	-0.474**	-0.407**	-0.697*	
	(0.195)	(0.179)	(0.416)	
p-value:	0.4641	0.8793	0.2331	0.1236
Plant F.E.	yes	yes	yes	yes
County-Year F.E.	yes	yes	yes	yes
Observations	48971	46106	37185	53908

An observation is a plant-year. Standard errors, clustered on the county-level, are reported in parentheses. The dependent variable is the natural logarithm of plant-level air emissions of VOC. *NAA* equals one in all years a county is classified as part of a nonattainment area for ozone according to the EPA Greenbook data and zero otherwise. *Dirty* is a binary indicator equal to one for plants in industries classified as heavy emitters of ozone precursors by Greenstone et al. (2012). Across columns (1)-(4), effects are estimated separately for each quartile of the set of variables used in the analysis of leakage mechanisms. Column (1) estimates effects separately by quartile of TFP, (2) by quartile of *Equipment Intensity*, (3) by quartile of the Kaplan and Zingales (1997) measure of financial constraints and (4) by quartile of a measure of tradability, similar to one constructed by Giroud and Rauh (forthcoming). In the last column, the fourth quartile features too few regulated plants to estimate effects separately. The F-Test for equality of coefficients is thus conducted for $Q_1 = Q_3$. $p < 0.1$ *, $p < 0.05$ **, $p < 0.01$ ***.