Do environmental markets improve allocative efficiency? Evidence from U.S. air pollution

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November 2022

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Abstract

Across many domains, market-based interventions hold the promise of reducing costs through improved allocative efficiency in settings where prices are otherwise missing. This claim is also fundamentally challenging to verify: the very absence of prices before a market makes establishing misallocation changes due to the market difficult. This paper develops an empirical framework showing how a theoretical change in allocative efficiency following a policy change can be recovered using a quasi-experimental panel data estimator, without needing input prices. We apply this framework, together with administrative data, to the study of two major U.S. markets for air pollution, a canonical missing markets setting where concerns over high abatement costs have made market-based interventions particularly appealing. We find that for California's RECLAIM program, where a pollution market replaced existing regulation, allocative efficiency improved by 10 percentage points. For the U.S.'s NO_x Budget Program (NBP) in which a pollution market was overlaid onto existing regulation, we do not detect efficiency gains. Heterogeneity analyses suggest plants with pre-existing distortions in capital and labor, and facing restricted abatement options experienced lower allocative efficiency gains. While noisy, these findings shed light on the second-best conditions that may dampen the efficiency gains of pollution markets.

1 Introduction

Economists have long argued that markets facilitate a more efficient allocation of resources. The idea that market forces could direct resources to those that value it more has motivated market-based interventions in education, healthcare, and food provision, among other domains.[1] Determining whether such interventions actually improve allocative efficiency, however, is challenging. Allocative inefficiency is tightly linked with the dispersion in input prices. By definition, input prices are unobserved when markets are missing, making it difficult to establish misallocation before the market, and thus any efficiency changes after it. This paper develops a quasi-experimental framework for estimating allocative efficiency changes when markets are introduced.

We apply this framework to the introduction of environmental markets, a domain where the promise of market-based interventions has been particularly influential, both because pollution is often regarded as a canonical "missing market" problem (Coase, 1960; Arrow, 1969) and because substantial heterogeneity across polluters suggests large allocative efficiency gains. Textbook theory developed over five decades ago established that an environmental market, sometimes known as "cap-and-trade", can achieve a total pollution target at minimum total cost by allocating pollution efficiently (Kneese, 1964; Crocker, 1966; Dales, 1968; Baumol and Oates, 1971; Montgomery, 1972). A subsequent second-best literature questions this prediction arguing that the presence of other distortions can in theory not only dampen first-best efficiency gains but in some cases even lead to efficiency losses when a market-based policy is adopted. Nonetheless, the promise of allocative efficiency gains continues to motivate the adoption of market-based policies in nearly every environmental domain, from fisheries, groundwater, ecosystem services, to the global climate, despite limited empirical support.

Our framework starts with the observation that allocative efficiency for any input occurs when its marginal product is equalized across producers. Distortions drive wedges between a producer's marginal product and the economy-wide input price, increasing misallocation costs by making input prices more disperse. This insight is widely used in the misallocation literature in which dispersion of appropriately-weighted input prices helps to quantify the aggregate productivity consequences of capital or labor misallocation (Restuccia and Rogerson, 2008; Hsieh and Klenow, 2009; Restuccia and Rogerson, 2013). Such an approach, however, is not possible when input prices are missing (or merely "shadow"), as in the case of pollution.

To make progress, we consider an economy-wide model of input allocation in which a producer's (unobserved) input distortion relates to its (observed) average revenue of emissions through a first order condition. This relationship informs our difference-in-differences research design which first recovers residuals of average revenue of emissions after accounting for other key determinants, and then estimates how a pollution market alters the variance of these residuals. We show that under certain structural assumptions, our quasi-experimental estimator recovers a lower bound on the relative change in abatement cost across policies, our theoretical estimand. We further discuss a semi-parametric approach for recovering our estimand.

Our framework has three additional advantages. First, our theory accommodates policies with any arbitrary allocation of inputs, regardless of institutional context. This flexibility allows us to study a wide-range of settings in which the pre-market policy can take on any form and does not pre-specify that a market-based policy necessarily achieves allocative efficiency. Crucially, this means our main statistical test is two-sided: a market-based policy can either decrease or increase allocative inefficiency, as allowed by second-best theory. Second, we allow policies to have different total levels of an input, accommodating the fact that in practice, many market-based environmental policies stipulate a drop in total pollution (i.e., the "cap" in cap-and-trade) in addition to reallocation in pollution. Third, our framework uses a quasi-experimental approach to account for several common

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[1]Examples of market-based interventions can be found in education (Ladd, 2002; Epple, Romano and Urquiola, 2017), healthcare (Roth, Sönmez and Ünver, 2007; Agarwal et al., 2019), food banks (Prendergast, 2022), and for allocating radio spectrum (Milgrom and Segal, 2020).

measurement concerns in the misallocation literature, including producer heterogeneity in market power and output elasticities, and changing macroeconomic conditions over time.

We study the introduction of two major U.S. markets for nitrogen oxides (NO_x): southern California's Regional Clean Air Incentives Market (RECLAIM) and the eastern U.S. NO_x Budget Program (NBP). These markets are notable both because of their scale, covering nearly all major polluting facilities within their jurisdiction, and for their reputation as precedent-setting pollution markets. The average emissions effects of these programs have also been extensively studied (Fowlie, Holland and Mansur, 2012; Deschenes, Greenstone and Shapiro, 2017), allowing us to build on established research designs to examine changes in allocative efficiency. Within each program, we focus on manufacturing facilities, in part because our framework may not apply to vertically-integrated electric utilities. For both programs, we build a new linking algorithm to merge facility-by-year NO_x emissions data from state and/or federal environmental agencies with restricted-use revenue data from the U.S. Census of Manufacturer (CM) and the Annual Survey of Manufacturing (ASM).

We find that RECLAIM and the NO_x Budget Programs lowered manufacturing NO_x emissions by an average of 18% after their introductions. Using our theory-based quasi-experimental estimator, we find that RECLAIM improved allocative efficiency by 10 percentage points on average in the six years after its cap began to bind. An event study specification shows that this effect grew by 2 percentage points annually. We find allocative improvements across different 2-digit Standard Industrial Classification (SIC) manufacturing industries. Further heterogeneity analyses reveal facilities with pre-existing distortions in capital and labor, and single-plant firms experienced smaller allocative efficiency gains, though these effects are imprecisely estimated. By contrast, we do not detect allocative efficiency changes under the NBP. We speculate two possible explanations. First, unlike RECLAIM which replaced more prescriptive (or command-and-control) regulations, the NBP was overlaid onto existing prescriptive regulations which may have continued to bind after the market was introduced. Second, the NBP was a summer-only pollution market which limits facilities to adopting pollution abatement options that can only be made seasonally. Across policies, we show that our results are relatively unaffected when considering alternative fixed effects, concerns about SUTVA violation, and various subsamples.

We contribute to a rich literature quantifying the total abatement cost of market-based environmental policies. In theory, a polluter's marginal abatement cost is the difference in optimized profit between no abatement and the specified abatement level. In practice, much of the empirical literature has relied on the cost minimizing dual of this problem whereby a particular cost function is assumed and then estimated in a cross-section of polluters.[2] As with any cost function estimation, these studies must argue that all relevant inputs and their prices are observed and vary exogenously. For the estimated cost function to be valid for counterfactual policies, this approach must also assume that polluters do not alter output in the counterfactual, restricting a potentially important abatement option. Additionally, prior approaches often assume that a market-based policy necessarily leads to allocative efficiency gains, leaving researchers with determining by just how much.[3] Our approach starts with the initial profit maximization problem, using its first order condition to inform an observable proxy for marginal product of emissions in a manner similar to Anderson and Sallee (2011). Our quasi-experimental estimator also allows for the possibility that a market-based policy could lead to more or less misallocation, consistent with second-best theory.

In doing so, this paper contributes to a growing quasi-experimental literature documenting the consequences

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[2]Seminal applications of this approach include ex-ante studies that forecast the allocative efficiency gains of hypothetical market-based policies (Gollop and Roberts, 1983, 1985; Carlson et al., 2000) and ex-post studies that quantify efficiency gains of realized policies (Keohane, 2006; Chan et al., 2018).

[3]In ex-ante studies, a cost minimizing algorithm is often assumed to characterize the counterfactual market-based policy. In some ex-post studies, the counterfactual uniform pollution standard is modeled as an extra constraint on the cost minimization problem, which necessarily increases total costs relative to the market-based policy.

of market-based environmental policies. Prior studies have focused on how such policies affect aggregate costs (Petrick and Wagner, 2014; Calel and Dechezleprêtre, 2016; Meng, 2017; Calel, 2020), aggregate benefits and their distribution (Fowlie, Holland and Mansur, 2012; Murray and Rivers, 2015; Deschenes, Greenstone and Shapiro, 2017; Lawley and Thivierge, 2018; Hernandez-Cortes and Meng, 2022; Colmer et al., 2022), or both aggregate costs and benefits (Ayres, Meng and Plantinga, 2021). Greenstone et al. (2022) extends this tradition by combining experimental evidence on emissions effects following the introduction of an Indian emissions market with structural estimation of the allocative efficiency gains. We focus on developing a quasi-experimental estimator for the change in allocative efficiency, bringing a causal inference perspective to testing arguably the central theoretical appeal of market-based environmental policies.

Finally, we contribute to a recent misallocation literature in macroeconomics and development economics. Input misallocation within an economy has been shown to be a strong determinant of aggregate productivity differences across economies (i.e., the indirect approach) (Restuccia and Rogerson, 2008; Hsieh and Klenow, 2009; Restuccia and Rogerson, 2013). More recently, researchers have turned to examining the causes of misallocation (i.e., the direct approach) (Restuccia and Rogerson, 2017). In that spirit, this paper is similar to Bau and Matray (2022), who use a quasi-experiment research design to estimate misallocation effects following capital market liberalization in India. Recent misallocation papers have also addressed concerns about measurement error (Bils, Klenow and Ruane, 2021), and misspecification (Haltiwanger, Kulick and Syverson, 2018). In contrast to Bau and Matray (2022), our approach does not require input prices making it more suitable for studying market introductions. Furthermore, the introduction of a market may be qualitatively different than changes to an existing one: when a market is introduced, agents must not only respond to price signals but must also learn to interact with a new institution.

Our approach has several limitations. First, we are unable to determine whether a market-based environmental policy achieved allocative efficiency, only that it led to more or less relative misallocation. Second, in contrast to studies that estimate a cost function, we do not analyze the specific abatement technologies adopted following a market-based policy, which may shed light on the abatement decisions that alter misallocation costs (Linn, 2008; Fowlie, 2010; Chan et al., 2018). Finally, we rely on parametric assumptions in our theory to facilitate a mapping between our quasi-experimental estimator and the change in allocative efficiency. While these assumptions are employed elsewhere in the misallocation literature, they are also untestable. Our alternative semi-parametric approach trades off these assumptions with other limitations.

The rest of the paper has the following structure. Section 2 provides background on market-based policies in the U.S. Section 3 presents our conceptual framework, linking theory with our empirical research design. Section 4 discusses our data. Section 5 presents our results. Section 6 concludes the paper. Appendix A, B, C, and D offer additional theoretical proofs, data, figures, and tables.

2 Background

2.1 Environmental markets and allocative efficiency

Environmental markets grew out of two strands of economic thinking over fifty years ago. The first was an institutionalist view, led by Coase (1960), that excessive pollution arose due to a lack of property rights to either pollute or to its damages. The second was Arrow (1969)'s notion from general-equilibrium theory that externalities (and thus pollution) can be regarded as a case of missing markets. Both views suggested a correction through some form of introduced market. Building on these foundations, environmental economists recognized that environ-

mental markets can in theory achieve a particular environmental target at minimal cost by allocating emissions across heterogeneous polluters efficiently. This cost-minimization property was articulated in early proposals for markets for water quality (Kneese, 1964) and air pollution (Crocker, 1966; Dales, 1968) and formally demonstrated soon after (Baumol and Oates, 1971; Montgomery, 1972).[4] Today, cost-effectiveness serves as the central appeal behind the modern environmental market, sometimes called "cap-and-trade". In such programs, a regulator establishes a limit (or cap) on total emissions by issuing a fixed supply of emission permits. Regulated facilities are then either given, or must purchase through auction or trade with other facilities, permits to cover their emissions. Cost-effectiveness has motivated the adoption of environmental markets in nearly every environmental domains: today, pricing policies cover 30% of global fisheries (Costello et al., 2016), account for over $36 billion in global ecosystem service payments (Salzman et al., 2018), govern 20% of global greenhouse gas (GHG) emissions (World Bank, 2021), and underlie many major air pollution policies.

This promise of cost-effectiveness has also been subjected to criticism, both theoretically and empirically. Indeed, a second-best theoretical literature emerged shortly after the cost-effectiveness was established in a first-best setting. This literature considered both existing distortions such as market power in output markets (Malueg, 1990), complementary policies (Bohi and Burtraw, 1992; Fowlie, 2010), and income taxation (Goulder et al., 1999; Fullerton and Metcalf, 2001), and distortions that come with the environmental market itself in the form of market power in the permit market (Hahn, 1984), transaction costs (Stavins, 1995), and non-compliance (Malik, 1990). These distortions can not only lower allocative efficiency gains when an environmental market is introduced relative to a first-best setting, but in some cases can even result in allocative efficiency losses. From this literature emerged a more modest view on cost-effectiveness, namely that in real-world settings where various imperfections can affect both market-based and non-market-based environmental policies, whether an environmental market improves allocative efficiency is essentially an empirical question (Stavins, 1995), a point that echoes Demsetz (1969) and indeed was raised back in Coase (1960).

The empirical critique of cost-effectiveness is of a more epistemic nature. Many early pioneers of environmental markets had worked on the theory of optimal environmental policy, which at the time was hitting practical limitations: setting optimal policy requires regulators to know, among other things, the marginal abatement cost curves of every polluter, objects that are unobserved. The impracticality of this informational requirement pivoted attention away from optimal policy towards the design of instruments that can achieve environmental and economic objectives with minimum regulatory information. An environmental market satisfies this criteria: in (first-best) theory, an economy-wide environmental objective can be met at minimum cost without the regulator needing to know every polluter's marginal abatement cost curve. But within this lies an inherent tension with empirical validation: if environmental markets are appealing because it does not require a regulator to know marginal abatement cost curves, is it reasonable to assume that researchers can estimate such curves when attempting to establish the allocative efficiency of environmental markets? We return to this point in Section (3.1) when discussing prevailing approaches to estimating allocative efficiency changes.

2.2 U.S. air pollution markets

Perhaps the domain where environmental markets have been most influential is in U.S. air pollution policy. Beginning with 1976, an offset market was introduced under the U.S. Clean Air Act (CAA) allowing new facilities entering into a county failing CAA air quality standards (i.e., in "nonattainment") to purchase pollution credits from existing facilities. Other experiments with market-based interventions followed.[5] These experiments

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[4]For excellent reviews of this intellectual history, see Tietenberg (2010*a*), Tietenberg (2010*b*), Berta (2017), and Banzhaf (2020).
[5]See Carlin (1992) for other early air pollution markets.

eventually led to the implementation of national and regional air pollution cap-and-trade programs.

Figure 1: Major air pollution cap-and-trade market programs in the U.S.
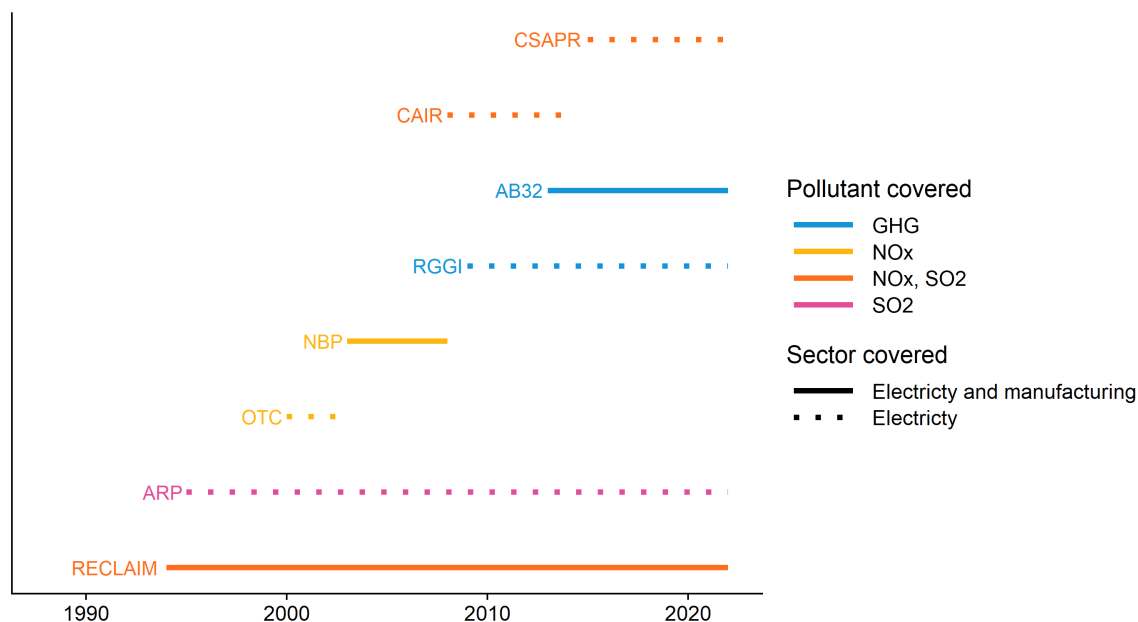
Notes: Figure 1 show the timeline of major global or local air pollution cap-and-trade markets in the U.S. from 1990 to 2020. The length of the line represents the start to end dates for each markets. The different SO_2 and NO_x markets under CAIR and CSARP are bundled together for visual ease. Colors represent the pollutant covered, and the line type indicate whether the market covered electricity facilities, or electricity and manufacturing facilities. The acronyms stand for: Cross-State Air Pollution Rule (CSAPR), Clean Air Interstate Rule (CAIR), Assembly Bill 32 (AB32), Regional Greenhouse Gas Initiative (RGGI), NO_x Budget Program (NBP), Ozone Transport Commission (OTC), Acid Rain Program (ARP), and Regional Clean Air Incentives Market (RECLAIM).

Figure 1 summarizes all such programs over the last three decades. For each market, we show its time duration, the pollutants regulated, and whether the policy covered manufacturing and/or electricity facilities. We employ two criteria in selecting the markets we study, both necessitated by our framework in Section 3. First, because we assume profit-maximizing facilities, we cannot study electricity generators that were part of vertically-integrated utilities. This rules out the SO_2 Acid Rain Program (ARP), which covers only electricity generators and was introduced when the electricity sector was composed largely of vertically-integrated utilities.[6] This requirement also complicates the study of electricity generators in later pollution markets when deregulation of electric utilities may have coincided with the introduction of pollution markets (Cicala, 2022), such as with the Regional Greenhouse Gas Initiative (RGGI). To avoid these complications, we focus on manufacturing facilities that participate in pollution markets. Second, because our framework is static, we omit cap-and-trade programs that allow dynamic banking and borrowing of permits such as California's AB32 greenhouse gas program. These restrictions leave us with two eligible air pollution markets, both for nitrogen oxides (NO_x): southern California's Regional Clean Air Incentives Market (RECLAIM) and the eastern U.S. NO_x Budget Program (NBP).

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[6]Additionally, our framework uses facility-level revenue data. For electricity generators that are part of a vertically-integrated utilities, it is not obvious what is an appropriate measure of revenue as the utility runs its own internal pricing system.

2.3 RECLAIM

The REgional CLean Air Incentives Market (RECLAIM) is a mandatory NO_x emission cap-and-trade program in southern California that was introduced in 1994 by the South Coast Air Quality Management District (SCAQMD). It was introduced to help the region reduce ground-level ozone or smog, and help the region achieve its Clear Air Act ambient standards for ozone.[7] Because NO_x is a precursor to ground-level ozone formation, reduction in NO_x emissions can help reduce ozone concentrations. The program's initial goal was to reduce NO_x emissions across the SCAQMD region from covered facilities by 70% between 1994 and 2003 (Burtraw and Szambelan, 2010). The market is still operating as of 2022.

Facilities emitting more than four tons of NO_x emissions per year are covered by RECLAIM. The market covers about 400 plants located in Los Angeles, Orange, Riverside and San Bernandino counties. These plants are mainly in the manufacturing, electricity generation, and the oil and gas extraction and distribution industries. Within the manufacturing sector, RECLAIM covers a wide range of industries, from food manufacturing, cement manufacturing, petroleum refining, to primary or secondary metal manufacturing. About 80% of observations are in 30 different 3-digit SIC sectors. Yearly permits are freely allocated according to a pre-determined formula based on historical emissions of facilities between 1989 and 1992. A common rate across facilities dictated the decrease in yearly allocations. Banking of permits is prohibited in the market. Ununsed permits expire at the end of a compliance period (Burtraw and Szambelan, 2010).

The introduction of RECLAIM replaced a pre-existing NO_x command-and-control (CAC) policy. Specifically, RECLAIM replaced over 40 prescriptive rules imposed by the SCAQMD. Under the previous CAC regulations, NO_x emissions from specific polluting equipment, such as industrial boilers, were mandated to adopt specific control technologies. With RECLAIM, facilities no longer needed to have equipment-specific controls other than New Source Review permitting requirements under the U.S. Clean Air Act. RECLAIM instead requires facilities to account for emissions from their sources, including specific sources not covered by technology requirements from the previous regulations (U.S. Environmental Protection Agency, 2002). The inclusion of all sources of emissions may expand the abatement options of plants.

Importantly for our empirical setting, while the market was introduced in 1994, the aggregate NO_x emission cap did not start binding until 2000, as covered emissions were far below aggregate permit allocations in the early periods of the program (Fowlie, Holland and Mansur, 2012). Furthermore, the lack of banking prohibited facilities from using their unused permits for future periods. Thus, we follow previous RECLAIM studies and consider the treatment period starting when the cap begins to bind in 2000 (Fowlie, Holland and Mansur, 2012; Grainger and Ruangmas, 2018; Mansur and Sheriff, 2021). Previous papers studying RECLAIM have explored its effects on the distribution of emissions (Fowlie, Holland and Mansur, 2012; Grainger and Ruangmas, 2018; Mansur and Sheriff, 2021), and the effect of initial permit allocation rules on final facility emissions (Fowlie and Perloff, 2013)

2.4 NO_x Budget Program

The NO_x Budget Program (NBP) was a NO_x emission cap-and-trade market operated by the U.S. EPA that ran from 2003 to 2008. The NBP covered NO_x emissions of over 700 large emitting facilities across 20 eastern states.[8]

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[7]Although RECLAIM also covers facilities SO_2 emissions, the main focus of the market was to combat ozone through the reduction of NO_x emissions. The SO_2 part of the market was relatively quite small (Fowlie and Perloff, 2013). Following other studies on RECLAIM, we focus on the NO_x emissions part of the program (Fowlie, Holland and Mansur, 2012; Fowlie and Perloff, 2013; Grainger and Ruangmas, 2018; Mansur and Sheriff, 2021).

[8]The NBP participating states include: Alabama, Connecticut, Delaware, Illinois, Indiana, Kentucky, Maryland, Massachusetts, Michigan, Missouri, New Jersey, New York, North Carolina, Ohio, Pennsylvania, Rhode Island, South Carolina, Tennessee, Virginia, and West Vir-

The market was implemented to help states comply with ozone standards under the 1990 Clean Air Act Amendments. The U.S. EPA assigned each state a summertime NO_x emission budget for large point sources, and encouraged states to participate in the NBP market to provide compliance flexibly to their regulated sources (Burtraw and Szambelan, 2010). The U.S. EPA allowed states to determine how their allowance budget would be allocated across facilities. About 90% of NBP-regulated facilities were large power plants and about 100 facilities were manufacturing plants. For the manufacturing plants covered, more than 90% of the facilities are included in only four 4-digit North American Industry Classification System (NAICS) industries, namely pulp and paper manufacturing, chemical manufacturing, petroleum refineries, and primary metal manufacturing.

Since the NBP was designed to reduce summer ozone, the market operated only between the months of May and September. As opposed to RECLAIM, the NBP did not cover emissions at the facility level, and instead regulated specific pollution sources within facilities, namely boilers. The NBP featured heavy restrictions on the banking of allowances. Once the allowance bank exceeded 10% of the yearly cap, banked allowances, when withdrawn, only counted towards half a ton of emissions. Figure A1 features the close trending of the aggregate emissions and cap under the NBP. In 2009, the NBP was replaced by the ozone air markets under the Clean Air Interstate Rule (CAIR).

The NBP was part of a larger effort by the U.S. EPA and state agencies to reduce NO_x emissions from large point sources. Facilities covered under the NBP were required through earlier regulation to install Reasonably Available Control Technologies (RACT). Such mandates were not removed after the beginning of the trading program. Indeed, the U.S. EPA required that states participating in the NBP to include "requirements that all major stationary sources located in nonattainment areas must install reasonably available control technology" (U.S. Environmental Protection Agency, 2007). Furthermore, each state implemented a variety of measures to continue incentivizing the adoption of specific emission control technologies (Burtraw and Szambelan, 2010).

Since 90% of the regulated boilers are power plants, most prior studies have focused on the NBP's impact on the electricity sector. Fowlie, Knittel and Wolfram (2012) use engineering estimates to build a marginal cost curve for power plants under the NBP. They compare total abatement cost of achieving NO_x emission reductions for power plants in the NBP to abatement costs for vehicle standards. Using difference-in-differences, and structural estimation approaches, studies have found evidence of small capital modifications and technology adoption in anticipation and after the introduction of the NBP (Linn, 2008; Fowlie, 2010; Popp, 2010). Other papers have looked at the health effects of the NBP, and the impact of differences in state permit allocation rules (Deschenes, Greenstone and Shapiro, 2017; Lange and Maniloff, 2021).

Fewer papers have looked at the impacts of the NBP on manufacturing facilities. Shapiro and Walker (2018) combine a theoretical model with a triple-differences research design to uncover the implied pollution tax faced by regulated manufacturing facilities. They find that in the years following the introduction of the NBP, manufacturing facilities saw a doubling of their pollution tax level. Curtis (2018) uses a triple-differences framework to study the county-level manufacturing employment impacts of the NBP, finding that counties with regulated manufacturing plants experienced decreases in manufacturing employment.

3   Conceptual framework

This section details our framework, linking theory and empirics, to estimate the change in allocative inefficiency following the introduction of a market-based policy. Section 3.1 begins with a stylized example to illustrate why this is empirically challenging. Section 3.2 presents a model of environmental policy and a definition of how

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allocative inefficiency in emissions changes across two arbitrary policies, which serves as our estimand. Section 3.3 introduce additional assumptions that enables this theoretical estimand to be recovered empirically, leading to our quasi-experimental estimator in Section 3.4. Section 3.5 explores various extensions to our framework that weakens some assumptions (while introducing others).

3.1 Stylized example

We begin with a 2-facility example to illustrate the empirical challenges of estimating the change in allocative efficiency following a market-based policy. The graphs in Figure 2 show emissions on the horizontal axis and its (shadow) price on the vertical axis. Facility 1 has a steeper marginal product of emissions curve than facility 2.[9] For a given allowable total emissions, E, there is a particular allocation of emissions that minimizes total cost, indicated by the sum of the shaded areas across the facilities. As panel (a) indicates, that efficient allocation occurs when the marginal product of emissions is equalized across facilities (i.e., the equimarginal principle is satisfied) at the economy-wide emissions price $\lambda(E)$ such that the more costly Facility 1 engages in less abatement while the less costly Facility 2 has more abatement.

Figure 2: Environmental policy and allocative (in)efficiency

(a) Efficient allocation   **(b)** Inefficient allocation
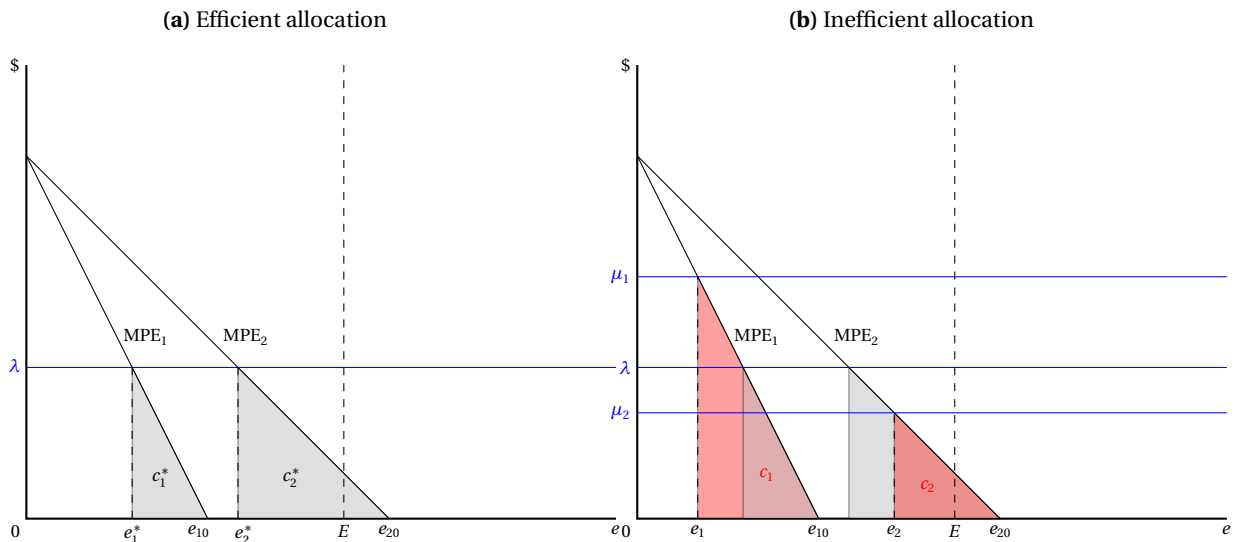
Notes: Panels illustrate allocative efficiency in emissions for a 2-facility economy. Horizontal axes indicate emissions. Vertical axes indicate emissions price. In panel (a), total emissions E is allocated at minimum total cost with facilities equating their marginal product of emissions (MPE) to the economy-wide emissions price $\lambda(E)$. In panel (b), facilities face separate emissions prices, resulting in misallocation and increased total cost.

Next, consider when total emissions E is not efficiently allocated across facilities, as shown in panel (b) of Figure 2. When this happens, the marginal product of emissions is no longer equalized with each facility facing its own emissions price, μ_i. There is too much abatement in one facility and not enough abatement in the other, leading total cost to increase. This can arise under any environmental policy, regardless of whether the policy is market- or non-market- based. That is one can imagine a version of panel (b) under a baseline policy and

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[9]The horizontal axes in Figure 2 indicates emissions rather than abatement in order to illustrate emissions levels when the emissions price is zero. When presented in terms of emissions abatement relative to the no-policy scenario, the marginal product of emissions curve becomes the marginal abatement cost curve.

another version under a market-based policy with a different set of distortions.

We are interested in quantifying the change in total cost between two policies (i.e., compare the total areas under the curves across policies). Answering this question would be straightforward if one observes every facility's marginal product of emissions curves. Because they are not observed, the typical approach is to obtain these curves via cost function estimation. Such an approach has several limitations. First, as with any cost function estimation, the researcher must argue that she observes all inputs and their prices and that each varies exogenously. Second, for the estimated cost functions to be valid for counterfactual policies, duality theory requires that facility-specific output be unchanged in the counterfactual, restricting a potentially important abatement option (Malueg, 1990). Third, many cost function studies implicitly assumes that a market-based policy would necessarily lead to greater allocative efficiency than the policy it replaces. For example, in ex-ante studies, a cost minimizing algorithm is often assumed to characterize the counterfactual market-based policy (Gollop and Roberts, 1983, 1985; Carlson et al., 2000). While in some ex-post studies, the counterfactual uniform pollution standard is modeled as an extra constraint on the cost minimization problem, which necessarily increases total costs relative to the market-based policy (Chan et al., 2018).[10] Finally, there is an epistemic tension with trying to estimate facility-specific marginal product of emissions curves: if a key appeal of environmental markets over command-and-control policies is that it is unreasonable to expect a regulator to know such curves, how does one reasonably expect researchers to be able to estimate them.

Panel (b) suggests an alternative approach. Rather than explicitly estimate each facility-level marginal product of emissions curve, perhaps something can be learned about allocative efficiency by looking at the dispersion in input prices, μ_i. This idea is leveraged by the misallocation literature, where the dispersion in appropriately-weighted input prices informs the aggregate productivity consequences of input misallocation (Restuccia and Rogerson, 2008; Hsieh and Klenow, 2009; Restuccia and Rogerson, 2013). We draw on this insight, but with one critical caveat: by definition, input prices are missing (or are "shadow") before a market-based policy and consistently missing for facilities in a control group. That is, one needs to adapt methods from the misallocation literature, designed for quantifying misallocation in existing markets, to the study of new markets. Furthermore, in contrast to the stylized example in Figure 2, an empirically-useful framework must allow for, among other things, an arbitrary number of heterogeneous facilities, policies that may have different total emissions, and policy changes that may coincide with changing macroeconomic conditions. We now turn to such a framework.

3.2 Theory

Let $i = 1, ..., N$ index facilities using emissions e_i and another input z_i in the production function $q_i(e_i, z_i)$. Let $p(q_i)$ denote output price, which may be affected by output, and w be price of input z. Policy state s is defined by two features: the vector of facility-level emissions $\mathbf{e}_s = \{e_{1s}, ..., e_{Ns}\}$ and total emissions across facilities, $E_s = \sum_i e_{is}$. Importantly, $\mathbf{e}_s$ need not be the efficient allocation of emissions across facilities for total emissions E_s.

Total abatement cost under allocative efficiency We are interested in quantifying the magnitude of allocative efficiency loss due to $\mathbf{e}_s$ under total emissions E_s. To do so, we must first establish total abatement cost when total emissions E_s is efficiently allocated across facilities. Following (Montgomery, 1972), this is the solution to

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[10]Another approach to recovering the marginal product of emissions is to estimate a distance output function following Färe et al. (1989, 1993). Because distance output, as a ratio of observed outputs to potential output under efficiency, is unobserved, its value relies heavily on functional form assumptions on how inputs and outputs map onto distance output, and exogeneity of these variables. Coggins and Swinton (1996), Swinton (2002), and Swinton (2004) conduct ex-post analyses of a market-based policy using this approach.

the regulator's problem of allocating E_s emissions across facilities to maximize total profit. That problem is

$$\Pi_i^* = \max_{e_i, z_i} \quad \sum_i p(q_i) q_i(e_i, z_i) - w z_i$$

$$s.t. \quad \sum_i e_i = E_s$$

$$= \max_{e_i, z_i} \quad \sum_i p(q_i) q_i(e_i, z_i) - w z_i - \lambda_s \left(\sum_i e_i - E_s \right) \tag{1}$$

where $\lambda_s(E_s)$ is the economy-wide (shadow) emissions price on the total emissions constraint when facility-level emissions are allocated efficiently, henceforth denoted as λ_s. Under efficient allocation, the total abatement cost of going from E_o, total emissions in the absence of policy, to E_s is

$$\Delta \Pi_s^* = (E_o - E_s) \frac{d\Pi_s}{dE_s}\bigg|_{E_s} + \mathcal{O}^2$$

$$\approx (E_o - E_s) \lambda_s \tag{2}$$

where the first line applies a Taylor expansion around E_s. The second line observes that via the envelope theorem the derivative of optimized aggregate profit with respect to emissions is the aggregate shadow price, and uses the first order term of the Taylor series as an approximation.

Total abatement cost under a particular policy We next consider total abatement cost under policy s. Optimal profit for facility i is

$$\pi_{is}(e_{is}) = \max_{e_i, z_i} \quad p(q_i) q_i(e_i, z_i) - w z_i$$

$$s.t. \quad e_i = e_{is}$$

$$= \max_{e_i, z_i} \quad p(q_i) q_i(e_i, z_i) - w z_i - \mu_{is}(e_i - e_{is}) \tag{3}$$

where $\mu_{is}(e_{is})$ is the Lagrange multiplier on the emissions constraint, henceforth denote as μ_{is}. Observe that eq. 3 encompasses a wide range of regulatory environments. For example, under a command-and-control regulation, the regulator may set e_{is} directly. Under a market-based policy, facilities may face an emissions price leading to e_{is}, which may or may not be allocatively efficient.

Regardless of policy, μ_{is} is the facility-level shadow price of emissions at e_{is}. We follow the misallocation literature and represent the facility-level shadow price as the product of the aggregate shadow price under efficient allocation and a facility-level distortion term, or wedge, $\mu_{is} = \lambda_s \phi_{is}$. Intuitively, the policy induces an efficient allocation of emissions when there are no distortions, $\phi_{is} = 1 \; \forall i$. Allocative inefficiency arises when distortions generate dispersion in facility-level shadow prices.

Let $\mathbf{e}^o = \{e_1^o, ..., e_N^o\}$ denote the vector of facility-level emissions in the absence of policy with $E_o = \sum_i e_{io}$. Under policy s, the total abatement cost of going from the no-policy vector of emissions, $\mathbf{e}^o$, to the policy s

vector of emissions, $\mathbf{e}^s$, is

$$
\begin{aligned}
\Delta\Pi_s &= \sum_i \Delta\pi_{is}(e_{is}) \\
&= \sum_i (e_{io} - e_{is}) \frac{d\pi_{is}}{de_{is}}\Big|_{e_{is}} + \mathcal{O}^2 \\
&\approx \sum_i (e_{io} - e_{is})\lambda_s \phi_{is}
\end{aligned}
\tag{4}
$$

where the second line applies a Taylor expansion around e_{is}. The third line observes that by the envelope theorem the derivative of optimized profit with respect to emissions is the facility-level shadow price, and uses the first order term of the Taylor series as an approximation.

Allocative inefficiency under a particular policy   What is the cost of emissions misallocation under state s? For a given total emissions E_s, one can examine the ratio of total abatement cost under the policy to total abatement cost under allocative efficiency. Combining eqs. 2 and 4, this measure is

$$
\frac{\sum_i (e_{io} - e_{is})\lambda_s \phi_{is}}{(E_o - E_s)\lambda_s} = \sum_i a_{is} \phi_{is}
$$

where $a_{is} = \frac{e_{io} - e_{is}}{E_o - E_s}$ are weights capturing facility-level shares of total abatement with $\sum_i a_{is} = 1$. When $\sum_i a_{is} \phi_{is} = 1$, policy s induces allocative efficiency; when $\sum_i a_{is} \phi_{is} > 1$, policy s lead to misallocation, with efficiency losses increasing with the ratio. Observe that this misallocation measure is independent of total emissions E_s, a normalization that is important when comparing policies that differ in total emissions.

Change in allocative inefficiency across policies   Consider two policy states $s \in \{b, m\}$, where b indicates the baseline policy and m indicates the market-based policy. The two policies can differ both by their vector of facility-level emissions, $\mathbf{e}_s$, and by total emissions E_s. We are interested in the ratio of misallocation costs for policy m relative to policy b, or

$$
\theta = \frac{\sum_i a_{im} \phi_{im}}{\sum_i a_{ib} \phi_{ib}}
\tag{5}
$$

When $\theta = 1$, misallocation costs are unchanged. When $\theta < 1$, state m lowers misallocation costs (or improved allocative efficiency) compared with state b. Likewise, when $\theta > 1$, state m increased misallocation costs (or worsened allocative efficiency) compared with state b. $\theta - 1$ denotes the percent change in misallocation costs across policies. We rewrite eq. (5) as

$$
\begin{aligned}
\theta &= \frac{N\left[(\frac{1}{N}\sum\phi_{im})(\frac{1}{N}\sum a_{im}) + \frac{1}{N}\sum_i(\phi_{im} - \frac{1}{N}\sum\phi_{im})(a_{im} - \frac{1}{N}\sum a_{im}\right]}{N\left[(\frac{1}{N}\sum\phi_{im})(\frac{1}{N}\sum a_{ib}) + \frac{1}{N}\sum_i(\phi_{ib} - \frac{1}{N}\sum\phi_{ib})(a_{ib} - \frac{1}{N}\sum a_{ib}\right]} \\
&= \frac{\left[\mu_m + N\rho_m\right]}{\left[\mu_b + N\rho_b\right]} \\
&= \frac{\mu_m}{\mu_b}\left[1 + N\left(\frac{\rho_m}{\mu_m} - \frac{\rho_b}{\mu_b}\right)\right] + \mathcal{O}^2 \\
&\approx \tilde{\theta}\left[1 + N\left(\frac{\rho_m}{\mu_m} - \frac{\rho_b}{\mu_b}\right)\right]
\end{aligned}
\tag{6}
$$

where the first line expands the expression. The second line defines population means, $\mu_s = (1/N)\sum_i \phi_{is}$, and covariances and $\rho_s = (1/N)\sum_i(\phi_{is} - \frac{1}{N}\sum\phi_{is})(a_{is} - \frac{1}{N}\sum a_{is})$, and uses $\sum_i a_{is} = 1$. The third line applies a Taylor expansion around μ_m and μ_b. The fourth line defines $\widetilde{\theta} = \frac{\mu_m}{\mu_b}$ and uses the first order term of the Taylor series as an approximation.

3.3   From theory to empirics

The change in allocative efficiency, θ in equation (6), is constructed from facility-level abatement share a_{is} and distortion ϕ_{is} for each policy state. These elements are not directly observed in data.[11] To facilitate our empirical investigation, we turn to additional assumptions.

Proposition 1. *If $\phi_{is} \sim \mathscr{LN}(0, \sigma_s^2)$ and $a_{is} = f(\phi_{is})$ where $f()$ is either an increasing linear or increasing power function of ϕ_{is}, then $\widetilde{\theta}$ is a lower bound on the allocative efficiency gain or loss, or $\theta - \widetilde{\theta} < 0$ if $\theta < 1$ and $\theta - \widetilde{\theta} > 0$ if $\theta > 1$.*

Appendix A.1 provides the proof. Here, we discuss some intuition behind the assumptions underlying Proposition 1. Note that assuming $\phi_{is} \sim \mathscr{LN}(0, \sigma_s^2)$ offers a link between $\widetilde{\theta}$ and the change in the variance of distortion across policies, an observation made elsewhere in the misallocation literature (Hsieh and Klenow, 2009). That is, $\widetilde{\theta} > 1$ when the variance of distortions increase following the policy change while $\widetilde{\theta} < 1$ when the variance of distortions decrease.[12] Second, it is natural assume that abatement increases with distortions: as the shadow price of pollution increases with higher distortions a facility cuts more pollution. In Section 3.5, we consider an extension that relax this functional form assumption.

Proposition 1 point to $\widetilde{\theta}$ as our object of interest. But estimating $\widetilde{\theta}$ still requires facility-level distortions across policy states, which are also not directly observed. To over come this, we turn to the first order condition for the firm problem in eq. (3), equating the marginal cost of emissions with its marginal revenue

$$\lambda_s \phi_{is} = (1 + \xi_i)\kappa_i \frac{p_i q_{is}}{e_{is}} \tag{7}$$

where $\kappa_i = \frac{\partial q_i}{\partial e_i}\frac{e_i}{q_i} > 1$ is output elasticity and $\xi_i = \frac{\partial p_i}{\partial q_i}\frac{q_i}{p_i}$ is the inverse price elasticity[13], both of which may be heterogeneous across facilities. On the demand side, a growing literature documents heterogenous markups, and thus demand elasticities, across firms even within narrow sectoral definitions (Nevo, 2001; Hottman, Redding and Weinstein, 2016). Rewriting eq. 7 as average revenue per emissions, $AR_{is} = \frac{p_i q_{is}}{e_{is}}$, yields

$$\ln AR_{is} = \ln(1/(1 + \xi_i)) - \ln \kappa_i + \ln \lambda_s + \ln \phi_{is} \tag{8}$$

3.4   Empirical specifications

Eq. (8) suggests a possible regression specification. However, any structural expression, when applied to data, must contend with the possibility of other changes coinciding with a policy introduction that are left out of the

---

[11]Observe that abatement share a_{is} requires facility-level emissions and total emissions in the absence of policy, e_{io} and E_o. The possibility that an existing pollution policy exists prior to the introduction of a market-based policy suggests that e_{io} and E_o may not be observed.

[12]To see this, let $\phi_{is} \sim \mathscr{LN}(0, \sigma_s^2)$, then

$$\widetilde{\theta} = e^{\frac{\sigma_m^2}{2} - \frac{\sigma_b^2}{2}}$$

Since $\frac{1}{2}(var(\ln \phi_{im}) - var(\ln \phi_{ib})) = \frac{\sigma_m^2}{2} - \frac{\sigma_b^2}{2}$, $\widetilde{\theta} > 1$ when $var(\ln \phi_{im}) - var(\ln \phi_{ib}) > 0$ and $\widetilde{\theta} < 1$ when $var(\ln \phi_{im}) - var(\ln \phi_{ib}) < 0$.

[13]Profit maximization requires a firm to operate in the elastic portion of its demand curve such that $\frac{1}{\epsilon_i} > -1$.

structural expression. For example, the introduction of a market-based policy may coincide with secular macroeconomic changes that jointly alters the aggregate shadow price of emissions[14] It is also possible that macroeconomic conditions jointly alter the dispersion of distortions for treated and control facilities and that facilities differ by baseline distortions, regardless of policy, such that $\phi_{it} \sim \mathcal{LN}(0, \sigma_i^2 + \sigma_{st}^2)$. For estimation, these possibilities necessitate the use for a control group of facilities that are subject to the same macroeconomic changes but not the change in policy and a two-way fixed effects panel data estimator. Define $\mathscr{B}$ as the set of control facilities and $\mathscr{M}$ as the set of treated facilities and let t indicate year relative to the last year before adoption of the market-based policy. We implement a two-step estimation procedure. The first step recovers changes in policy-wide mean parameters. The second step estimates changes in policy-wide dispersion in distortions.

Our first step estimation involves an event study regression analog to structural equation (8)

$$\ln AR_{it} = \underbrace{\eta_i}_{\substack{\ln\left(\frac{1}{1+\xi_i}\right)-\ln\kappa_i \\ +\begin{cases}\ln\lambda_{b0} & \text{if } i \in \mathscr{B} \\ \ln\lambda_{m0} & \text{if } i \in \mathscr{M}\end{cases}}} + \underbrace{\gamma_t}_{\ln\lambda_{bt}-\ln\lambda_{b0}} + \sum_{\substack{-\underline{\tau}\leq\tau\leq\overline{\tau} \\ \tau\neq 0}} \underbrace{\alpha^\tau D_i \times \mathbf{1}(\tau=t)}_{\substack{(\ln\lambda_{mt}-\ln\lambda_{bt}) \\ -(\ln\lambda_{m0}-\ln\lambda_{b0})}} + \underbrace{\nu_{it}}_{\ln\phi_{it}+\epsilon_{it}} \tag{9}$$

where D_i is a dummy variable that equals one for treated facility $i \in \mathscr{M}$ eventually subject to the market-based policy. The facility-level fixed effect, η_i, captures captures facility-specific demand and supply side parameters, ξ_i and κ_i, respectively, as well as the aggregate shadow price for each respective group in the omitted year, or the last year before the policy change, $t = 0$. The year fixed effect, γ_t, captures any annual changes in the aggregate shadow price for the control group relative to the omitted year. The coefficients of interest are α^τ, capturing the difference in the aggregate shadow price between treated and control facilities in each year τ relative to that difference in the omitted year. When $\tau < 0$, α^τ tests for the presence of pre-trends in the relative aggregate shadow price. When $\tau > 0$, α^τ examines whether the aggregate shadow price changed due to the market-based policy. Eq. (9) is our most flexible specification, designed to detect the presence of pre-trends and time-varying policy change effects. To obtain and average treatment effect across the post change period, we also estimate a difference-in-differences version of eq. (9)

$$\ln AR_{it} = \eta_i + \gamma_t + \alpha D_i \times \mathbf{1}(\tau > 0) + \nu_{it} \tag{9'}$$

The residual ν_{it} in eq. (9) captures distortions, $\ln\phi_{it}$. It also captures any remaining error, ϵ_{it}, due to mismeasurement and misspecification (Haltiwanger, Kulick and Syverson, 2018; Bils, Klenow and Ruane, 2021). To recover our dispersion parameters and ultimately $\widetilde{\theta}$, we square the predicted residuals $\widehat{\nu}_{it}$ after estimating eq. (9) and estimate a similar second-stage regression

$$\widehat{\nu}_{it}^2 = \underbrace{\psi_i}_{\substack{\sigma_i^2 \\ +\begin{cases}\sigma_{b0}^2 & \text{if } i \in \mathscr{B} \\ \sigma_{m0}^2 & \text{if } i \in \mathscr{M}\end{cases}}} + \underbrace{\upsilon_t}_{\sigma_{bt}^2-\sigma_{b0}^2} + \sum_{\substack{-\underline{\tau}\leq\tau\leq\overline{\tau} \\ \tau\neq 0}} \underbrace{\beta^\tau D_i \times \mathbf{1}(\tau=t)}_{\substack{(\sigma_{mt}^2-\sigma_{bt}^2) \\ -(\sigma_{m0}^2-\sigma_{b0}^2)}} + \epsilon_{it} \tag{10}$$

where the facility-level fixed effect, ψ_i, captures any heteroscedasticity across facilities and any baseline difference in the dispersion of distortions between treated and treated facilities in the omitted year. The year fixed effect, υ_t captures annual changes in the dispersion of distortions for the control group relative to the omitted

---

[14]For example, an increase in aggregate demand would drive up total emissions in the no-policy scenario, E_o, increasing $E_o - E_s$ and hence λ_s.

year. For both ν_{it} and ξ_{it}, we cluster standard errors at a broader jurisdictional level (e.g., zip code or county depending on application) to account for arbitrary forms of spatial correlation and serial correlation within facilities of that jurisdiction.

Our main reduced-form coefficients of interest are β^τ. When $\tau < 0$, β^τ tests for pre-trends in the relative dispersion of distortions between treated and control facilities, relative to the omitted year. When $\tau > 0$, β^τ estimates the difference in the dispersion of distortions between treated and control facilities due to the market-based policy, relative to the omitted year. The flexible function form of eq. (10) allows for the testing of pre-trends and time-varying policy change effects. As with our first stage estimation, we also consider a difference-in-differences variant of eq. (10)

$$\widehat{\nu}_{it}^2 = \psi_i + \upsilon_t + \beta D_i \times \mathbf{1}(\tau > 0) + \epsilon_{it} \tag{10'}$$

For identification, we assume that any pre-treatment difference in the squared residuals, $\widehat{\nu^2}$, between treated and control facilities would have continued if not for the introduction of the market-based environmental policy.

3.5 Extension

Semi-parametric recovery of θ Proposition 1 employs parametric assumptions on both the distribution of distortions ϕ_{is} and on the functional mapping between ϕ_{is} and abatement share a_{is} to show that $\widetilde{\theta}$ can be a lower bound on θ. These assumptions help circumvent the issue that facility-level emissions and total emissions in the absence of policy, e_{io} and E_o may not be observed.

Semi-parametric recovery of θ is still possible without the functional form assumption on $f()$, as detailed in Appendix A.2, provided we now employ the assumption that distortions ϕ_{is} are uncorrelated with emissions in the absence of policy, e_{io}. We have

$$\overline{\theta} \approx \widetilde{\theta}\left[1 - \frac{N(\delta - 1)}{E_b - E_m}\left(\frac{\varrho_m}{\delta \mu_m} - \frac{\varrho_b}{\mu_b}\right)\right] \tag{11}$$

where $\varrho_s = \frac{1}{N}\sum(\phi_{is} - \frac{1}{N}\sum\phi_{is})(e_{io} - \frac{1}{N}\sum e_{io})$ is the population covariance between distortions and emissions. Observe that each element in eq. (11) is either directly observed from data (e.g., $e_{im}, e_{ib}, E_m - E_b$) or can be estimated (e.g., ϕ_{im}, ϕ_{ib}). We use the difference-in-differences estimate of the policy-induced effect on emissions to obtain $\ln \delta$.

4   Data

Our empirical framework requires observing both pollution emissions and revenue at the facility level for both regulated and unregulated facilities, and for periods before and after a market introduction. To achieve this, we link facility-level U.S. Census restricted-use data from the Annual Survey of Manufacturing (ASM) and the Census of Manufacturer (CM) with data on air pollution emissions and air pollution markets from state and federal environmental agencies. We refer to the merged panel of U.S. Census data between years of the ASM and CM as the ASMCM.[15]

A contribution of this paper is the creation of a U.S. facility-level panel of economic and air pollution variables. Previous papers have matched panel of US plant-level pollution to a single year of ASM data (Shapiro and

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[15]We use interchangeably the terms plant or facility to refer to a manufacturing plant or manufacturing facility.

Walker, 2018) or used private plant-level data that proxy plant revenue.[16] We instead match facility level pollution data to restricted U.S. Census manufacturing economic variables over time. The following subsections detail the pollution data, the U.S. Census ASM and CM data, and how we link combine them.

CARB data

Yearly plant NO_x emissions and facility characteristics in California for 1990, 1993, and annually from 1995 to 2005 come from the California Air Resources Board (CARB). Emissions for the years 1991, 1992, and 1994 are not available. CARB collects criteria air pollution data under various state and federal mandates, and is aggregated from its thirty-five local air quality districts (CARB, 2017). Under California mandates, facilities emitting above 10 tons of criteria pollution per year are required to report emissions annually.[17] This threshold is much higher at the federal level: the U.S. EPA's national emissions inventory covers only facilities with at least 100 tons per year of a criteria air pollutant. Since RECLAIM covers plants that emit as low as four tons of NO_x emissions per year, we follow previous studies in the literature by restricting our control plants those in the CARB data as it covers smaller emitting facilities than data from the U.S. EPA.

The RECLAIM treatment status of plants is provided by the SCAQMD. We use the merged CARB and SCAQMD data from Fowlie, Holland and Mansur (2012). Facility-level characteristics in the CARB data that we use for the matching to the ASMCM (detailed below) include facility name, address, SIC code, zip code, and county code.

U.S. EPA data

We use two separate U.S. EPA datasets to obtain pollution emissions and NBP treatment status. Data on yearly NO_x emissions for plants covered under the NBP are available through the U.S. EPA's Air Market Program Data (AMPD). There are two reasons why we cannot solely rely on the AMPD for NO_x emissions: (1) less than 30 out of the nearly 100 treated manufacturing plants report pre-2003 emissions, and (2) there are no untreated manufacturing plants. To be included in the AMPD, a facility needs to be covered by a U.S. EPA cap-and-trade program. For example, the control plants in Deschenes, Greenstone and Shapiro (2017) are mostly power plants covered by the Acid Rain Program's (ARP) SO_2 cap-and-trade market, but not by the NBP. Since ARP does not cover manufacturing plants, we cannot use this approach.

Instead, we supplement the AMPD with data from the U.S EPA National Emissions Inventory (NEI). The NEI reports emissions of criteria pollutants for large point sources every three years. For treated plants without pre-treatment emissions, we use their U.S. EPA's Facility Registration Services (FRS) ID to obtain emissions in the NEI. For these facilities, we use their NEI NO_x emissions for both the pre- and post-periods. NO_x emissions for control manufacturing plants are entirely from the NEI. Since the NEI only reports emissions every three year, we constrain the NBP sample to the years 1999, 2002, and 2005.[18] Facility-level characteristics in the combined AMPD and NEI used in our merge with the ASMCM (detailed below) include facility name, address, NAICS code, zip code, and county code.

---

[16]For example, Cherniwchan (2017); Cui, Lapan and Moschini (2016) use the privately-constructed National Establishment Time-Series (NETS) data which includes common unique identifiers to match facility-level outcomes such as sales and employment to facility-level pollution from the US EPA data. One issue with the NETS is that its facility revenue is imputed using employment at the facility level multiplied by industry sales per employee (Walls & Associates, 2020). This implies that variation in the NETS imputed revenue is essentially driven by variation in employment.

[17]Criteria pollutants include particulate matter (PM), nitrogen oxides (NO_x), sulfur oxides (SO_x), volatile organic compounds (VOCs), and ammonia (NH_3).

[18]Since the 2005 NEI operated under a reduced budget, about 1/3 of facilities reported the same 2002 emissions for 2005 (Cui, Lapan and Moschini, 2016). We drop these plants from both our treated and control groups.

U.S. Census Bureau data

We use the total value of shipment (TVS) variable included in the ASMCM as our revenue measure. The ASM is conducted every non-census year, and the CM is conducted every 5 years. The ASM includes approximately 50,000 plants out of the CM population of about 300,000 manufacturing plants. For ASM years, the 10,000 largest plants by revenue are selected with certainty, and the remaining 40,000 are a representative sample selected randomly. We use the U.S. Census Bureau's Longitudinal Business Database (LBD) to create a panel of plants linking ASM and CM data from 1990 to 2005 (Chow et al., 2021). We use the LBD plant identifier as our main unique facility identifier for plant fixed effects in the analysis as opposed to the facility identifier from the pollution data. This is because the LBD identifier has been continuously cleaned and scrutinized by U.S. Census Bureau researchers over the last decades (Chow et al., 2021). We also merge NAICS and SIC industry classifiers, zip code, and FIPS county code from the LBD to the ASMCM panel. Using the LBD identifier, we further merge facility names and address from the U.S. Census Bureau Standard Statistical Establishment List (SSEL) (DeSalvo, Limehouse and Klimek, 2016).

Record linkage algorithm

Since there are no common unique facility identifiers between our state and federal pollution data and the confidential ASMCM panel, we use non-unique identifiers such as facility name and address in both datasets to create a crosswalk between the unique facility identifier in each dataset. To implement this record linkage problem (Cuffe and Goldschlag, 2018), we develop a matching algorithm using the following standard procedures: (1) preprocessing data, (2) sorting the data into blocks, (3) identifying potential matches, and (4) resolving the best matches (Massey and O'Hara, 2014). We match facilities use different combinations of non-unique identifiers, namely facility name, facility address, industry classifiers, zip code, and county codes. Appendix B provides further details on our matching procedure.

Since our outcome variable is a natural log transformation of a ratio, we drop plants who report either zero emissions or zero revenue. For RECLAIM, we match about 70% of the treated manufacturing plants to the ASMCM data, and about 40% of the control plants. One reason for the differential match rate is that the CARB data features smaller emitters that not included in the Annual Survey of Manufacturers. Indeed, the ASM probabilistically samples the smaller manufacturers. On the other hand, the average RECLAIM plant is a larger emitter than the average control plant in California, therefore making it more likely to be in the U.S. Census data. Similarly, we match nearly all of the 95 NBP manufacturing facilities to the ASMCM data, since the NBP covered very large emitters. However, we drop about 30 plants since they do not report pre-emissions in the AMPD data, and reported the same emissions for 2002 and 2005 in the NEI. Appendix D presents further summary statistics for the unmatched and matched samples for RECLAIM and NBP.

5   Results

This section applies our empirical framework to the RECLAIM and NBP NO_x cap-and-trade markets. Using event-study and difference-in-differences models, we first establish that the introduction of the markets reduced NO_x emissions, consistent with results found elsewhere in the literature. We then report the first stage of our empirical procedure showing the effect of the pollution markets on average revenue of emissions by estimating equations (9) and (9'). In our second stage, we take first-stage residuals and estimate the market-induced change in the variance of residuals using equations (10) and (10'). We then explore potential mechanisms driving differences in allocative efficiency gains and present several robustness checks. Section 5.1 presents results

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for RECLAIM program while Section 5.2 presents results for the NBP.

5.1 RECLAIM

We begin by estimating the effect of RECLAIM on NO_x emissions, the targeted pollutant. We do this both to quantify the emissions effect of RECLAIM for our sample of manufacturing facilities and to compare these effects with previous emissions effects reported in the literature using a similar research design. Figure 3 presents RECLAIM NO_x emissions estimates using the event-study model in equation 9 with facility-year log NO_x emissions as the outcome. To verify the quality of our record linking procedure, we display annual coefficients for both the full sample of manufacturing facilities available in CARB's emissions dataset (in gold) and the matched sample following the CARB and ASMCM data merge (in blue).

Each point represents the difference in NO_x emissions changes between treated and control plants compared to the year 1999, the last year before the overall cap became binding. For the CARB sample, NO_x emissions of treated plants significantly decreased compared to control plants, relative to their differences before the cap was binding. This effect in the post-period is the same for the matched plants. For both samples, the emissions effects increase in magnitude from 2000 to 2005 as the aggregate emissions cap continues to fall. There are also no pre-trends in NO_x emission changes between the treated and control plants in the CARB data prior to the cap binding. In the case of the matched sample, there is a pre-trend in emissions for the treated plants compared to the control plants. However, these effects were increasing before the cap was binding, hence trending in the opposite direction than the post-market effects. Pre-treatment emissions effects are also not statistically distinguishable across the two samples in all years but 1990.

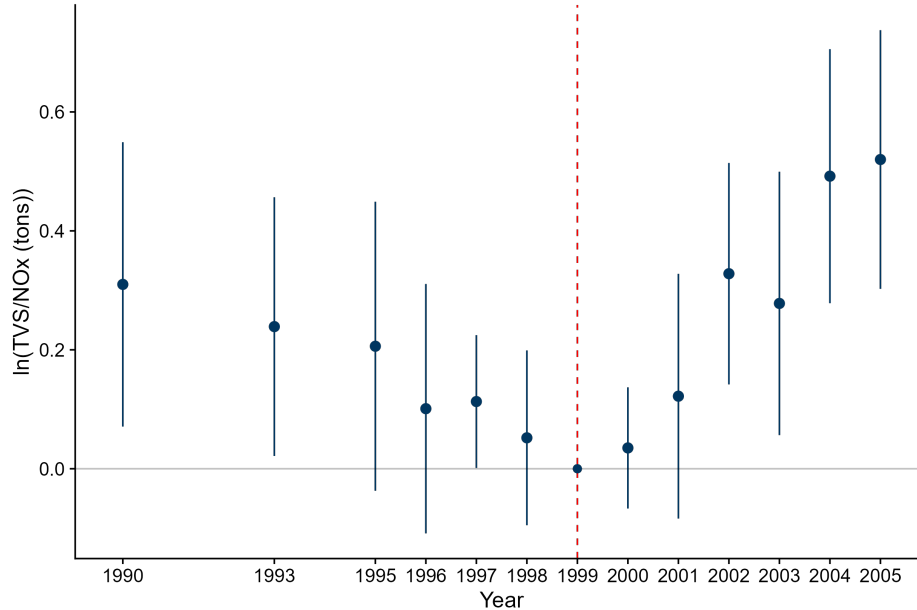**Figure 3:** Event-study model of the effect of RECLAIM on NO_x emissions by sample
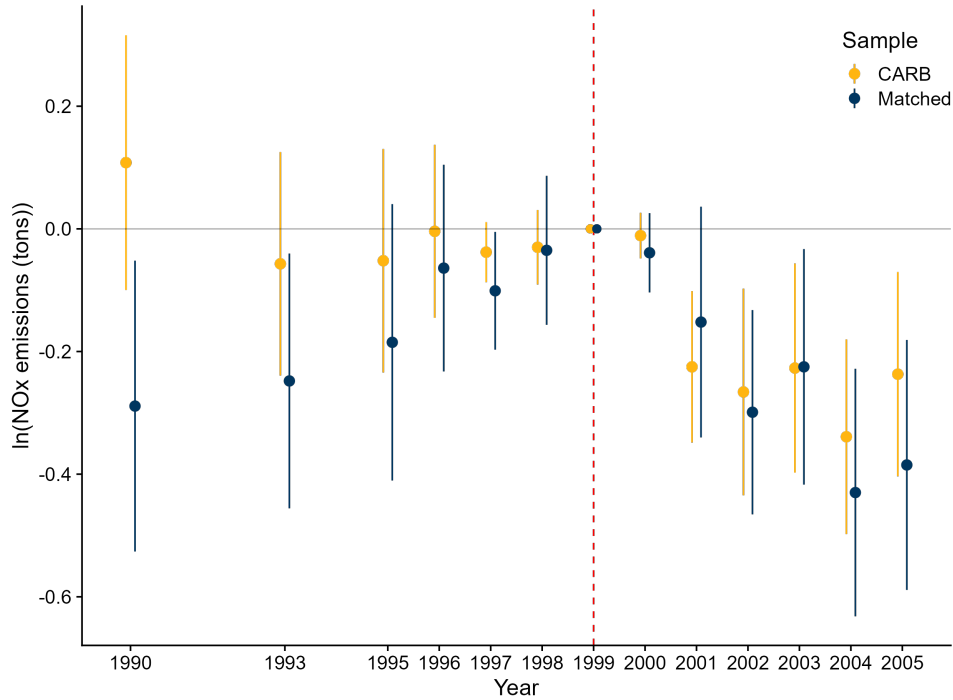
Notes: Point estimates and 95% confidence intervals of the yearly effect of RECLAIM on log NO_x emissions relative to 1999 using eq. (9). Estimates for the full sample of manufacturing facilities in CARB shown in gold. Estimates for the CARB-ASMCM matched sample shown in blue. Standard errors are clustered at the zip code level.

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Columns (1) and (2) of Table 1 reports the average treatment effect of RECLAIM on NO_x emissions using the difference-in-differences specification in eq. (9') for CARB and matched CARB-ASMCM samples, respectively. The full sample suggests manufacturing plants covered by RECLAIM reduced their emissions by 18% compared to polluting facilities in the rest of California. While the average emissions effect for the CARB-ASMCM matched sample is a smaller -12%, it is not statistically different from the full CARB sample. This NO_x emission reduction effect is broadly consistent with emissions estimates from previous studies using similar research design, though these studies have not separately examined only manufacturing facilities (Fowlie, Holland and Mansur, 2012; Grainger and Ruangmas, 2018; Mansur and Sheriff, 2021). The reduction in total NO_x emissions after the introduction of RECLAIM highlights the importance of having a framework that allows total emissions target to change across policies, as considered in Section 3.

We now turn to our main empirical results. We start with our first stage estimates of the effect of RECLAIM on the economy-wide efficient shadow price of NO_x emissions. If emissions decreased for RECLAIM plants relative to the control plants, we should expect this to translate to an increased NO_x shadow price relative to the NO_x shadow price for plants in the rest of California. Figure 4 shows the estimates α^τ, or the difference in the shadow price for treated and control plants for each year, relative to their difference in 1999, from equation 9. Consistent with the emission effect of the policy shown in Figure 3, the shadow price of NO_x emission increased for treated plants after the cap binds. As the aggregate cap further falls during 2000 to 2005, the aggregate NO_x shadow price trends upwards. In terms of differential pre-trends, Figure 4 shows the shadow price of NO_x emissions trending downward prior to the cap binding for treated plants. RECLAIM reverses this trend.

Figure 4: Event-study model of the effect of RECLAIM on NO_x shadow price

Notes: Point estimates and 95% confidence intervals of the yearly effect of RECLAIM on log revenue per emissions relative to 1999, or $\hat{\alpha}^\tau$ using eq. (9). Standard errors are clustered at the zip code level.

Column (3) of Table 1 presents the average treatment effect of RECLAIM on log average revenue using equation (9'). The estimated coefficient represents the effect of RECLAIM on the aggregate NO_x shadow price. Consistent with the emissions effects detected in columns (1) and (2), RECLAIM increased the aggregate shadow price for NO_x emissions by 14%.

Before turning to our theory-informed estimate of the change in allocative efficiency under RECLAIM, we turn first to a more intuitive test of the policy-driven change in dispersion of distortions. Recall that allocative inefficiencies increase as the dispersion of distortions increase. As such, if a pollution market were to lower allocative inefficiencies, one should also see a drop in the annual cross-sectional variance of estimated residuals, $\widehat{v}_{it}$, from eq. (9), for treated facilities relative to control facilities after the market introduction. While the change in cross-sectional variance is directly linked to our theory, its intuitive connection with the dispersion in distortions can help build confidence in our eventual theory-based measure from Section 3.2.

This is shown in Figure 5. If RECLAIM led to allocative efficiency gains in NO_x emissions for treated plants, we should expect the variance of the plant emission distortions to reduce after RECLAIM relative to that of the control plants. Prior to the binding of the cap, the difference in variances for treated and control facilities generally follow a similar pattern. A divergence occurs after RECLAIM binds with the variance of treated facilities being consistently lower than that for control facilities. Figure 5 hints at allocative efficiency gains from RECLAIM. Lower variance in residuals for treated facilities relative to control facilities suggest lower misallocation.

Figure 5: Difference in variance of distortions between RECLAIM treated and control plants
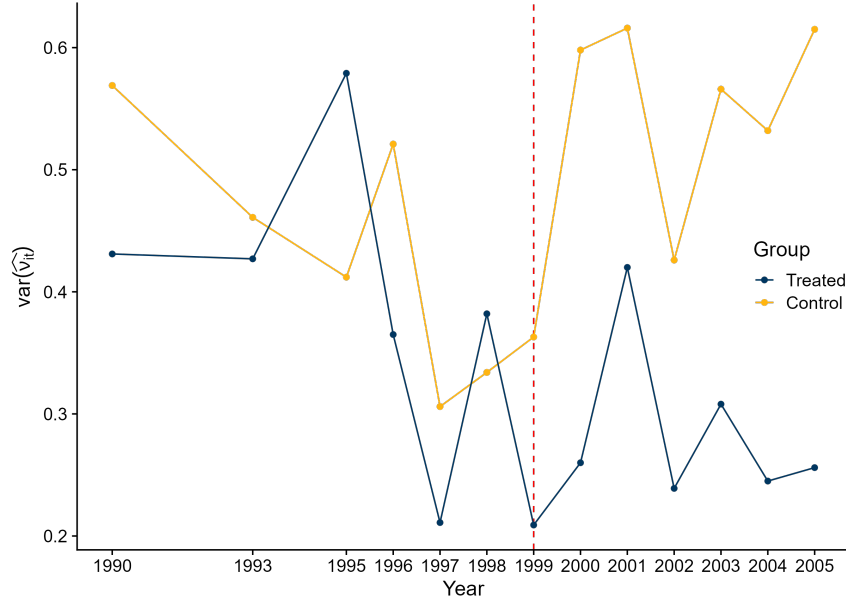
Notes: Blue and gold lines show the annual variance of the predicted residual, $\widehat{v}_{it}$, from equation 9 for treated and control plants, respectively.

The top panel of Figure 6 shows our main allocative efficiency effect for RECLAIM, plotting estimates $\widehat{\beta^\tau}$ from eq. (10). In the post period, $\widehat{\beta^\tau}$ is consistently negative, implying allocative efficiency gains. They are also downward trending, indicating allocative efficiency gains that improve over time. Pre-trend coefficients suggests there were no strong differential effect on the dispersion of NO_x emission distortions between control and treated plants in California before the RECLAIM cap was binding. If anything, the dispersion of distortions for treated plants were trending in an opposite direction. The bottom panel of Figure 6 presents the corresponding allocative efficiency measure, $\widehat{\widetilde{\theta}} = e^{\frac{\widehat{\beta^\tau}}{2}}$. RECLAIM has lowered allocative inefficiency by about 10 percentage points. This effect increases in magnitude over time at an annual rate of roughly 2 percentage points.

Figure 6: Annual effects of RECLAIM on allocative efficiency

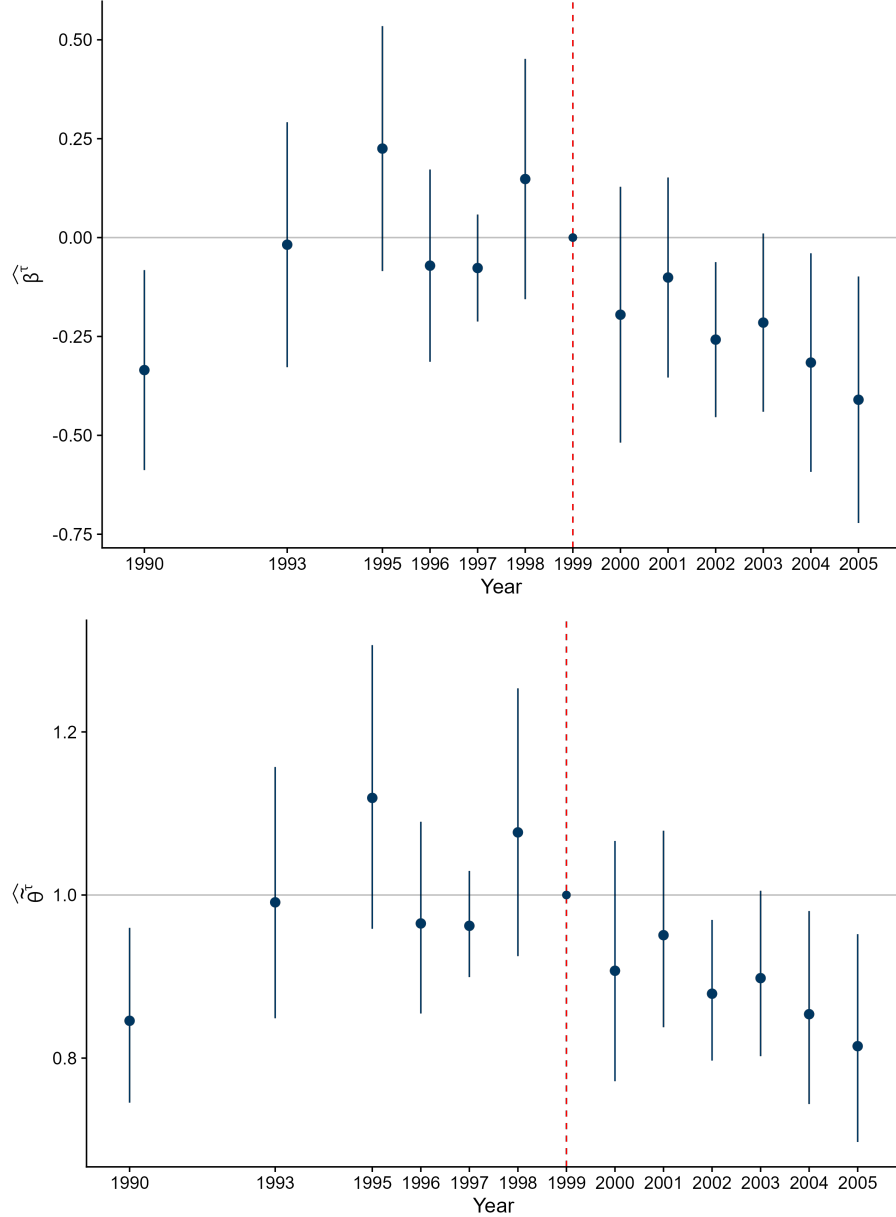**Notes**: Top panel shows point estimates and 95% confidence intervals of the yearly effect of RECLAIM on squared residuals relative to 1999, or $\widehat{\beta}^\tau$ using eq. (10). Bottom panel shows for $\widehat{\widetilde{\theta}}^\tau = e^{\frac{\widehat{\beta}^\tau}{2}}$. Standard errors are clustered at the zip code level.

Column (4) of Table 1 presents the average treatment effect of RECLAIM on the dispersion of distortions or $\widehat{\beta}$ from equation (10'). Column (4) also shows the implied lower bound on the change in allocative efficiency, $\widehat{\widetilde{\theta}} = e^{\frac{\widehat{\beta}}{2}}$. A $\widehat{\widetilde{\theta}}$ of 0.898 indicates that the RECLAIM market led to allocative efficiency gains in NO_x emissions of 10 percentage points (i.e., $\widehat{\widetilde{\theta}}$-1). Under the parametric assumptions maintained in Proposition 1, $\widetilde{\theta}$ is a lower bound on, θ, the theory-based changed in allocative efficiency. Table 1 also presents a semi-parametric estimate of θ using equation 11 showing $\widehat{\overline{\theta}} = 0.5$. Finding that $\widehat{\overline{\theta}} < \widehat{\widetilde{\theta}}$ also reaffirms that $\widetilde{\theta}$ serve as a lower bound on the true allocative efficiency change.[19] Taken together, these estimates provides causal evidence that RECLAIM led to

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[19]When recovering $\widehat{\overline{\theta}}$, we also consider varying the the ratio of total abatement under RECLAIM compared to the control group, or δ, as

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improvements in allocative efficiency in NO_x emissions.

Table 1: Average treatment effect of RECLAIM

	$\ln NO_x$ emissions (1)	$\ln NO_x$ emissions (2)	$\ln AR_{it}$ (3)	$\widehat{\nu}_{it}^2$ (4)
RECLAIM X Post	−0.182***	−0.116*	0.142*	−0.215**
	(0.049)	(0.062)	(0.073)	(0.092)
$\widehat{\widetilde{\theta}}$				0.898
				[0.821, 0.983]
$\widehat{\overline{\theta}}$				0.500
Observations	27,000	11,500	11,500	11,500
Sample	CARB	Matched	Matched	Matched

Notes: Estimates of the average treatment effect of RECLAIM using a difference-in-difference model. All models include year- and facility-level fixed effects. Columns (1) and (2) examine log NO_x emissions as outcome using eq. (9). Column (3) models log average revenue per emissions as outcome using eq. (9'). Column (4) models the squared predicted residuals from eq. 9 as outcome using eq. (10). Column (1) uses the full CARB sample of manufacturing plants and the CARB facility identifier for facility fixed effects. Columns (2)-(4) uses the matched CARB-ASMCM sample and the LBD facility identifier for facility fixed effects. The lower bound on allocative efficiency change is $\widehat{\overline{\theta}} = e^{\frac{\hat{\beta}}{2}}$. The semi-parametric measure of allocative efficiency change is $\widehat{\widetilde{\theta}}$ from eq. 11. Robust standard errors clustered at the zip code in parentheses.

Robustness checks   We conduct several robustness checks of the average RECLAIM effects on NO_x emissions, average revenue of emissions (i.e., eq. 9'), and the dispersion of residuals (i.e., eq. 10'). Because of potential disclosure risks from increased information releases, the U.S. Census Bureau encourages the release of qualitative result for robustness checks, namely just the sign and statistical significance of the coefficients (U.S. Census Bureau, 2022). We henceforth follow this guidance.

   We find that our estimates are robust to variations in sample, and different sets of fixed effects. In our setting qualitatively robust means that the sign of the coefficient doesn't change. In terms of inference, estimates might be more or less precise. We examine four robustness checks. First, we find that our results are robust to restricting our samples to a balanced panel. The precision is reduced for all coefficients since our sample size drops from 10,000 to 1,000 observations. Second, we restrict the control group to the same set of manufacturing industries as the treated group. The estimates have the same sign and are statistically precise for all coefficients. Third, our results are robust when we replace year fixed effects with industry (SIC 3-digit) by year fixed effects. Finally, we cluster bootstrap our standard errors over the whole two-step estimation procedure. Results are statistically more precise after bootstrapping.

Heterogeneity and mechanisms   We now turn to heterogeneity analyses, some of which may inform potential underlying mechanisms for allocative efficiency gains under RECLAIM.

   Table 2 estimates $\widehat{\widetilde{\theta}}$ for each 2-digit SIC manufacturing sector by re-estimate eq. (10) in which the treatment variable is interacted with industry indicators. We find $\widehat{\widetilde{\theta}} < 1$ for every sector, suggesting that allocative efficiency

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opposed to using the implied ratio from our estimates. Panel (a) of Figure A2 shows that for a reasonable range of δ, our estimates of $\widehat{\widetilde{\theta}}$ for RECLAIM are always less than $\widehat{\overline{\theta}}$.

gains are shared broadly. These effects, however, are only statistically different from zero at the 5% level for petroleum refineries and primary metal manufacturers, possibly due to reduced statistical power.

Table 2: Allocative efficiency effect of RECLAIM by industry

Industry	$\widehat{\widehat{\theta}}$	95% CI
Petroleum refineries (SIC 29)	0.829	[0.719, 0.956]
Primary metal manufacturing (SIC 33)	0.857	[0.789, 0.930]
Other manufacturing	0.917	[0.813, 1.035]
Cement and glass manufacturing (SIC 32)	0.924	[0.851, 1.003]
Secondary metal manufacturing (SIC 34)	0.938	[0.779, 1.131]
Food manufacturing (SIC 20)	0.965	[0.851, 1.093]

Notes: Point estimates and 95% confidence interval of allocative efficiency effect, $\widehat{\widehat{\theta}}$, by industry. Robust standard errors are clustered at the zip code level.

To explore potential mechanisms, we turn to two dimensions. Table 3 examines heterogeneity in the RE-CLAIM effect on the squared residuals, $\widehat{\beta}$, by whether a plant is owned by a single- or multi-plant firm. One hypothesis is that firms with more than one plant might have more abatement reallocation options than firms that only operate a single plant. This logic is consistent with prior work that has shown increased production or abatement flexibility by multi-plant firms (Gibson, 2019; Cui and Moschini, 2020). Column (1) interacts the treatment variable with a dummy variable equal to one if it is operated by a multi plant firm. The uninteracted coefficient therefore represents the allocative efficiency gains from RECLAIM for single plant firms. The inter-acted coefficient suggests that there are imprecisely estimated small allocative efficiency gains for multi-plant firms relative to single plant firms from the market.

Table 3: Average treatment effect of RECLAIM by type of firm

	$\widehat{\nu}_{it}^2$ (1)	$\widehat{\nu}_{it}^2$ (2)	$\widehat{\nu}_{it}^2$ (3)
RECLAIM X Post	−0.187**	−0.181**	−0.128
	(0.087)	(0.081)	(0.080)
RECLAIM X Post X Multi-plant firm	−0.044		−0.163
	(0.080)		(0.146)
Observations	11,500	9,500	9,500
Sample	Matched	Single region firms	Single region firms

Notes: Estimates of the effect of RECLAIM on the dispersion of distortions. Column (1) further interacts the treatment variable with a dummy equal to one if the firm is a multi plant firm. Column (2) drops multi-plant firms that operate plants both inside and outside RECLAIM. Column (3) interacts the multi-plant dummy with the treatment variable for the sample that drops firm with plant inside and outside RECLAIM. All models include plant and year fixed effects. Robust standard errors clustered at the zip code level in parentheses.

Columns (2) and (3) analyze a separate sample whereby multi-facility firms that operate plants both inside and outside of RECLAIM are dropped. Column (2) replicates our main estimate without these firms that po-

tentially violate the SUTVA assumption through reallocation of production or emissions to control plants. The overall allocative efficiency effect is statistically indistinguishable from our main estimate in column (4) of Table 1. However when dropping these multi region firms, column (3) shows larger effects on allocative efficiency gains between single- and multi-facility firms. In this sample, multi-facility firms are imprecisely estimated to have twice the efficiency gains versus single plant firms. Thus, Table 1 provides suggestive (and imprecise) evidence that firms with more abatement options experience larger allocative efficiency gains.

Next, we consider whether distortions in other inputs, namely labor as measured by facility employment and capital as measured by capital expenditures, affect efficiency gains in emissions. We begin by constructing a measure of labor and capital distortions using data from the pre-period. Specifically, we run the following model:

$$\ln AR^o_{it} = \eta^o_i + \gamma^o_t + \nu^o_{it} \tag{12}$$

where $o \in \{l, k\}$ for labor and capital, respectively. AR^o_{it} is now either average revenue per employee or dollar of capital expenditure. For each input and facility, we average the pre-period predicted residual from estimating eq. 12 and take the absolute value, denoted as $d^o_i \geq 0$. When $d^o_i = 0$, labor or capital distortions are zero on average for facility i before RECLAIM.

We interact the treatment variable from equation 10' separately with d^o_i for each input, and also jointly for both. Columns (1) and (2) of Table 4 separately interact the RECLAIM treatment variable with the capital and labor distortion measure, respectively. Column (3) jointly interacts both distortion measures. For facilities in which there are no distortions in other inputs, the uninteracted coefficient shows a drop in allocative inefficiency for NO_x emissions that are larger in magnitude than our full-sample results in column (4) of Table 1. Facilities with baseline labor and capital distortions experience smaller improvements in allocative efficiency, as indicated by the positive interaction coefficients. While statistically imprecise, the point estimate of the interacted effect is around one half of the average treatment effect. These heterogeneity results suggest that distortions in other inputs can prevent plants from achieving efficiency gains in the allocation of emissions following the introduction of a pollution market.

Table 4: Average treatment effect of RECLAIM by distortions in other inputs

|	$\widehat{\nu}^2_{it}$ (1)	$\widehat{\nu}^2_{it}$ (2)	$\widehat{\nu}^2_{it}$ (3)
RECLAIM X Post	−0.291*	−0.245**	−0.311*
	(0.149)	(0.112)	(0.161)
RECLAIM X Post X Absolute value of capital distortion	0.111		0.105
	(0.130)		(0.129)
RECLAIM X Post X Absolute value of labor distortion		0.142	0.114
		(0.237)	(0.235)
Observations	9,200	9,200	9,200

Notes: Estimates of the effect of RECLAIM on the dispersion of distortions. Column (1) interacts the RECLAIM treatment variable with absolute value in pre-policy capital distortions. Column (2) interacts the treatment variable with absolute value in pre-policy clabor distortions. Column (3) includes both interactions jointly. All models include plant and year fixed effects. Robust standard errors clustered at the zip code level in parentheses.

5.2 NO$_x$ Budget Program

This section presents our NBP results. Because the U.S. EPA's NEI data is available every three years, NBP results only use data from the years 1999, 2002, and 2005.

Figure 7 presents the NBP effects on NO$_x$ emissions using an event-study model. The coefficients capture the difference in NO$_x$ emission changes between treated and control manufacturing plants compared to the difference in 2002 before the introduction of the market in 2003. The 2005 coefficient shows that the program lowered NO$_x$ emissions. While there are pre-policy differences in 1999, they are trending in the opposition direction of the post-policy trend. Column (1) of Table 5 presents the average treatment effect of the NBP on NO$_x$ emissions. The NBP market reduced manufacturing facility emissions by about 18%. As a basis for comparison, using a triple-differences research design applied to a sample of power plants, Deschenes, Greenstone and Shapiro (2017) find that the NBP lowered seasonal NO$_x$ emissions by 44%.
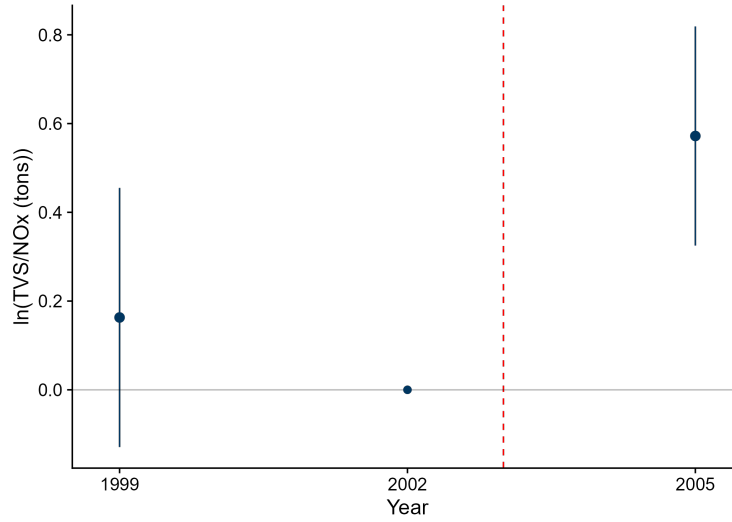
Figure 7: Event-study model of the effect of NBP on NO$_x$ emissions
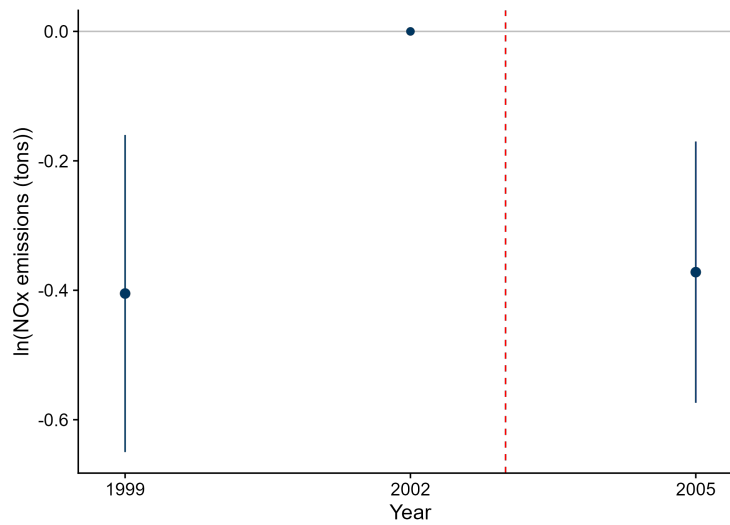
Notes: Point estimates and 95% confidence intervals of the yearly effect of NBP on log NO$_x$ emissions relative to 2002, the year before the NBP was introduced, using eq. (9). Standard errors are clustered at the county level.

Figure 8 shows the effect of the NBP on average revenue per emissions using eq. (9'), which following Section 3.4 can be interpreted as the aggregate shadow-price of emissions under efficient allocation. Estimates indeed show an increase in the shadow price after the introduction of the NBP, consistent with the negative NBP emissions effect. The coefficient for 1999 does not indicate a precise 1999 difference between treated and control facilities. If anything, the 1999 effect trends in the opposite direction than the 2005 effect. Interestingly, these estimates of the NBP aggregate NO$_x$ shadow price in Figure 8 mirrors closely the marginal pollution tax effect found in Shapiro and Walker (2018). Using a structural model, the authors find that the NBP increased the pollution tax of covered manufacturing plants by one log point in 2005, whereas our quasi-experimental estimate shows a 0.6 log point increase. Our 1999 effect also closely mirrors their estimate. Column (2) of 5 presents the average treatment effect of NBP on average revenue of emissions, or α from eq. 9. The estimate suggests an increase of 50% in aggregate NO$_x$ price under efficient allocation for treated plants.

Figure 8: Event-study model of the effect of NBP on NO$_x$ shadow price

As with RECLAIM, we look at the annual cross-sectional variance in estimated residuals, $\widehat{\nu}_{it}$, separately for treated and control facilities, for an intuitive test for whether the program altered allocative efficiency. Unlike with RECLAIM, Figure 9 shows that no change in trend in the differential variance across treated and control facilities. The two time series exhibit similar gaps both before and after the introduction of NPB, suggesting that allocative efficiency changes may have been limited under the NBP.

Figure 9: Difference in variance of plant level distortions between NBP treated and control plants
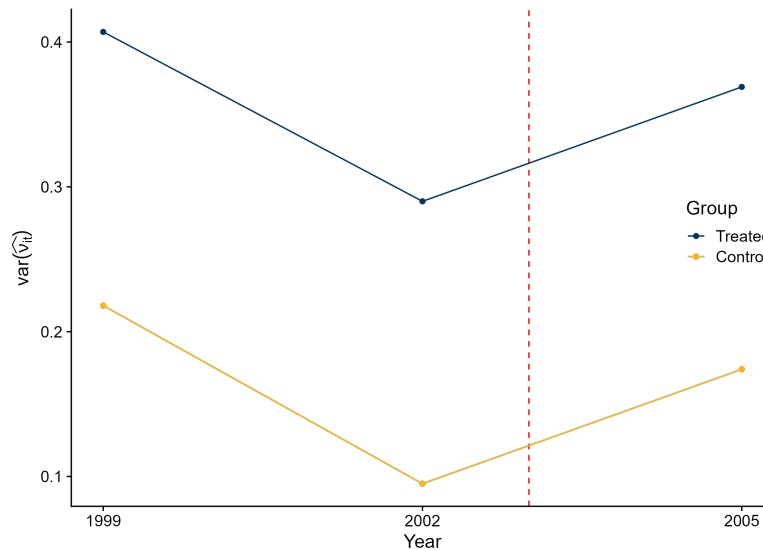
We now turn to our main estimate of the allocative efficiency effect of the NBP. The top panel of Figure 10

plots estimates of β^τ from eq. (9') or the effect of NBP on the squared residual of average revenue per emissions. Coefficients before and after the NBP are not statistically significant. If anything, the positive post-treatment coefficient suggests a slight increase in misallocation from the policy. The bottom panel of Figure 10 presents the corresponding allocative efficiency measure, $\widehat{\widetilde{\theta}} = e^{\frac{\widehat{\beta^\tau}}{2}}$.

Figure 10: Annual effects of NBP on allocative efficiency
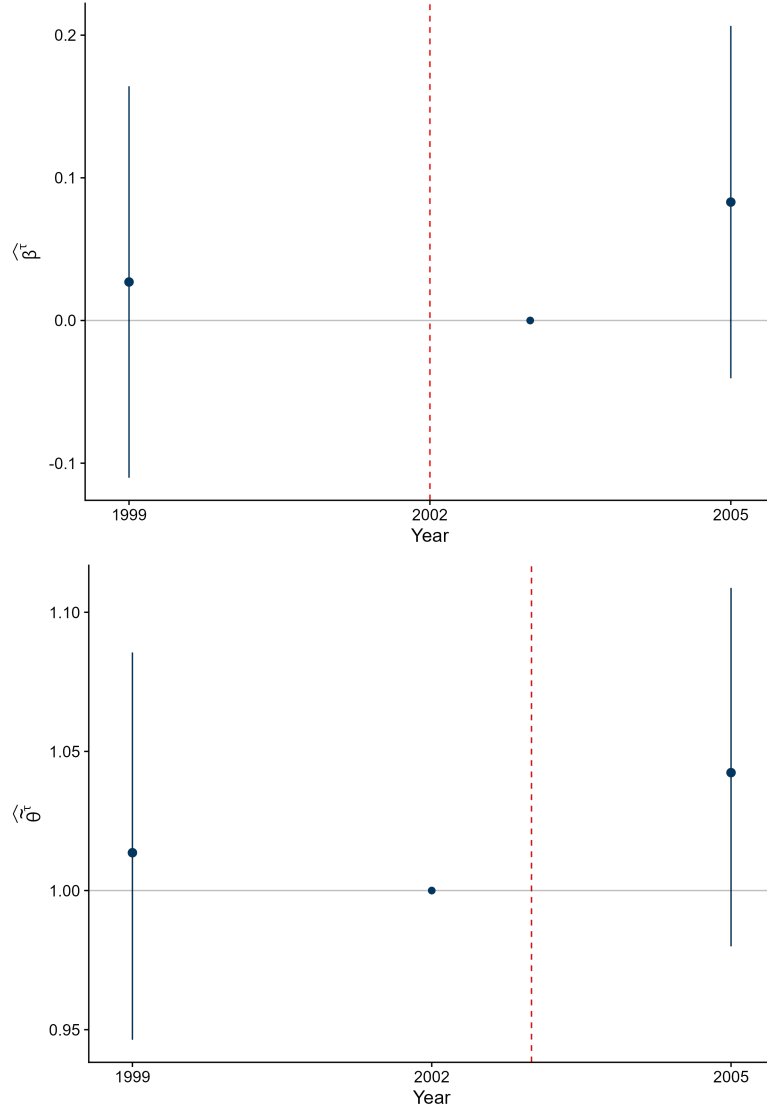
Notes: Top panel shows point estimates and 95% confidence intervals of the yearly effect of NBP on squared residuals relative to 2002, or $\widehat{\beta}^\tau$ using eq. (10). Bottom panel shows $\widehat{\widetilde{\theta}}^\tau = e^{\frac{\widehat{\beta}^\tau}{2}}$. Standard errors are clustered at the county code level.

The last column of Table 5 show the average treatment effect of NBP on squared residuals, or $\widehat{\beta}$ from eq. (10), and our related measure of allocative efficiency changes, $\widehat{\widetilde{\theta}} = e^{\frac{\widehat{\beta}}{2}}$. The table suggest an imprecise increase in allocative inefficiency as a result from the policy. The semi-parametric version of θ, or $\widehat{\overline{\theta}}$ from eq. 11, also suggests a small increase in allocative inefficiency.

Table 5: Average treatment effect of NBP

	$\ln NO_x$ emissions	$\ln AR_{it}$	$\widehat{\nu}_{it}^2$
	(1)	(2)	(3)
NBP X Post	−0.176*	0.494***	0.070
	(0.105)	(0.133)	(0.056)
$\widehat{\widetilde{\theta}}$			1.036
			[0.98, 1.094]
$\widehat{\widehat{\theta}}$			1.083
Observations	16,500	16,500	16,500

Notes: Estimates of the average treatment effect of NBP using a difference-in-difference model. All models include year- and facility-level fixed effects. Columns (1) examines log NO_x emissions as outcome using eq. (9). Column (2) models log average revenue per emissions as outcome using eq. (9'). Column (3) models the squared predicted residuals from eq. 9 as outcome using eq. (10). The lower bound on allocative efficiency change is $\widehat{\widetilde{\theta}} = e^{\frac{\widehat{\beta}}{2}}$. The semi-parametric measure of allocative efficiency change is $\widehat{\widehat{\theta}}$ from eq. 11. Robust standard errors clustered at the county level in parentheses.

Why did the NBP not deliver allocative efficiency gains? Given the smaller number of treated plants in the NBP compared with RECLAIM, we are severely underpowered from conducting the same set of heterogeneity analyses. More critically, the smaller number of treated facilities under NBP mean that we fail disclosure requirements mandated by the U.S. Census Bureau U.S. Census Bureau (2022). We therefore speculate two possible explanations. First, unlike RECLAIM which replaced prescriptive regulations, the NBP was overlaid onto prescriptive regulations that continued after the market's introduction. Insofar as those regulations continued to bind, improvements in allocative efficiency will be limited. Second, the NBP was a summer-only pollution market which limits facilities from adopting pollution abatement options that can only be made seasonally.

Robustness checks We conduct several robustness checks our NBP effects. As with our RECLAIM results, for US Census Bureau disclosure reasons, we discuss only the sign and statistical significance of these results.

We first consider a balanced panel. The sign and significance of the results are robust to that change. About half of the sample is maintained in the balanced panel. Since not all manufacturing facilities are covered in each NBP state, we also consider the subsample composed only of treated and control facilities in states with both sets of facilities. Again, our results are robust to this subset. Third, we restrict the control group to the same set of manufacturing industries as the treated group. Results are of the same sign and significance as in Table 5. Finally, to account for potential confounding changes at the state level and differential shocks at the industry level, we also run our main effects using state-by-year and industry-by-year fixed effects. Our results are again robust to those additions.

6   Conclusion

Market-based interventions hold the promise of improving allocative inefficiencies in settings where prices are otherwise missing. Pollution provides a classic example: the introduction of a market can in theory efficiently allocate emissions across heterogeneous polluters, lowering the total cost of meeting an aggregate pollution tar-

get compared with more prescriptive regulations. However, validating this prediction is fundamentally difficult: the lack of prices before the introduction of a pollution market makes it challenging to determine the change in allocative efficiency due to the market.

In this paper, we develop a framework for empirically testing the change in allocative efficiency across two arbitrary policy regimes when input prices are unobservable. We lean on a producer's first order condition to relate its observed average revenue of emissions to its unobservable marginal product of emissions. We then show how a difference-in-differences research design links a quasi-experimental estimator to the theory-based change in allocative efficiency. In contrast to prior approaches, our framework does not assume that a market-based policy necessarily improves allocative efficiency. The resulting two-sided statistical test is consistent with second-best theories showing it is possible for a pollution market to not only have limited allocative efficiency gains, but in some cases even efficiency losses. In doing so, we add to an emerging literature using quasi-experimental approaches to quantify the aggregate consequences of input misallocation. Here, our key contribution is that our framework can be applied to settings where a new market is being introduced.

We study the introduction of two landmark U.S. air pollution cap-and-trade markets aimed at reducing NO_x emissions: Southern California's Regional Clean Air Incentives Market (RECLAIM), and the eastern U.S. NO_x Budget Program (NBP). This requires developing a linking algorithm to match manufacturing facility emissions data from regional and national environmental agencies with restricted-use revenue data from the U.S. Census of Manufacturers and the Annual Survey of Manufacturing. We find that RECLAIM improved allocative efficiency by 10 percentage points in the six years after its cap starts binding. This effect grows by 2 percentage points annually. Heterogeneity analyses suggest that facilities with less labor and capital distortions, and more abatement and output reshuffling options experienced greater efficiency gains.

By contrast, we do not find evidence of allocative efficiency gains for manufacturing plants covered by the NBP. We speculate two possible explanations. First, unlike RECLAIM which replaced more prescriptive (or command-and-control) regulations, the NBP was overlaid onto existing prescriptive regulations which may have continued to bind after the market was introduced. Second, the NBP was a summer-only pollution market which limits facilities to adopting pollution abatement options that can only be made seasonally. Taken together, these results highlight the conditions whereby market-based environmental policies may deliver promised allocative efficiency gains and when those gains may be limited.

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A Theory appendix

A.1 Proposition 1

This appendix section proves Proposition 1. Recall eq. (6)

$$\theta \approx \widetilde{\theta} \left[1 + N \left(\underbrace{\frac{\rho_m}{\mu_m}}_{Z_m} - \underbrace{\frac{\rho_b}{\mu_b}}_{Z_b} \right) \right]$$

To establish Proposition 1, it is sufficient to demonstrate that $dZ_s/d\mu_s > 0$. When $\widetilde{\theta} = \frac{\mu_m}{\mu_b} < 1$ or $\mu_m - \mu_b < 0$, having $Z_m - Z_b < 0$ implies $\theta < \widetilde{\theta}$ and thus $\widetilde{\theta}$ is a lower bound on the true allocative efficiency gain θ. Conversely, when $\widetilde{\theta} > 1$ or $\mu_m - \mu_b > 0$, having $Z_m - Z_b > 0$ implies $\theta > \widetilde{\theta}$ and so $\widetilde{\theta}$ is a lower bound on the true allocative efficiency loss θ. We establish this for two functional forms $f()$ relating distortion ϕ_{is} and abatement share a_{is} under the assumption that $\phi_{is} \sim \mathcal{LN}(0, \sigma_s^2)$.

Power function   Let $a_{is} = \phi_{is}^p$, where p is the power parameter. We have

$$Z_s = (e^{\frac{\sigma_s^2}{2}})^{((p+1)^2 - 1)} - (e^{\frac{\sigma_s^2}{2}})^{p^2}$$

$$\frac{dZ_s}{dE[\phi_{is}]} = ((p+1)^2 - 1)(e^{\frac{\sigma_s^2}{2}})^{((p+1)^2 - 2)} - p^2 (e^{\frac{\sigma_s^2}{2}})^{(p^2 - 1)}$$

$$= p^2 (e^{\frac{\sigma_s^2}{2}})^{(p^2 - 1)} \left[(e^{\frac{\sigma_s^2}{2}})^{2p} - 1 \right] + 2p(e^{\frac{\sigma_s^2}{2}})^{((p+1)^2 - 2)}$$

Thus, $\frac{dZ}{d\mu_s} > 0$ if $p > 0$, or when $f()$ is an increasing power function of ϕ_{is}.

Linear function   Let $a_{is} = \alpha \phi_{is}$, where α is the linear multiplier parameter. We have

$$Z_s = \alpha(e^{\sigma_s^2} - 1)e^{\frac{\sigma_s^2}{2}}$$

$$\frac{dZ_s}{dE[\phi_{is}]} = \alpha(3e^{\sigma_s^2} - 1)$$

Thus, $\frac{dZ}{d\mu_s} > 0$ if $\alpha > 0$, or when $f()$ is an increasing linear function of ϕ_{is}.

A.2 Semi-parametric recovery of θ

To see how θ can be recovered with data without the parametric assumptions on $f()$ in Proposition 1, we first expand eq. (6)

$$
\begin{aligned}
\theta \approx{}& \widetilde{\theta}\left[1 + N\left(\frac{\rho_m}{\mu_m} - \frac{\rho_b}{\mu_b}\right)\right] \\
={}& \widetilde{\theta}\left[1 + N\left(\frac{(1/N)\sum_i(\phi_{im} - \frac{1}{N}\sum\phi_{im})(a_{im} - \frac{1}{N}\sum a_{im})}{\mu_m} - \frac{(1/N)\sum_i(\phi_{ib} - \frac{1}{N}\sum\phi_{ib})(a_{ib} - \frac{1}{N}\sum a_{ib})}{\mu_b}\right)\right] \\
={}& \widetilde{\theta}\left[1 + N\left(\frac{\frac{1}{N}\sum(\phi_{im} - \frac{1}{N}\sum\phi_{im})(e_{io} - \frac{1}{N}\sum e_{io}) - \frac{1}{N}\sum\phi_{im} - \frac{1}{N}\sum\phi_{im})(e_{im} - \frac{1}{N}\sum e_{im})}{(E_o - E_m)\mu_m} - \right.\right. \\
&\left.\left. \frac{\frac{1}{N}\sum(\phi_{ib} - \frac{1}{N}\sum\phi_{ib})(e_{io} - \frac{1}{N}\sum e_{io}) - \frac{1}{N}\sum\phi_{ib} - \frac{1}{N}\sum\phi_{ib})(e_{im} - \frac{1}{N}\sum e_{im})}{(E_o - E_b)\mu_b}\right)\right]
\end{aligned}
$$

Define the ratio of total abatement under policy m to that under policy b as $\delta = (E_o - E_m)/(E_o - E_b)$. If we assume that distortions ϕ_{is} are uncorrelated with emissions in the absence of policy e_{io}, we have

$$
\overline{\theta} \approx \widetilde{\theta}\left[1 - \frac{N(\delta - 1)}{E_b - E_m}\left(\frac{\varrho_m}{\delta\mu_m} - \frac{\varrho_b}{\mu_b}\right)\right]
$$

where $\varrho_s = \frac{1}{N}\sum(\phi_{is} - \frac{1}{N}\sum\phi_{is})(e_{io} - \frac{1}{N}\sum e_{io})$ is the population covariance between distortions and emissions. Observe that $\ln\delta$ can be recovered directly from our difference-in-differences estimator on the policy-induced change in emissions.

B Data appendix

B.1 Record linkage procedure

To match plants over time between the U.S. Census Bureau and the pollution data, we use different combinations of non-unique identifiers, namely plant name, plant address, industry classifiers, zip code, and FIPS county codes.

Specifically, we first clean plant name and plant address in both the external and the ASCM data by performing a series of corrections and standardizations. For example, for plant names we remove a large range of company suffixes such as CO and INC, and for addresses we remove common street identifiers. We further drop and clean common expressions, special characters, and spelling errors from the plant names and addresses. This step is crucial to increase the quality of plant names and address between the data.

In the second step, we iteratively block match our standardized data using different combinations of non-unique identifiers. Specifically, for each plant in the external pollution data, we attempt to find them in the ASMCM. By blocking, we reduce the number of potential comparisons made. For example, if we block on FIPS code and 6-digit NAICS, then the names and addresses of a refineries in Santa Barbara County in the CARB data are only matched to name and addresses of refineries in Santa Barbara County in the ASMCM data. Importantly, we do not block on matches on years. This allows us to account for variation in plant names, addresses, or other identifiers over time between plants. Changes in plant name could reflect typographical error, but it could also reflect changes in ownership. Similarly, changes in industry classifier could be a consequence of spurious industry switching in the data, or could be legitimate industry switching documented as establishments respond to economic shocks (Chow et al., 2021).

After each matching iteration, we remove the uniquely match plants from each data before moving on to the next matching iteration. In the first iteration, we use the most stringent matching statement by matching exactly by name, address, within industry and geographic blocks. All uniquely matched pairs of plant IDs between the two data are removed from the data. More than half of our matches come from this most stringent matching argument. In the following iterations of matching, we block the data on different combinations of industry identifiers and geographic identifiers, and then exact or fuzzy match on plant name or plant address. We again keep the sets of matched unique plants identifiers. To further ensure the quality of the matches, hours of clerical review by the researchers were conducted to review matches at all steps of the record linkage algorithm.

Table B.1 and B.2 provide an highly stylized example of our matching procedure. Hypothetical data 1 and data 2 each have a unique plant with varying plant names and NAICS across three year. Such missing or changing of plant identifiers is common in both our external pollution and ASMCM data. In this hypothetical case, for any given year, exact matching on year, standardized name, and 3-digit NAICS would not return any match. However, matching instead on the respective sets of names and NAICS for both plants, the year 2002 combination for data 1 would exactly match to the year 2000 combination for data 2. We use a similar approach of comparing the sets of non-unique identifiers for each unique plant between the data for our formal match.

Table B.1: Potential match candidate from hypothetical data 1

unique ID data 1	Year	Plant name	NAICS (3-digit)
plant_1	2000	GOLETA REFINERY	324
plant_1	2001	GOLETA REFINERY	
plant_1	2002	COASTAL PETROLEUM	324

Table B.2: Potential match candidate from hypothetical data 2

unique ID data 2	Year	Plant name	NAICS (3-digit)
A001	2000	COASTAL PETROLEUM	324
A001	2001	GOLETA REFINERY	325
A001	2002		324

C   Figure appendix

Figure A1: NBP NO$_x$ emissions and cap
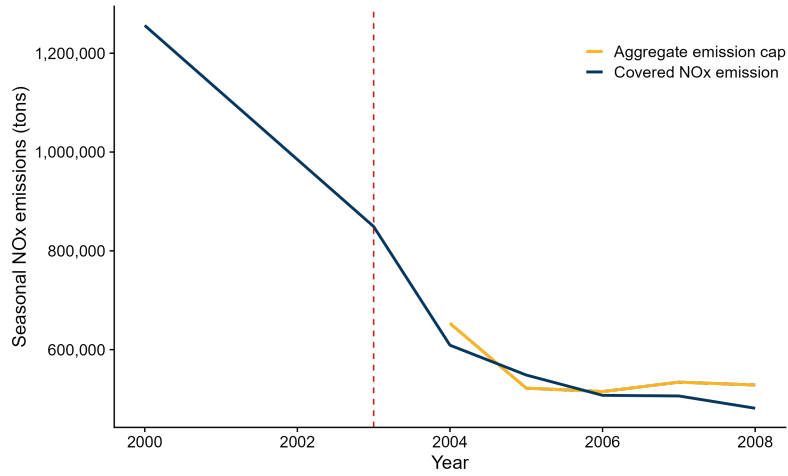
Notes: Seasonal NBP NO$_x$ emission trends, and aggregate emission allowance budgets. The year 2003 cap is omitted from the graph since not all states had joined the NBP yet (U.S. Environmental Protection Agency, 2009)

Figure A2: Semi-parametric allocative efficiency effects by policy
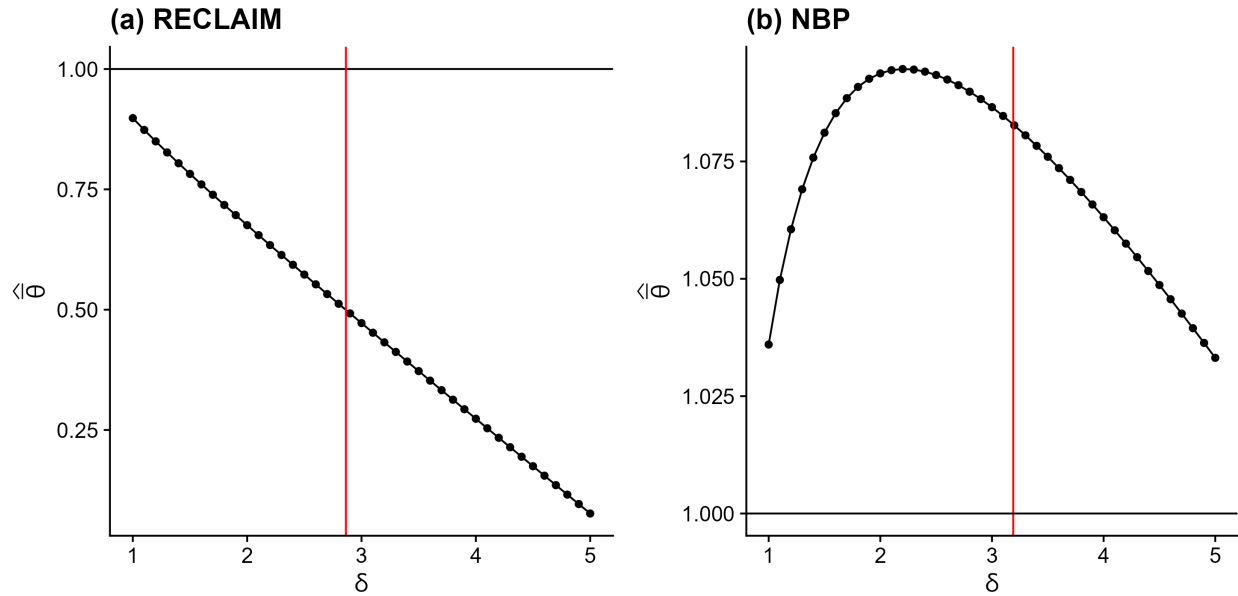
Notes: Semi-parametric measure of allocative efficiency change $\widehat{\overline{\theta}}$ from eq. 11 by policy and range of abatement ratio across policies (δ).

D Table appendix

Table A1: Summary statistics of RECLAIM treated and control plants in CARB data

Post	RECLAIM	Observations	Plants	Mean of NOx	SD of NOx
0	0	12,910	3,838	31.46	244.21
0	1	1,686	304	70.26	282.4
1	0	11,177	3,425	23.18	212.6
1	1	1,198	285	48.45	179.36

Notes: Post is a dummy variable equal to one for the years after 1999. RECLAIM is a dummy equal to one if a California manufacturing plant is covered by RECLAIM. SD = standard deviation. NOx emissions are measured in tons, and TVS in dollars.

Table A2: Summary statistics of RECLAIM treated and control plants in matched data

Post	RECLAIM	Observations	Plants	Mean of NOx	SD of NOx	Mean of TVS/NOx	SD of TVS/NOx
0	0	5,300	1,900	57.95	337.2	242,000	2,703,000
0	1	900	200	101.40	354.7	29,000	103,000
1	0	4,500	1,600	39.01	265.9	1,352,000	14,040,000
1	1	700	200	66.89	223.5	129,000	1,429,000

Notes: Post is a dummy variable equal to one for the years after 1999. RECLAIM is a dummy equal to one if a California manufacturing plant is covered by RECLAIM. Numbers are rounded based on the U.S. Census Bureau's rounding rules (U.S. Census Bureau, 2022). SD = standard deviation. TVS = Total Value of Shipment. NOx emissions are measured in tons, and TVS in dollars.

Table A3: Summary statistics of NBP treated and control plants in matched data

Post	RECLAIM	Observations	Plants	Mean of NOx	SD of NOx	Mean of TVS/NOx	SD of TVS/NOx
0	0	11,500	8,100	65.91	411.2	5,281,000	475,700,000
0	1	100	50	2,200.00	2,900.0	4,800	13,500
1	0	4,800	4,800	67.68	311.3	3,079,000	130,000,000
1	1	60	60	1,500.00	1,900.0	13,000	32,000

Notes: Post is a dummy variable equal to one for the years after 2002, NBP is a dummy equal to one if a U.S. manufacturing plant is covered by NBP. California plants are excluded because of the confounding of RECLAIM. Numbers are rounded based on the U.S. Census Bureau's rounding rules (U.S. Census Bureau, 2022). SD = standard deviation. TVS = Total Value of Shipment. NOx emissions are measured in tons, and TVS in dollars.