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

Kyle C. Meng and Vincent Thivierge*

April 16, 2025

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 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.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, allocative efficiency improved by 10 percentage points. For the U.S.'s NOx Budget Program (NBP), we do not detect efficiency gains. We rationalize this result by showing that prior to the introduction of the emission markets, baseline levels of heterogeneity in the marginal abatement cost (MAC) for regulated plants in RECLAIM was higher than for manufacturing plants in the NBP. This heterogeneity MAC is directly related to potential cost savings from the introduction of emission markets. Furthermore, heterogeneity analyses suggest the plant and firm-level flexibility in pollution abatement options, and the regulator's time commitment to the policy matter for the efficiency gains of pollution markets.

*Meng: Bren School, Dept. of Economics, and emLab, UC Santa Barbara and NBER (email: kmeng@bren.ucsb.edu) Thivierge: Dept. of Economics, and Institute of the Environment, University of Ottawa (email: vincent.thivierge@uottawa.ca). We thank Antony Millner, Chris Costello, Olivier Deschênes, Bernard Salanie, Gonzalo Vazquez-Bare, and members of the UCSB environmental economics research group, and seminar participants at the Université de Montréal, University of Ottawa, Georgia Tech, Resources for the Future, UC Berkekley, and the 7th Annual LSE/Imperial Workshop on Environmental Economics. Any views expressed are those of the authors and not those of the U.S. Census Bureau. The Census Bureau's Disclosure Review Board and Disclosure Avoidance Officers have reviewed this information product for unauthorized disclosure of confidential information and have approved the disclosure avoidance practices applied to this release. This research was performed at a Federal Statistical Research Data Center under FSRDC Project Number 2581. (CBDRB-FY22-P2581-R10061).

1 Introduction

Policies that introduce market incentives can enable greater allocative efficiency in settings where markets are traditionally missing.[1] Determining actual efficiency changes following a market-based policy, however, is challenging. Allocative efficiency is tightly linked to the dispersion in input prices. When markets are missing, so too are prices, making it difficult to establish the extent of misallocation before a policy and thus any changes following it. This paper develops a quasi-experimental framework for estimating allocative efficiency changes in such settings.

We apply this framework to environmental markets, a domain particularly well-suited for market-based policies both because of the canonical view of pollution as a "missing market" problem (Coase, 1960; Arrow, 1969) and because substantial heterogeneity across polluters suggests allocative efficiency gains. Theory developed five decades ago establishes that an environmental market, sometimes known as "cap-and-trade", can achieve an aggregate pollution target at minimum total cost through allocating pollution cuts 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, local air pollution, 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 producers' marginal products, leading to misallocation (Restuccia and Rogerson, 2008; Hsieh and Klenow, 2009; Restuccia and Rogerson, 2013). 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 assumptions, our quasi-experimental estimator recovers a lower bound on the relative change in abatement cost across policies, our theoretical 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 concerns in the misallocation literature, including cross-sectional heterogeneity and endogeneity in firm-specific demand 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

[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).

Regional Clean Air Incentives Market (RECLAIM) and the eastern U.S. NO_x Budget Program (NBP). These markets are notable for their scale, covering nearly all major polluting facilities within their jurisdiction.[2] 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 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 respectively 18% and 26% 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.

By contrast, we do not detect allocative efficiency changes under the NBP, nor across different manufacturing industries. Between policies, we rationalize this result through the observation that across a wide range of underlying characteristics of MAC of regulated plants, baseline heterogeneity of MAC is systematically greater for the set of manufacturing plants covered by RECLAIM than under the NBP. Hence, before the implementation of the markets, there was more potential for cost-savings under RECLAIM than under NBP, and our ex-post data supports this ex-ante observation. Within policies, heterogeneity analyses provide suggestive evidence that plant and firm-level flexibility in pollution abatement options, and the regulator's time commitment to the policy matter for the efficiency gains of pollution markets.

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.[3] 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.[4] 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 of market-based environmental policies. Prior studies have focused on how such policies affect

[2]The words plant and facility are used interchangeably throughout the manuscript. Importantly, either are different than a firm who could own or operate more than one plant or facility.

[3]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).

[4]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.

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).

Recent paper have extended this tradition by combining causal inference evidence with structural models to study the efficiency consequences of market-based environmental policies. Rafey (2023), and Aronoff and Rafey (2023) combine causal parameter identification with structural models to study the gains from trade in water and wetland markets. Greenstone et al. (2023) combine 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. Also, when comparing emission markets to alternative policy instruments, these papers often assume the same aggregate environmental target across policy options. In this paper we document an important feature of the introduction of environmental markets, which is that they reduce substantially aggregate emissions. This implies that facilities under the market can be operating in very different regions of the abatement space then in the counterfactual. Our quasi-experimental framework allows for different caps across policy spaces.

Shapiro and Walker (2024) use regional air pollution offset markets under the US Clean Air Act as a revealed preference measure of the marginal abatement cost of pollutants. The authors compare these regional offset to the marginal benefit of pollution abatement to look at the efficient level of air pollution across the US. Our study departs from Shapiro and Walker (2024) by allowing for plant specific distortions, which implies that observed market prices in emission markets need not be the true cost-minimizing MAC.

Finally, we contribute to the 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 quasi-experimental approaches to examine the causes of misallocation (i.e., the direct approach) (Restuccia and Rogerson, 2017), with a focus on capital market liberalization policies (Bau and Matray, 2023; Sraer and Thesmar, 2023). As with Bau and Matray (2023), we argue that a quasi-experimental estimator can address potential concerns about measurement error (Bils, Klenow and Ruane, 2021) and misspecification (Haltiwanger, Kulick and Syverson, 2018). However, in contrast to Bau and Matray (2023) and Sraer and Thesmar (2023), we develop a direct link between our quasi-experimental estimate and our theoretical estimand, enabling us to quantify misallocation directly without requiring a separate aggregation formula that needs either calibrated structural parameters or assumptions about input prices.Finally, the introduction of a market may be qualitatively different than changes to existing capital markets: 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 or production function, we do not analyze the specific abatement decisions of facilities following a market-based policy, such as abatement technology adoption, which may shed light on the type of decisions that alter misallocation costs (Linn, 2008; Fowlie, 2010; Chan et al., 2018). Finally, we rely on distributional and production function assumptions in our theory to facilitate a mapping between our quasi-experimental estimator and the change in allocative efficiency.

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 main results. Section 6 discusses mechanisms for our results. Section 7 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 environmental 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).[5] 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; Godby, 2002), complementary policies (Bohi and Burtraw, 1992; Fowlie, 2010), and input 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; Godby, 2002), transaction costs (Stavins, 1995), non-compliance (Malik, 1990), and rent-seeking (Harstad and Eskeland, 2010). 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

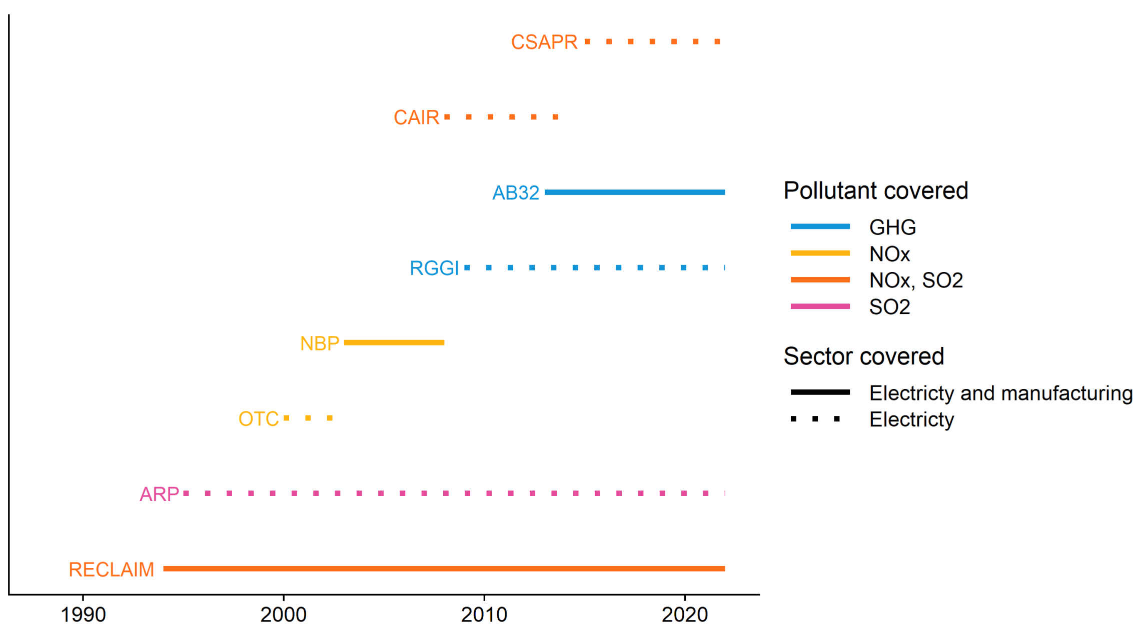
[5]For excellent reviews of this intellectual history, see Tietenberg (2010*a*), Tietenberg (2010*b*), Berta (2017), and Banzhaf (2020).

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.[6] These experiments 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.

[6]See Carlin (1992) for other early air pollution markets.

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.[7] 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). While the Ozone Transport Commission (OTC) and Clean Air Interstate Rule (CAIR) covered a some manufacturing facilities, it was less than in the NBP that respectively followed and preceded both markets. RECLAIM, ARP, RGGI, AB32 and the markets under the Cross-State Air Pollution Rule (CSAPR) are still operational as of 2024.

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.[8] 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).

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

[7] 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.

[8] 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).

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 NOx 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.[9] 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.[10]

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 A2 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).[11]

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

[9]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 Virginia, and Washington, DC.

[10]SIC and NAICS are separate classification regimes with no one-to-one mapping for detailed classification levels. SIC 3-digit provides about the same level of detail as NAICS 4-digit.

[11]As detailed in Section 2.2, we do not consider the CAIR market since the market drops most NBP-covered manufacturing plants.

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

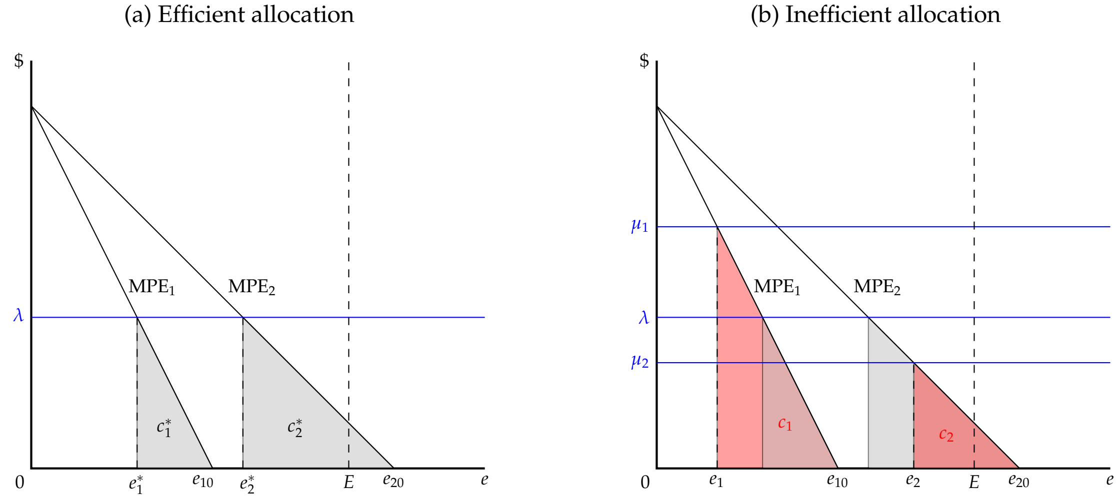
This section details our framework, linking theory and empirics, to estimate the change in allocative inefficiency following the introduction of a market-based policy. Section 3.1 begins with a stylized example to illustrate why this is empirically challenging. Section 3.2 presents a model of environmental policy that informs our estimand, a measure capturing the change in allocative inefficiency of emissions across two arbitrary policies that can be brought to data. Section 3.3 introduces our quasi-experimental estimator.

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.[12] 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.

[12]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.

Figure 2: Environmental policy and allocative (in)efficiency



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 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).[13] 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

[13] 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.

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

$$
\begin{aligned}
\Pi_i^* = \max_{e_i, z_i} \quad & \sum_i p(q_i) q_i(e_i, z_i) - w z_i \\
s.t. \quad & \sum_i e_i = E_s \\
= \max_{e_i, z_i} \quad & \sum_i p(q_i) q_i(e_i, z_i) - w z_i - \lambda_s \Big(\sum_i e_i - E_s\Big)
\end{aligned} \tag{1}
$$

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

$$
\begin{aligned}
\Delta \Pi_s^* &= (E_o - E_s) \frac{d\Pi_s}{dE_s}_{|E_s} + \mathcal{O}^2 \\
&\approx (E_o - E_s)\lambda_s
\end{aligned} \tag{2}
$$

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

$$\begin{aligned}
\pi_{is}(e_{is}) = & \max_{e_i, z_i} \quad p(q_i)q_i(e_i, z_i) - wz_i \\
& \quad s.t. \quad e_i = e_{is} \\
= & \max_{e_i, z_i} \quad p(q_i)q_i(e_i, z_i) - wz_i - \lambda_s \phi_{is}(e_i - e_{is})
\end{aligned} \tag{3}$$

where, following the misallocation literature, ϕ_{is} is a facility-level distortion term, or wedge, that potentially breaks the equivalence between the aggregate shadow price under efficient allocation and the facility-level shadow price at e_{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. Observe that eq. 3 encompasses a wide range of regulatory environments. For example, under a command-and-control regulation, one can view the regulator as explicitly setting each facility's $\lambda_s \phi_{is}$ in order to achieve a prescribed e_{is}. Under an emissions trading policy, the aggregate emissions cap would determine λ_s while any facility-specific trading frictions would be captured in ϕ_{is}.

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

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

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

$$\theta_s = \frac{\sum_i (e_{io} - e_{is}) \lambda_s \phi_{is}}{(E_o - E_s)\lambda_s} = \sum_i a_{is} \phi_{is} \tag{5}$$

where $a_{is} = \frac{e_{io} - e_{is}}{E_o - E_s}$ are weights capturing facility-level shares of total abatement with $\sum_i a_{is} = 1$. There are two limitations to θ_s. First, observe that under allocative efficiency, $\phi_{is} = 1\ \forall i$ implies $\theta_s = 1$. However, the reverse is not in general true. Second, empirically, we do not directly observe facility-level abatement shares a_{is}[14] nor facility-level distortions, ϕ_{is}, the two ingredients that go into θ_s. To make progress on both

[14] 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

issues, we turn to two additional assumptions.

Assumption 1. *Facility distortions are distributed* $\phi_{is} \sim \mathcal{LN}(0, \sigma_s^2)$

Assumption 2. *Facility abatement share* a_{is} *is increasing in (a)* ϕ_{is} *and (b)* $\phi_{is} \frac{\partial a_{is}}{\partial \phi_{is}}$.

Assumption 1 provides two intuitive restrictions on facility-level distortions. First, $\phi_{is} > 0$ when it is lognormally distributed, ensuring that the facility-level shadow price of emissions is never negative.[15] Second, $\phi_{is} \sim \mathcal{LN}(0, \sigma_s^2)$ has a median value of one such that there is an equal number of over- and under-abating facilities relative to the efficient allocation. This normalization allows for distortions to alter the dispersion of emissions across facilities through σ_s without changing the aggregate emissions level. Assumption 2 also has an intuitive interpretation, capturing the idea that emission abatement is increasing in distortions but not excessively.[16] This leads to our first proposition establishing our measure of allocative efficiency.

Proposition 1. *Under Assumptions 1, 2a and 2b, (a)* $\theta_s = 1$ *implies allocative efficiency, or* $\phi_{is} = 1 \ \forall i$ *when* $\sigma_s^2 = 0$ *and (b)* θ_s *is increasing in* σ_s^2.

That is, not only does $\theta_s = 1$ imply allocative efficiency, but efficiency losses are increasing in the dispersion of distortions. Appendix A.1 details the proof. In practice, policy changes often entail both a change in total emissions across facilities, E_s, as well as the vector of facility-level emissions, $\mathbf{e}_s$. In such settings, a natural measure is the ratio of misallocation costs across policies. That is, if we consider two policy states $s \in \{b, m\}$, where b indicates the baseline policy and m indicates the market-based policy, we are interested in

$$\frac{\theta_m}{\theta_b} = \frac{\sum_i a_{im}\phi_{im}}{\sum_i a_{ib}\phi_{ib}} \tag{6}$$

$\frac{\theta_m}{\theta_b}$ cannot be directly estimated because facility-level abatement shares, a_{is}, and distortions, ϕ_{is} are unobserved. Instead our second proposition provides a bounding argument for a statistic that could be estimated.

Proposition 2. *Under Assumptions 1 and 2b,* $\frac{E[\phi_m]}{E[\phi_b]}$ *is a lower bound on* $\frac{\theta_m}{\theta_b}$. *That is,* $\frac{\theta_m}{\theta_b} - \frac{E[\phi_m]}{E[\phi_b]} < 0$ *if* $\frac{\theta_m}{\theta_b} < 1$ *and* $\frac{\theta_m}{\theta_b} - \frac{E[\phi_m]}{E[\phi_b]} > 0$ *if* $\frac{\theta_m}{\theta_b} > 1$.

Appendix A.2 provides the proof. Here, we highlight there is a natural link between $\frac{E[\phi_m]}{E[\phi_b]}$ and the change in the variance of distortion across policies, an observation made elsewhere in the misallocation literature (Hsieh and Klenow, 2009). That is, $\frac{E[\phi_m]}{E[\phi_b]} > 1$ when the variance of distortions increase following the policy change while $\frac{E[\phi_m]}{E[\phi_b]} < 1$ when the variance of distortions decrease.[17]

---

be observed.

[15] We take a super-population perspective whereby our set of N facilities is drawn from a super-population of facilities with lognormally distributed distortions. Expectations are therefore taken over sampling uncertainty when drawing from the super-population.

[16] In Appendix A.2, we discuss how Assumption 2b also implies weakly positive abatement shares.

[17] To see this, under Assumption 1

$$\frac{E[\phi_m]}{E[\phi_b]} = e^{\frac{\sigma_m^2}{2} - \frac{\sigma_b^2}{2}}$$

Since $\frac{1}{2}(var(\ln\phi_{im}) - var(\ln\phi_{ib})) = \frac{\sigma_m^2}{2} - \frac{\sigma_b^2}{2}$, $\frac{E[\phi_m]}{E[\phi_b]} > 1$ when $var(\ln\phi_{im}) - var(\ln\phi_{ib}) > 0$ and $\frac{E[\phi_m]}{E[\phi_b]} < 1$ when $var(\ln\phi_{im}) - var(\ln\phi_{ib}) < 0$.

3.3 From theory to empirics

Proposition 2 points to $\frac{E[\phi_m]}{E[\phi_b]}$ as our estimand. But estimation still requires the change in expected (though not 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[18], 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). On the supply side, firm-heterogeneity in output elasticities provides the impetus for market-based environmental policies in the first place as they related to heterogeneity in abatement costs. Rewriting eq. 7 as average revenue per emissions, $AR_{is} = \frac{p_i q_{is}}{e_{is}}$, yields

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

Eq. (8) suggests a possible regression specification. However, two additional considerations arise when bringing eq. (8) to any empirical setting, both of which can be addressed using a quasi-experimental estimation approach. First, there is the possibility of other changes coinciding with a policy introduction that are left out of the 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.[19] 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 in a quasi-experimental framework.

Second, the first order condition in eq. (7) may be misspecified. For example, rather than being fixed, firm-specific demand and output elasticities may themselves be functions of distortions. If so, one wants to quantify misallocation as a consequence of both direct distortion effects and indirect effects mediated through changes in demand and output elasticities. A quasi-experimental approach facilitates this by providing a reduced-form effect of a policy on misallocation inclusive of all potential endogenous channels.

We implement a two-step quasi-experimental estimation procedure. The first step recovers changes in policy-wide mean parameters. The second step estimates changes in policy-wide dispersion in distortions. Define $\mathcal{B}$ as the set of control facilities and $\mathcal{M}$ as the set of treated facilities and let t indicate year relative to the last year before adoption of the market-based policy. Our first step estimation involves an event study regression analog to structural equation (8)

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

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

[19] 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.

where D_i is a dummy variable that equals one for treated facility $i \in \mathcal{M}$ 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 contains any remaining error, ζ_{it}, perhaps due to misspecification or mismeasurement. To recover our dispersion parameters and ultimately $\frac{E[\phi_m]}{E[\phi_b]}$, we square the predicted residuals $\widehat{\nu}_{it}$ after estimating eq. (9) and estimate a similar second-stage regression

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

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, v_t captures annual changes in the dispersion of distortions for the control group relative to the omitted year.

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. The flexible function form of eq. (10) allows for the testing of pre-trends and time-varying policy change effects. 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. This maps to our estimand: $e^{\frac{\widehat{\beta}^\tau}{2}} = \frac{E[\phi_{mt}]}{E[\phi_{bt}]}$ Observe that these reduced-form coefficients incorporate any endogenous changes in firm-level parameters - such as demand and output elasticities - in response to distortions and as such is inclusive of potential misspecification in these parameters in the first order condition contained in eq. (7).

As with our first stage estimation, we also consider a difference-in-differences variant of eq. (10)

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

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.

Finally, for eqs. (9), (9'), (10), and (10'), we cluster standard errors at a broader jurisdictional level (e.g., zip code for RECLAIM or county for the NBP) to account for arbitrary forms of spatial correlation and serial correlation in the residual within facilities of that jurisdiction.[20]

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.

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 Walker, 2018) or used private plant-level data that proxy plant revenue.[21] 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.[22] 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

To study the NBP, facility NO_x pollution emissions and facility characteristics come from the U.S EPA National Emissions Inventory (NEI), which reports emissions of criteria pollutants for large point sources. Since the NEI only reports emissions every three year starting in 1996, the NBP sample is constrained to

[20] Ideally we would cluster also at the county level for RECLAIM, however there are only 4 counties in the treated group, which would lead to a over-rejection of the null Carter, Schnepel and Steigerwald (2017). We therefore follow Grainger and Ruangmas (2018) who causally study the empirical effects of RECLAIM and cluster their standard errors at the zip-code level.

[21] 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.

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

every three years from 1996 to 2008.[23] Facility-level characteristics in the NEI used in our merge with the ASMCM (detailed below) include facility name, address, NAICS code, zip code, and county code.

To find the NBP treatment status of plants in the NEI, we supplement the NEI data with data on the plants covered under the NBP through the U.S. EPA's Air Market Program Data (AMPD). Through the U.S. EPA's Facility Registration Services (FRS), we use the common AMPD ID between the AMPD and the NEI to find treated manufacturing plants. Since not all NEI plants include a AMPD ID, we also merge data on the treatment status of plants from Curtis (2018) using other common U.S. EPA's FRS IDs to find more treated plants.[24]

Lastly, we obtain a measure of plant-level pollution and abatement capital expenditure from the 1999 restricted-use U.S. EPA Pollution Abatement Costs and Expenditures survey (PACE) accessed through the U.S. Census Bureau.

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.

[23]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.

[24]The AMPD also reports yearly NO_x emissions for plants covered by the NBP, and other facilities covered by other US EPA air pollution markets, like the Acid Rain Program's (ARP).There are three reasons why we do not 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 none report pre-1999 emissions, (2) supplementing missing facility-level AMPD emissions data with NEI data risks downward biasing our results since AMPD data records a subset of facility-level emissions relative to the NEI since it reports boiler-level and not facility-level emissions like the NEI, and (3) 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.

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 more than 90% of the 100 NBP manufacturing facilities to the ASMCM data by combining our algorithm and supplementing it with the matched NBP regulated plants to the NEI in Curtis (2018).

5 Empirical results

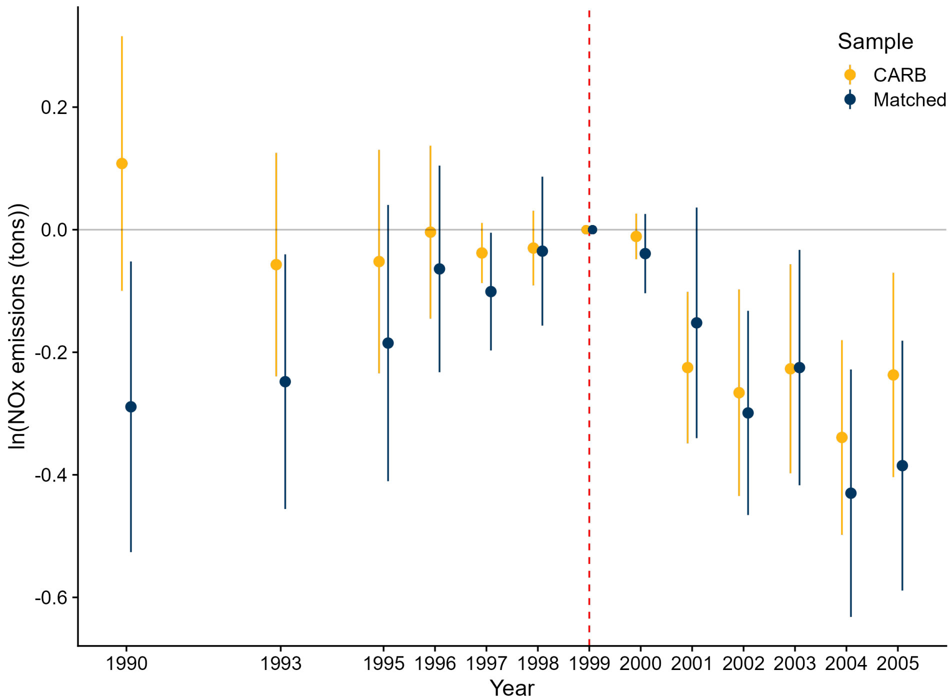
This section applies our empirical framework to the RECLAIM and NBP NO_x cap-and-trade markets. Using event-study and difference-in-differences models, we first establish that the introduction of the markets reduced NO_x emissions, consistent with results found elsewhere in the literature. We then report 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′). Section 5.1 presents results 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 by the cap-and-trade program. 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

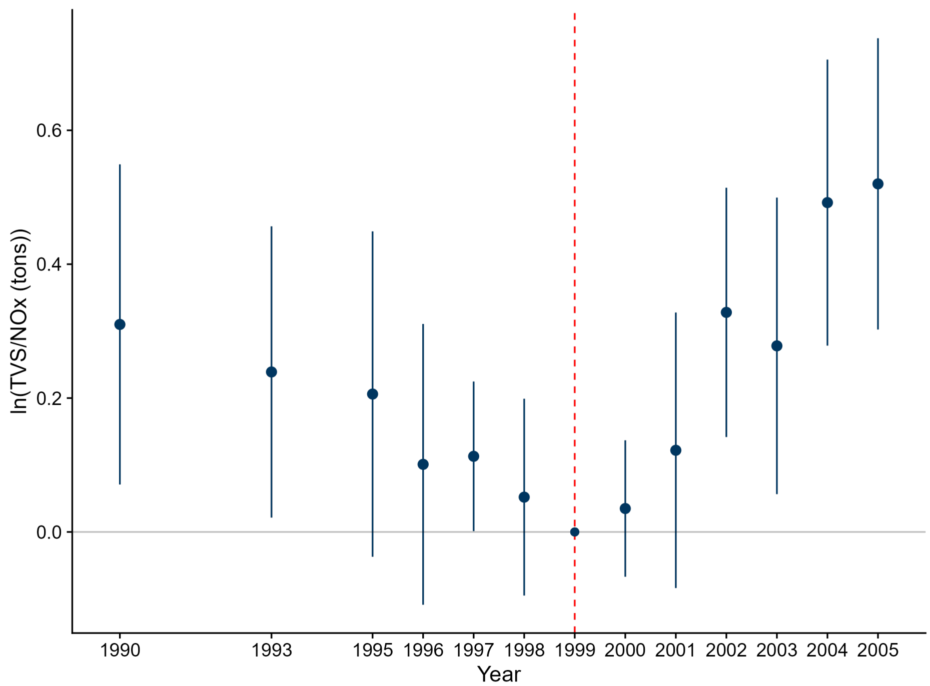


Notes: Point estimates and 95% confidence intervals of the yearly effect of RECLAIM on log NO_x emissions relative to 1999 using eq. (9). Estimates for the full sample of manufacturing facilities in CARB shown in gold. Estimates for the CARB-ASMCM matched sample shown in blue. Standard errors are clustered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

Columns (1) and (2) of Table 2 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 0.18 log points or 17% reduction compared to polluting facilities in the rest of California. While the average emissions effect for the CARB-ASMCM matched sample is a smaller 0.12 log points or 11% reduction, it is not statistically different from the full CARB sample. This NO_x emission reduction effect is broadly consistent with causal emissions estimates from previous studies, though these studies have not separately examined only manufacturing facilities (Fowlie, Holland and Mansur, 2012; Grainger and Ruangmas, 2018; Mansur and Sheriff, 2021). Importantly, 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 $\widehat{\alpha}^{\tau}$ using eq. (9). Standard errors are clustered at the zip code level.

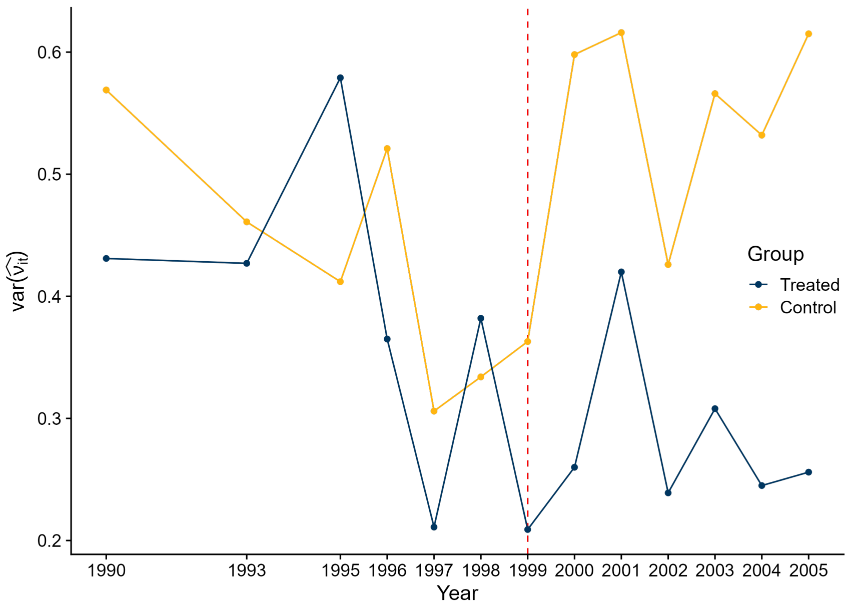
Column (3) of Table 2 presents the average treatment effect of RECLAIM on log average revenue using equation (9′). The estimated coefficient represents the effect of RECLAIM on the aggregate NO_x shadow price. Consistent with the emissions effects detected in columns (1) and (2), RECLAIM increased the aggregate shadow price for NO_x emissions by 14 log points or 15%.

Before turning to our theory-informed estimate of the change in allocative efficiency under RECLAIM, we turn to two separate intuitive tests of the policy-driven change in the dispersion of distortions. We look at, (1) the change of the variance of distortions across treated and control plants over time, and (2) we estimate the differential treatment effects by an ex-ante measure of facility-level distortions.

First, 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, $\widehat{\nu}_{it}$, from eq. (9), for treated facilities relative to control facilities after the market introduction. While the change in cross-sectional variance is not 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.

Second, following Bau and Matray (2023), we look at differential treatment effects by our ex-ante measure of facility-level distortion. Table 1 shows the estimates of differential effects for RECLAIM by interacting the treatment variable in equation 10′ with a dummy variable equal to 1 if the regulated facility has a lower than median average predicted residual from 9 in the pre-treatment period. The Table looks at these differential effects on log NO_x emissions and the predicted residual value. If RECLAIM is operating as intended, we should see differential changes in emissions and predicted residual, a proxy for facility-level distortions, for the ex-ante low versus high MAC plants.

Following the introduction of RECLAIM, high MAC treated plants increased their emissions relative to high MAC control plants. This suggests that prior to the introduction of RECLAIM, high MAC plants were over abating NO_x emissions. Following the market introduction, low MAC plants decreased their emissions by 47% relative to high MAC plants. This suggests that the market is working as intended, since more emission abatement is driven by lower MAC plants, and less by high MAC plants.

Turning to the second column, we see that for RECLAIM, the market also leads to an statistically precise increase in the MAC of ex-ante low MAC plants, proxied by the predicted residual, relative to high MAC plants, and a concurrent statistically precise reduction in the MAC by high MAC plants. We should expect that the market leads to the MAC of regulated plants to get closer to each other, that is towards equimarginality. Taken together, both these tests suggest that RECLAIM is leading to allocative efficiency gains for NO_x emissions.

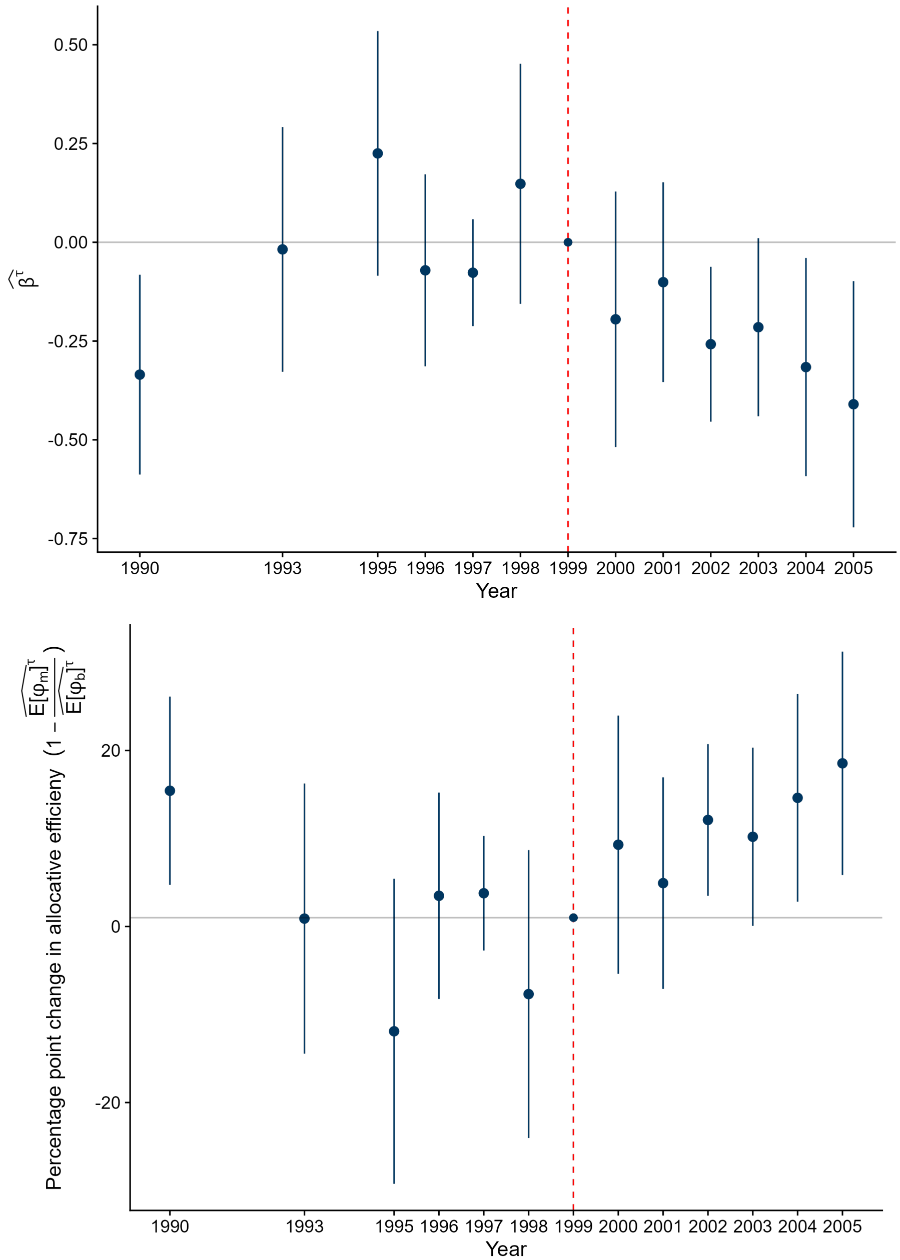
Table 1: Average treatment effect of RECLAIM by ex-ante MAC

	$\ln \mathrm{NO}_x$ emissions	$\widehat{\nu}_{it}$
	(1)	(2)
RECLAIM X Post	0.181^{***}	-1.247^{***}
	(0.067)	(0.286)
RECLAIM X Post X Low MAC	-0.645^{***}	1.384^{***}
	(0.112)	(0.230)
Observations	11,500	11,500

Notes: Estimates of the differential effect of RECLAIM by whether a treated plant had a lower than median average predicted residual from eq 9 in the pre-treatment period for log NO_x emissions and predicted residuals across columns. All models include plant and year fixed effects. Robust standard errors clustered at the zip code level in parentheses.

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, $1 - \frac{\widehat{E[\phi_m]}}{\widehat{E[\phi_b]}} = 1 - e^{\frac{\widehat{\beta^{\tau}}}{2}}$. RECLAIM has increased allocative efficiency 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 $1 - \frac{\widehat{E[\phi_m]}^{\tau}}{\widehat{E[\phi_b]}^{\tau}} = 1 - e^{\frac{\widehat{\beta}^{\tau}}{2}}$. Standard errors are clustered at the zip code level.

Column (4) of Table 2 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, $1 - \frac{\widehat{E[\phi_m]}}{\widehat{E[\phi_b]}} = 1 - e^{\frac{\widehat{\beta}}{2}}$. RECLAIM market led to allocative efficiency gains in NO_x emissions of 10 percentage points. Under the assumptions maintained in Proposition 1, $\widetilde{\theta}$ is a lower bound on, θ, the theory-based changed in allocative efficiency. These estimates provides causal evidence that RECLAIM led to improvements in allocative efficiency in NO_x emissions.

Table 2: Average treatment effects of RECLAIM

	$\ln \text{NO}_x$ emissions	$\ln \text{NO}_x$ emissions	$\ln AR_{it}$	$\widehat{\nu}_{it}^2$
	(1)	(2)	(3)	(4)
RECLAIM X Post	-0.182^{***}	-0.116^{*}	0.142^{*}	-0.215^{**}
	(0.049)	(0.062)	(0.073)	(0.092)
$1 - \frac{\widehat{E[\phi_m]}}{\widehat{E[\phi_b]}}$				0.102
				[0.017, 0.179]
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 $1 - \frac{\widehat{E[\phi_m]}}{\widehat{E[\phi_b]}} = 1 - e^{\frac{\hat{\beta}}{2}}$. Robust standard errors clustered at the zip code in parentheses, and 95% confidence interval in brackets.

Table 3 estimates $1 - \frac{\widehat{E[\phi_m]}}{\widehat{E[\phi_b]}}$ for each 2-digit SIC manufacturing sector by re-estimating eq. (10) in which the treatment variable is interacted with industry indicators. We find $1 - \frac{\widehat{E[\phi_m]}}{\widehat{E[\phi_b]}} > 0$ for every sector, suggesting that allocative efficiency 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 3: Allocative efficiency effect of RECLAIM by industry

Industry	$1 - \frac{\widehat{E[\phi_m]}}{\widehat{E[\phi_b]}}$	95% CI
Petroleum refineries (SIC 29)	0.171	[0.044, 0.281]
Primary metal manufacturing (SIC 33)	0.143	[0.07, 0.211]
Other manufacturing	0.083	[-0.035, 0.187]
Cement and glass manufacturing (SIC 32)	0.076	[-0.003, 0.149]
Secondary metal manufacturing (SIC 34)	0.062	[-0.131, 0.221]
Food manufacturing (SIC 20)	0.035	[-0.093, 0.149]

Notes: Point estimates and 95% confidence interval of allocative efficiency effect, $1 - \frac{\widehat{E[\phi_m]}}{\widehat{E[\phi_b]}}$, by industry. Robust standard errors are clustered at the zip code level.

Following the data cleaning procedure in Lyubich, Shapiro and Walker (2018), we also replicate the results of Table 2 using a sample that was trimmed for extreme changes in emissions or revenue are 100X greater than the 99th percentile change or 100X smaller than the 1th percentile change. 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′) for the trimmed sample shown in Table A2 are quantitatively similar to those in Table 2.[25] Also, in order to account for differential time trends in the effects of RECLAIM shown in Figures 3, 4 and 6, Table A1 estimates a trend-break version of the difference-in-differences models 9′ and 10′. The trend-break model results are consistent with the previous event-study and DID model results.

Results in this section show that RECLAIM led to reductions in NO_x emissions, increases in the NO_x shadow price, and led to increase allocative efficiency of NO_x emissions across regulated plants, as at increasing rate over time. The next sections explores a similar analysis for the NBP NO_x emission market.

5.2 NOx Budget Program

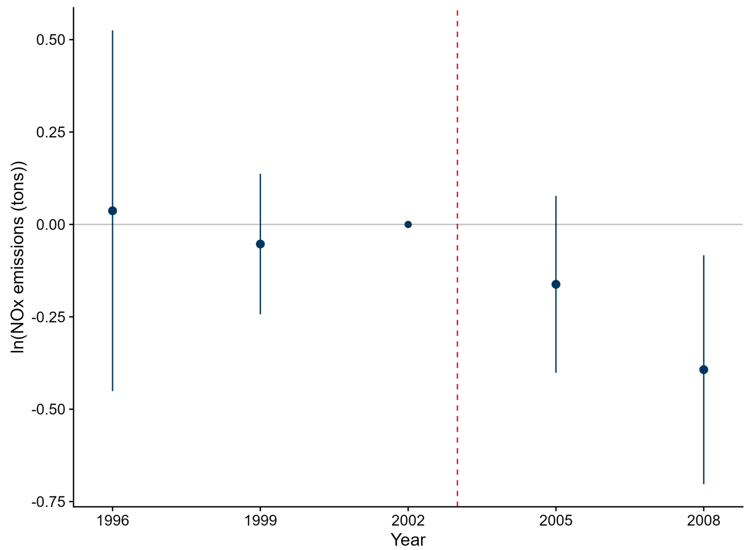
This section presents our results for the NBP emission market. Because the U.S. EPA's NEI data is available triennially, NBP results use data every three years from 1996 to 2008.

Figure 7 presents the NBP effects on NO_x emissions by changing the outcome variable in the event-study model presented in equation 9. 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 post-treatment period coefficients suggest that the program lowered NO_x emissions, especially 5-years after its introduction. Coefficients in the pre-treatment period are not statistically different than zero, therefore suggesting a lack of differential pre-trends across the treated and control plants, a necessary condition for causal identification in event-study and difference-in-differences models.

Column (1) of Table 5 presents the average treatment effect of the NBP on NO_x emissions. The NBP market reduced annual manufacturing facility emissions by about 29% ($e^{-0.342} - 1$). For the NEI sample period used in this study, there does not exist a longitudinal facility identifier. Therefore, we cannot compare the NO_x emission effect across the NEI-ASMCM matched sample, and an NEI-only sample to verify the quality of the matching procedure as we do for the NO_xemission effect for RECLAIM in Figure 3. Instead, 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%, which is included in the 95% confidence interval of our average treatment on emissions.

[25] Ideally, we would replicate the event-study figures 3, 4 and 6, but due to disclosure requirements from the US Census Bureau on sample sizes, we could only output the average treatment effects shown in Table A2.

Figure 7: Event-study model of the effect of NBP on NO_x emissions

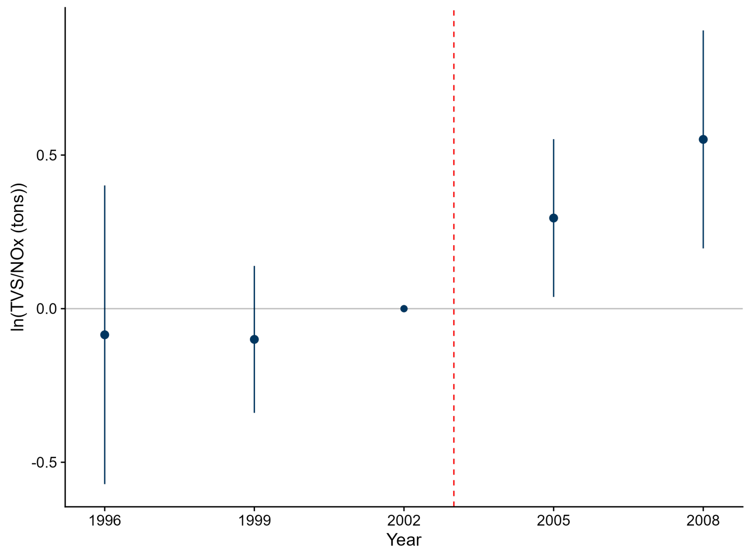


Notes: Point estimates and 95% confidence intervals of the yearly effect of NBP on log NO_x emissions relative to 2002, the year before the NBP was introduced, using eq. (9). Standard errors are clustered at the county level.

Figure 8 shows the effect of the NBP on average revenue per emissions using eq. (9′), which following Section 3.3 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 coefficients in the pre-treatment period suggest a lack of differential pre-trends across treated and control plants.

Interestingly, these estimates of the NBP aggregate NO_x shadow price in Figure 8 qualitatively mirrors the marginal pollution tax effect found in Figure 6 in Shapiro and Walker (2018). Using a structural model, the authors find that the NBP increased the pollution tax of covered manufacturing plants by 1.195 log point. Our quasi-experimental estimate shows a 0.452 log point increase. Indeed, 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 57% 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



Notes: Point estimates and 95% confidence intervals of the yearly effect of NBP on log revenue per emissions relative to 2002, or $\widehat{\alpha}^{\tau}$ using eq. (9). Standard errors are clustered at the county level

As with RECLAIM, before turning to our main estimating equation 10, we first look at two more intuitive tests for whether the NBP altered allocative efficiency, namely (1) the change in the annual cross-sectional variance in estimated residuals, and (2) the differential treatment effect by ex-ante measure of plant-level MAC.

First, we consider the annual cross-sectional variance in estimated residuals, $\widehat{\nu}_{it}$, separately for treated and control facilities. Unlike with RECLAIM, Figure 9 does not shows a clear change in trends in variance of predicted residuals across treated and control facilities. The two time series exhibit roughly similar trends both before and after the introduction of NPB, suggesting that allocative efficiency changes may have been limited under the NBP. If anything, the crossing of the lines between 2005 and 2008 suggest allocative inefficiency increased for manufacturing plants under the NBP.

We now look at differential treatment effects by our ex-ante measure of facility-level distortion as in Bau and Matray (2023). Table 4 shows the estimates of differential effects for the NBP by interacting the treatment variable in equation 10′ with a dummy variable equal to 1 if the regulated facility has a lower than median average predicted residual from 9 in the pre-treatment period. The Table looks at these differential effects on log NO_x emissions and the predicted residual value.

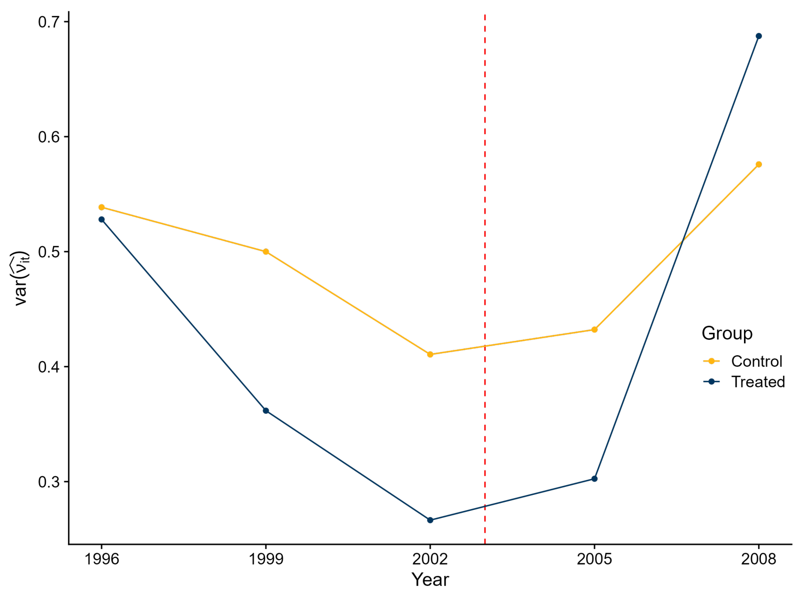
Following the introduction of the NBP, high MAC plants didn't change their emissions, while low MAC plants decreased their emissions by 46% relative to high MAC plants. It appears that the market is working for low MAC plants, since more emission abatement is driven by lower MAC plants, however emissions are not increasing for ex-ante high MAC plants. The story is similar for the second column of Table 4. We see that the market does not lead to a statistically precise decrease in the MAC for ex-ante high MAC plants, while it lead to the an increase in the MAC, proxied by the predicted residual, of ex-ante low MAC plants relative to high MAC plants. We are therefore left with imprecise evidence of the overall performance of the market, i.e. it is working as expected for for low ex-ante MAC plants but not high MAC plants. We now turn to the formal test of the overall market allocative efficiency effect of the NBP for manufacturing plants.

Table 4: Average treatment effect of NBP by ex-ante MAC

	ln NO_x emissions	$\widehat{v}_{it}$
	(1)	(2)
NBP X Post	0.009	−4.696
	(0.074)	(5.291)
NBP X Post X Low MAC	−0.613**	1.848**
	(0.262)	(0.748)
Observations	32,500	32,500

Notes: Estimates of the differential effect of NBP by whether a treated plant had a lower than median average predicted residual from eq 9 in the pre-treatment period for log NO_x emissions and predicted residuals across columns. All models include plant and year fixed effects. Robust standard errors clustered at the county level in parentheses.

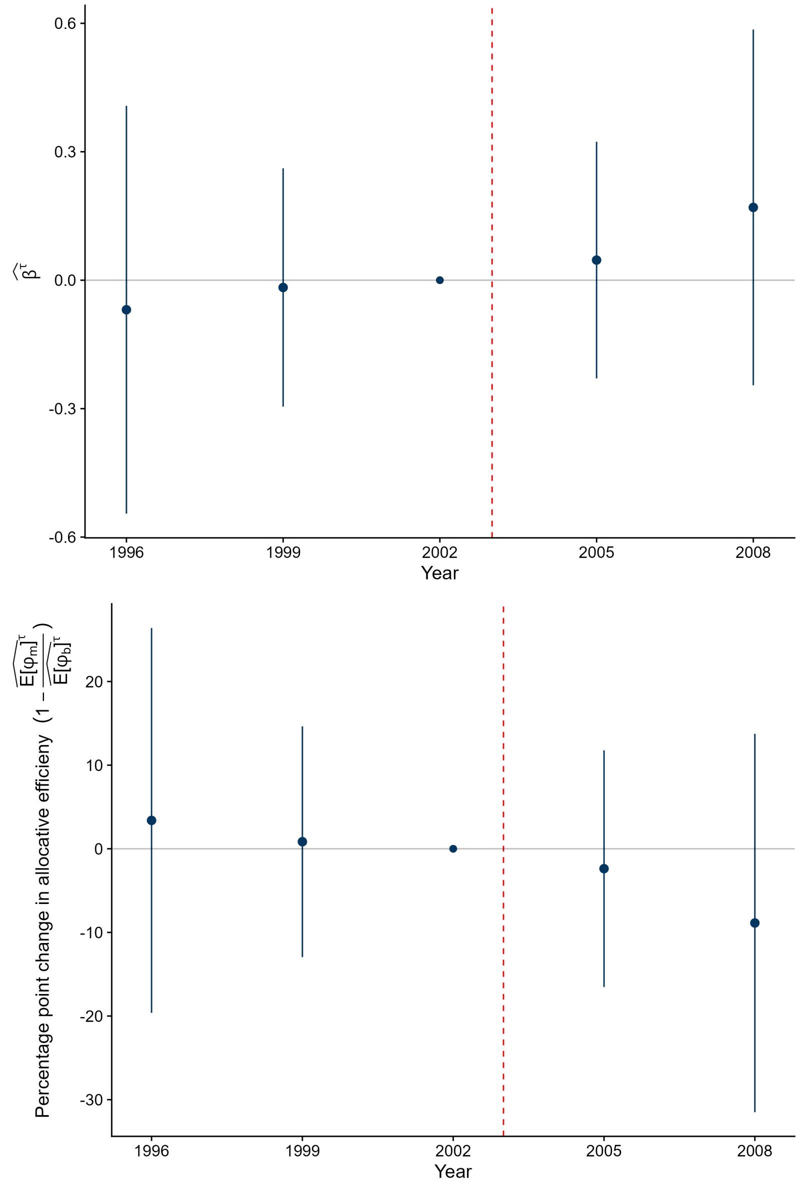
Figure 9: Difference in variance of plant level distortions between NBP treated and control plants



Notes: The blue and gold lines show the yearly variance of the predicted residual, $\widehat{v}_{it}$, from equation 9 for treated and control plants, respectively.

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. (10) 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, $1 - \frac{\widehat{E[\phi_m]}}{\widehat{E[\phi_b]}} = 1 - e^{\frac{\widehat{\beta^{\tau}}}{2}}$, where a decrease in the measure translates to a decrease in allocative efficiency.

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 $1 - \frac{\widehat{E[\phi_m]}^{\tau}}{\widehat{E[\phi_b]}^{\tau}} = 1 - e^{\frac{\widehat{\beta}^{\tau}}{2}}$. 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, $1 - \frac{\widehat{E[\phi_m]}}{\widehat{E[\phi_b]}} = 1 - e^{\frac{\widehat{\beta}}{2}}$. The table suggest an imprecise increase in allocative inefficiency as a result from the policy.

Table 5: Average treatment effect of NBP

	ln NO_x emissions	ln AR_{it}	$\widehat{v}^2_{it}$
	(1)	(2)	(3)
NBP X Post	-0.257^{**}	0.471^{***}	0.124
	(0.123)	(0.151)	(0.134)
$1 - \frac{\widehat{E[\phi_m]}}{\widehat{E[\phi_b]}}$			-0.064
			[-0.213, 0.067]
Observations	32,500	32,500	32,500

Notes: Estimates of the average treatment effect of NBP using a difference-in-differences 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 $1 - \frac{\widehat{E[\phi_m]}}{\widehat{E[\phi_b]}} = 1 - e^{\frac{\widehat{\beta}}{2}}$. Robust standard errors clustered at the county level in parentheses, and 95% confidence interval in brackets.

Table 6 estimates $1 - \frac{\widehat{E[\phi_m]}}{\widehat{E[\phi_b]}}$ for 3 different manufacturing sectors by re-estimating eq. (10) in which the treatment variable is interacted with industry indicators.[26] We find $1 - \frac{\widehat{E[\phi_m]}}{\widehat{E[\phi_b]}} < 0$ for every sector, suggesting small reductions in allocative efficiency across sectors. However, these results are statistically indistinguishable from zero for all sectors.

Table 6: Allocative efficiency effect of NBP by industry

Industry	$1 - \frac{\widehat{E[\phi_m]}}{\widehat{E[\phi_b]}}$	95% CI
Paper manufacturing (NAICS 322)	-0.067	[0.054, -0.202]
Refineries (NAICS 324 and 325)	-0.003	[0.12, -0.144]
Other manufacturing	-0.1	[0.116, -0.368]

Notes: Point estimates and 95% confidence interval of allocative efficiency effect, $1 - \frac{\widehat{E[\phi_m]}}{\widehat{E[\phi_b]}}$, by industry. Robust standard errors are clustered at the county code level.

As for RECLAIM, we also replicate the main NBP results using a similar data trimming procedure as in Lyubich, Shapiro and Walker (2018), where facilties are dropped if changes in NOx emissions or revenue are more than the 100 times the 99th percentile or less than 100 times the 1 percentile of changes. Figures A3 to A5 show that the main results for NO_x emissions, average revenue, and allocative efficiency for the effect of the NBP are robust to trimming the estimation sample.

We have shown that following the introduction of the RECLAIM and NBP NO_x emission markets, average emissions of regulated plants go down, and the NO_x shadow price increases. However, these effects do not necessarily imply allocative efficiency gains. Indeed, we only identify statistically significant

[26]Due to disclosure requirements from the US Census Bureau on sample sizes, these three industry groups are the most disaggregated categories for which we could output results.

improvements in allocative efficiency of NO_x emissions for plants covered by RECLAIM. The following section explores potential mechanisms to explain this result.

6 Mechanisms

Why did RECLAIM but not the NBP deliver allocative efficiency gains? Where did allocative efficiency gains come from? To shed light on these questions, we conduct a series of heterogeneity analyses between the emission markets and within each market.

6.1 Between markets

We first focus on differences across the two NO_x emission markets to help rationalize the evidence of cost-savings from RECLAIM but not the NBP. Particularly, we focus on heterogeneity in plausible proxies of MAC for covered plants in the markets prior to the introduction of each programs. We use proxies, since the MAC of plants is typically thought of as unobserved. Intuitively, all else equal, a set of covered polluting facilities with greater ex-ante heterogeneity in MAC should have greater potential cost-savings from the introduction of an emissions market as opposed to a less flexible environmental regulation to achieve the same aggregate emission reduction target.

More formally, this is what Newell and Stavins (2003) show. They derive an analytical model to estimate the cost-savings of implementing market-based versus other environmental policy instruments. Using a second-order approximation of the regulated entity's cost function, Newell and Stavins (2003) show that potential cost-savings in implementing a market-based policy depends on the coefficients of variation of the slope and intercept of the linear MAC curves of regulated entities. While not explicitly incorporated in their model, the authors highlight differences in location, age, size, and production technology as likely underlying characteristic that explain heterogeneity in MAC of covered plants by a policy.

We draw inspiration from the analytical result in Newell and Stavins (2003) by presenting in Table 7 the ex-ante coefficient of variation (CV) of plausible proxies for differences in MAC for regulated manufacturing plants under RECLAIM and NBP. Specifically, we include the CV of predicted residual from equation 9, since they include a measure of facility-specific distortion which is closely related to MAC in our theoretical framework. We also look at the CV of total of value of shipment, NO_x emissions, total capital expenditures, total employment, plant age, pollution and abatement capital expenditures. Lastly, we also consider the number of unique NAICS-6 industries amongst the covered facilities, as a proxy for diversity of production technologies.

Table 7: Ex-ante heterogeneity of MAC across NO_x emission markets

Variable	$\widehat{v}_{it}$	TVS	NOx	Capital exp.	Emp.	Age	PACE	NAICS
RECLAIM	0.86	2.36	3.66	3.06	2.71	0.43	3.72	100
NBP	0.54	1.77	1.21	1.63	1.95	0.33	2.52	30
Statistic	CV	CV	CV	CV	CV	CV	CV	Count

Notes: The first seven columns show the coefficient of variations across different proxy variables for the marginal abatement cost of treated plants covered by either emission markets in the pre-treatment period. For RECLAIM, the pre-treatment period is before 2000, and it is before 2003 for the NBP. The last column shoes for the same periods andthe unique numbers of 6-digit NAICS in the treated plants. $\widehat{v}_{it}$ are the predicted residuals from eq. 9. All statistics are calculated over the full pre-treatment period, with the exception of $\widehat{v}_{it}$ for the NBP which omits the year 1990 because of an outlier plant which cannot be dropped for disclosure requirements as explained in section XYZ. TVS = Total value of shipment. Capital exp. = Capital expenditures. Emp. = Total employment. PACE = Pollution and abatement capital expenditures. CV = coefficient of variation.

The overall takeaway from Table 7 is that across all measures, the degree of ex-ante heterogeneity is systematically larger for the set of RECLAIM manufacturing facilities as opposed to the set of manufacturing plants under the NBP. In line with Newell and Stavins (2003), these stylize facts suggest that there are more potential cost-savings from implementing a market-based environmental policy for the manufacturing plants covered in RECLAIM versus the NBP. While not a definitive test, we view this as evidence which helps rationalize the ranking of allocative efficiency gains across the two emission markets.

Unfortunately, this test does not explain fully why there are no gains to be found under NBP for its manufacturing plants. In the next section, we explore differences in allocative efficiency gains within market participants in each program.

6.2 Within market

We now turn to heterogeneity analysis within each NO_x emission market to explore potential channels that can either dampen or improve allocative efficiency gains. Within each market, we focus on heterogeneity analyses centered on institutional features and facility characteristics.

First, heterogeneity results point to the importance of plant and firm-level flexibility of pollution abatement options in order to achieve greater allocative efficiency gains from pollution markets. Column (1) of Table 8 interacts the treatment variable with a dummy variable equal to one if it is operated by a multi plant firm.[27] The uninteracted coefficient therefore represents the allocative efficiency gains from RECLAIM for single plant firms. 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). The interacted coefficient suggests that there are imprecisely estimated small allocative efficiency gains for multi-plant firms relative to single plant firms from the market. Unfortunately, since NBP regulated manufacturing plants are on average larger than RECLAIM regulated facilities, there are no single plant firms in the NBP treated sample, and therefore the same exercise cannot be replicated.

[27] The presence of multi-plant firms operating facilities both inside and outside of RECLAIM presents a potential SUTVA violation through reallocation of production or emissions to control plants. Table A3 replicates our main estimate and column (1) of Table 8 without these firms. The overall allocative efficiency effect is statistically indistinguishable from our main estimate in column (4) of Table 2. Table A3 also confirms the qualitative result of larger efficiency gains for multi-facility firms in the sample where multi-facility firms that operate plants both inside and outside of RECLAIM are dropped.

Table 8: Within market heterogeneity estimates for RECLAIM

	$\widehat{\nu}_{it}^2$	$\widehat{\nu}_{it}^2$
	(1)	(2)
RECLAIM X Post	-0.187^{**}	-0.207^{**}
	(0.087)	(0.097)
RECLAIM X Post X Multi-plant firm	-0.044	
	(0.080)	
RECLAIM X Post X Inland		-0.040
		(0.081)
Observations	11,500	11,500

Notes: Estimates of the effect of RECLAIM on the dispersion of distortions. Column (1) interacts the treatment variable with a dummy equal to one if the treated firm is a multi-plant firm. Column (2) interacts the treatment variable with a dummy equal to one if the treated plant is located in the Inland counties without permit trading restrictions. All models include plant and year fixed effects. Robust standard errors clustered at the zip code level in parentheses.

Second, institutional features can play a role in both temporal and spatial dimensions. Theoretical papers highlighting the importance of policy commitment by regulators to incentivize efficient abatement behavior by firms (Requate, 2005). For RECLAIM, this result is empirically exemplified by the increasing efficiency gains from the policy over time shown in the previous section in Figure 6. Column (2) of Table 9 interacts the treatment variable with a dummy equal to one if the plant is coverd by the NO_x emission market under the US EPA Clean Air Interstate Rule (CAIR) which superseded the NBP market after 2008. Slightly more than half of the NBP covered manufacturing plants ended up being also covered by the CAIR market. In the case of the NBP, greater efficiency gains are seen for plants that were covered by the CAIR NOx markets compared to manufacturing plants only covered by the NBP. However, this is a noisily estimated coefficient. Taken together, the gains over time in RECLAIM, and the positive coefficient for CAIR covered NBP facilities provides tentative evidence of the importance of the regulator's time commitment to the environmental market in delivering allocative efficiency gains.

Table 9: Within market heterogeneity estimates for NBP

	$\widehat{\nu}^2_{it}$	$\widehat{\nu}^2_{it}$
	(1)	(2)
NBP X Post	0.124	0.160
	(0.143)	(0.238)
NBP X Post X Updating allocation	−0.001	
	(0.261)	
NBP X Post X CAIR		−0.054
		(0.242)
Observations	32,500	32,500

Notes: Estimates of the effect of RECLAIM on the dispersion of distortions. Column (1) [NEED TO ADJUST THIS]. Column (2) interacts the treatment variable with a dummy equal to one if the treated plant is covered by the Clean Air Interstate Rule (CAIR) NO_x market after 2008 which superseded the NBP. All models include plant and year fixed effects. Robust standard errors clustered at the county code level in parentheses.

More tentative evidence on institutional details also points to restrictions in trading as hindering efficiency gains. Indeed, under RECLAIM two trading zone partially restricted the trading of permits across zones. Column (2) in Table 8 interacts the treatment variable with a dummy equal to one if the regulated facility is located in the unrestricted trading zone. Plants in the unrestricted trading zone saw greater allocative efficiency gains. The importance of transaction costs for the cost-effectiveness of environmental markets has been highlighted theoretically by Stavins (1995) .

Previous work as also shown that fixed versus updating allocation of permits for electricity producers covered by the NBP affected production decisions (Lange and Maniloff, 2021). Column (1) of Table 9 interacts the treatment variable with a dummy equal to one if a manufacturing plant is location in a NBP state that adopted updating allocation of permits as opposed to a fixed allocation scheme. The coefficient noisily rejects any differences in allocative efficiency gains for manufacturing plants covered by the NBP in state with updating or fixed allocation schemes. While this result is at odds with (Lange and Maniloff, 2021), the authors do not look at manufacturers, and do not directly look at allocative efficiency but production.

The above heterogeneity analysis provides some support to the importance of ex-ante heterogeneity in MAC across regulated facilities, plant and firm abatement flexibility and regulatory commitment in enabling environmental markets to deliver cost-savings. Unfortunatley given relative small number of treated manufacturing plants across both markets and disclosure requirements of the U.S. Census Bureau, we are limited in the number and types of heterogeneity analysis we can conduct. Qualitatively, other differences across markets could help explains allocative efficiency differences. We speculate three possible explanations. First, unlike RECLAIM which replaced prescriptive regulations, the NBP was overlaid onto prescriptive regulations that continued after the market's introduction (Fowlie, Holland and Mansur, 2012). 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. Third, the asymmetric deregulation of the electricity output market around the introduction of the NBP affected the decision of electricity producers. This matters because the electricity sector accounts about 90% of market participants and emissions in the NBP, and hence distortions in electricity producers abatement decisions will in turn affect abatement decisions of manufacturing participants. Fowlie (2010) finds that abatement investment decisions by NBP electricity plants differed from the first-best depending on the electricity market regulation they faced. Mansur (2008) furthermore finds evidence for market power enforced by electricity producers in the deregulated markets. Fizsbein et al. (2020) find empirical evidence that distortionary decisions in output markets can spillover to input markets. Therefore, both the documented asymmetric economic regulation and market power by electricity producers covered by the NBP likely affected the ability of manufacturers covered by the NBP to make allocatively efficient decisions. In the case of RECLAIM, the majority of participants are manufacturers instead of electricity producers.

7 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 target 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.

By contrast, we do not find evidence of allocative efficiency gains for manufacturing plants covered by the NBP. We rationalize this result through the observation that across a wide range of underlying characteristics of MAC of regulated plants, baseline heterogeneity of MAC is systematically greater for the set of manufacturing plants covered by RECLAIM than under the NBP. Furthermore, heterogeneity analyses also suggest the plant and firm-level flexibility in pollution abatement options, and the regulator's time commitment to the policy matter for the efficiency gains of pollution markets. 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

It is obvious from eq. (5) that if there is allocative efficiency with $\phi_{is} = 1\ \forall i$ then $\theta_s = 1$. To establish that $\theta_s = 1$ implies efficiency, we rewrite θ_s as

$$\begin{aligned}\theta_s &= NE[a_{is}\phi_{is}] \\ &= NE[a_{is}]E[\phi_{is}] + Ncov(a_{is}(\phi_{is}), \phi_{is}) \\ &= e^{\sigma_s^2/2} + Ncov(a_{is}(\phi_{is}), \phi_{is})\end{aligned}$$

where the second line applies the covariance definition and the third line uses $\sum_i a_{is} = 1$ and $E[\phi_{is}] = e^{\sigma_s^2/2}$ when $\phi_{is} \sim \mathcal{LN}(0, \sigma_s^2)$ from Assumption 1. Observe that because $e^{\sigma_s^2/2} \geq 1$, $\theta_s = 1$ or $\frac{1-e^{\sigma_s^2/2}}{N} = cov(a_{is}(\phi_{is}), \phi_{is})$ is satisfied under two cases: (i) when $cov(a_{is}(\phi_{is}), \phi_{is}) < 0$ and $e^{\sigma_s^2/2} > 1$ and (ii) when $cov(a_{is}(\phi_{is}), \phi_{is}) = 0$ and $e^{\sigma_s^2/2} = 1$. Note that

$$\begin{aligned}cov(a_{is}(\phi_{is}), \phi_{is}) =& E[\phi_{is}a_{is}(\phi_{is})] - E[\phi_{is}]E[a_{is}(\phi_{is})] \\ =& E[\phi_{is}a_{is}(\phi_{is}) - e^{\sigma_s^2} * a_{is}(\phi_{is})] \\ =& E[(\phi_{is} - e^{\sigma_s^2})a_{is}(\phi_{is})] \\ =& E[(\phi_{is} - e^{\sigma_s^2})(a_{is}(\phi_{is}) - a_{is}(e^{\sigma_s^2})]\end{aligned}$$

where the final line follows from $E[(\phi_{is} - 1)a_{is}(\phi_{is})c]$=0 for any constant c, in this case $e^{\sigma_s^2}$. By Assumption 2a, a increasing in ϕ implies $(\phi_{is} - e^{\sigma_s^2})(a_{is}(\phi_{is}) - a_{is}(e^{\sigma_s^2})) \geq 0$ and thus $cov(a_{is}(\phi_{is}), \phi_{is}) \geq 0$. This rules out case (i). Next observe that $(\phi_{is} - e^{\sigma_s^2})(a_{is}(\phi_{is}) - a_{is}(e^{\sigma_s^2})) = 0$ only when $\phi_{is} = 1, \forall i$, in which case $\sigma_s^2 = 0$ and $e^{\sigma_s^2/2}$=1. Thus, we have case (ii) in which $cov(a_{is}(\phi_{is}), \phi_{is}) = 0$ and $e^{\sigma_s^2/2} = 1$, which implies $\phi_{is} = 1$ $\forall i$. That is, under Assumptions 1 and 2a, $\theta_s = 1$ implies allocative efficiency, or $\phi_{is} = 1\ \forall i$, establishing Proposition 1a.

To show that θ_s is increasing in the dispersion of distortions, σ_s^2, we must establish that

$$\frac{d\theta_s}{d\sigma_s^2} = \frac{de^{\sigma_s^2/2}}{d\sigma_s^2} + N\frac{dcov(a_{is}(\phi_{is}), \phi_{is})}{d\sigma_s^2} > 0$$

where the first right hand side term is positive. Turning to the second right hand side term and dropping subscripts, it is sufficient to establish $\frac{dcov(a(\phi),\phi)}{dE[\phi]} > 0$. Observe that we can define $h(\phi) = m + u$ where $h()$ is increasing u is functionally independent of m, or $\phi = h^{-1}(m + u)$, allowing us to write

$$\frac{dcov(a(\phi), \phi)}{dE[\phi]} = \frac{dcov(a(\phi), \phi)}{dm}\frac{dm}{dE[\phi]}$$

where because $h()$ is increasing u is functionally independent of m, $E[\phi]$ is increasing in m and vice-versa. Thus establishing $\frac{dcov(a(\phi),\phi)}{dE[\phi]} > 0$ requires $\frac{dcov(a(\phi),\phi)}{dm} > 0$. Applying $cov(a(\phi), \phi) = E[\phi a(\phi)] - E[\phi]E[a(\phi)]$

and taking a derivative, we have

$$\begin{aligned}\frac{dcov(a(\phi),\phi)}{dm} &= E\left[\frac{a(\phi)+\phi a'(\phi)}{h'(\phi)}\right] - E[a(\phi)]E\left[\frac{1}{h'(\phi)}\right] - E[\phi]E\left[\frac{a'(\phi)}{h'(\phi)}\right] \\ &= cov\left(a(\phi), \frac{1}{h'(\phi)}\right) + E[a(\phi)]E\left[\frac{1}{h'(\phi)}\right] + cov\left(\phi, \frac{a'(\phi)}{h'(\phi)}\right) \\ &+ E[\phi]E\left[\frac{a'(\phi)}{h'(\phi)}\right] - E[a(\phi)]E\left[\frac{1}{h'(\phi)}\right] - E[\phi]E\left[\frac{a'(\phi)}{h'(\phi)}\right] \\ &= cov\left(a(\phi), \frac{1}{h'(\phi)}\right) + cov\left(\phi, \frac{a'(\phi)}{h'(\phi)}\right) \\ &= cov(a(\phi),\phi) + cov(\phi, a'(\phi)\phi)\end{aligned}$$

where ones applies the Leibniz integral rule and the inverse function theorem after the first equality and the covariance definition after the second equality. The last equality follows under Assumption 1, whereby a lognormal distribution implies $h() = ln()$ such that $h'(\phi) = 1/\phi$. By Assumption 2a, a increasing in ϕ implies $cov(a(\phi),\phi) \geq 0$. By Assumption 2b, $a'(\phi)\phi$ increasing in ϕ implies $cov(\phi, a'(\phi)\phi) \geq 0$. This implies $\frac{d\theta_s}{d\sigma_s^2} > 0$, establishing Proposition 1b.

A.2 Proposition 2

We expand $\frac{\theta_m}{\theta_b}$ from eq. (6)

$$\begin{aligned}\frac{\theta_m}{\theta_b} =& \frac{N(E[a_{im}]E[\phi_{im}] + cov(\phi_{im}, a_{im}))}{N(E[a_{ib}]E[\phi_{ib}] + cov(\phi_{ib}, a_{ib}))} \\ =& \frac{E[\phi_{im}]}{E[\phi_{ib}]}\left[1 + N\left(\frac{cov(\phi_{im}, a_{im})}{E[\phi_{im}]} - \frac{cov(\phi_{ib}, a_{ib})}{E[\phi_{ib}]}\right)\right] + \mathcal{O}^2 \\ \approx& \frac{E[\phi_{im}]}{E[\phi_{ib}]}\left[1 + N\left(\underbrace{\frac{cov(\phi_{im}, a_{im})}{E[\phi_{im}]}}_{Z_m} - \underbrace{\frac{cov(\phi_{ib}, a_{ib})}{E[\phi_{ib}]}}_{Z_b}\right)\right]\end{aligned} \tag{A.1}$$

where the first line applies the definition of a covariance; second line applies a Taylor expansion around $E[\phi_{im}]$ and $E[\phi_{ib}]$ and uses $\sum_i a_{is} = 1$; and the third line retains first order term of the Taylor series as an approximation.

To establish Proposition 2, we must demonstrate $dZ_s/dE[\phi_{is}] > 0$. When $\frac{E[\phi_{im}]}{E[\phi_{ib}]} < 1$ or $E[\phi_{im}] - E[\phi_{ib}] < 0$, having $Z_m - Z_b < 0$ implies $\frac{\theta_m}{\theta_b} < \frac{E[\phi_{im}]}{E[\phi_{ib}]}$ and thus $\frac{E[\phi_{im}]}{E[\phi_{ib}]}$ is a lower bound on the true allocative efficiency gain $\frac{\theta_m}{\theta_b}$. Conversely, when $\frac{E[\phi_{im}]}{E[\phi_{ib}]} > 1$ or $E[\phi_{im}] - E[\phi_{ib}] > 0$, having $Z_m - Z_b > 0$ implies $\frac{\theta_m}{\theta_b} > \frac{E[\phi_{im}]}{E[\phi_{ib}]}$ and so $\frac{E[\phi_{im}]}{E[\phi_{ib}]}$ is a lower bound on the true allocative efficiency loss $\frac{\theta_m}{\theta_b}$.

First, observe that we can define $h(\phi) = m + u$ where $h()$ is increasing u is functionally independent of m, or $\phi = h^{-1}(m+u)$, allowing us to write

$$\frac{dZ}{dE[\phi]} = \frac{dZ}{dm}\frac{dm}{dE[\phi]}$$

where because $h()$ is increasing u is functionally independent of m, $E[\phi]$ is increasing in m and vice-versa. Thus establishing $\frac{dZ}{dE[\phi]} > 0$ requires $\frac{dZ}{dm} > 0$. Rearranging Z_s into $Z_s E[\phi_{is}] = cov(\phi_{is}, a_{is}(\phi_i s)) = E[\phi_{is} a_{is}(\phi_{is})] - E[\phi_{is}]E[a_{is}(\phi_{is})]$, dropping subscripts, and taking a derivative, we have

$$\begin{aligned}
\frac{dZ}{dm}E[\phi] + ZE\left[\frac{1}{h'(\phi)}\right] &= E\left[\frac{a(\phi)+\phi a'(\phi)}{h'(\phi)}\right] - E[a(\phi)]E\left[\frac{1}{h'(\phi)}\right] - E[\phi]E\left[\frac{a'(\phi)}{h'(\phi)}\right] \\
&= cov\left(a(\phi), \frac{1}{h'(\phi)}\right) + E[a(\phi)]E\left[\frac{1}{h'(\phi)}\right] + cov\left(\phi, \frac{a'(\phi)}{h'(\phi)}\right) \\
&+ E[\phi]E\left[\frac{a'(\phi)}{h'(\phi)}\right] - E[a(\phi)]E\left[\frac{1}{h'(\phi)}\right] - E[\phi]E\left[\frac{a'(\phi)}{h'(\phi)}\right] \\
&= cov\left(a(\phi), \frac{1}{h'(\phi)}\right) + cov\left(\phi, \frac{a'(\phi)}{h'(\phi)}\right) \\
\Rightarrow \frac{dZ}{dm} &= \frac{1}{E[\phi]}\left(cov\left(a(\phi), \frac{1}{h'(\phi)}\right) + cov\left(\phi, \frac{a'(\phi)}{h'(\phi)}\right) - \frac{cov\,(\phi, a(\phi))}{E[\phi]}E\left[\frac{1}{h'(\phi)}\right]\right)
\end{aligned}$$

where ones applies the Leibniz integral rule and the inverse function theorem after the first equality and the covariance definition after the second equality. The last line follows by rearranging terms and dividing by $E[\phi]$. Under Assumption 1, a lognormal distribution implies $h() = ln()$ such that $h'(\phi) = 1/\phi$. This implies

$$\frac{dZ}{dm} = \frac{cov(\phi, a'(\phi)\phi)}{E[\phi]}$$

Under Assumption 1, $E[\phi] > 0$, thus the sign of $\frac{dZ}{dm}$ has the sign of $cov(\phi, a'(\phi)\phi)$, which is positive if $a'(\phi)\phi$ is increasing in ϕ, or when $\frac{-\phi a''(\phi)}{a'(\phi)} < 1$ as required by Assumption 2b. This establishes Proposition 2. Observe that in a consumption setting, the condition $\frac{-\phi a''(\phi)}{a'(\phi)} < 1$ occurs with a constant relative risk aversion utility function with weakly positive utility values, which mirrors our setting in which abatement shares must also be weakly positive.

B Data appendix

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

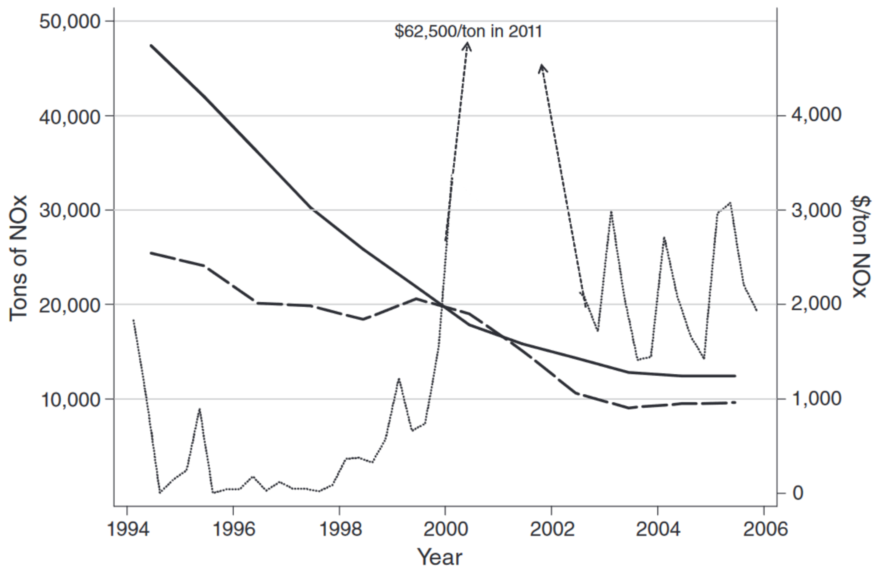
unique ID data 1	Year	Plant name	NAICS (3-digit)
plant_1	2000	GOLETA REFINERY	324
plant_1	2001	GOLETA REFINERY	
plant_1	2002	COASTAL PETROLEUM	324

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

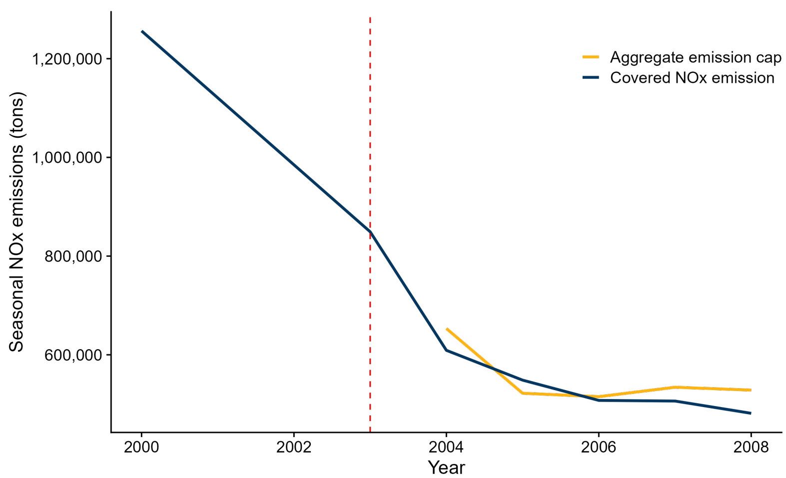
C Figure appendix

Figure A1: NO_x emissions, cap, and price trends in RECLAIM



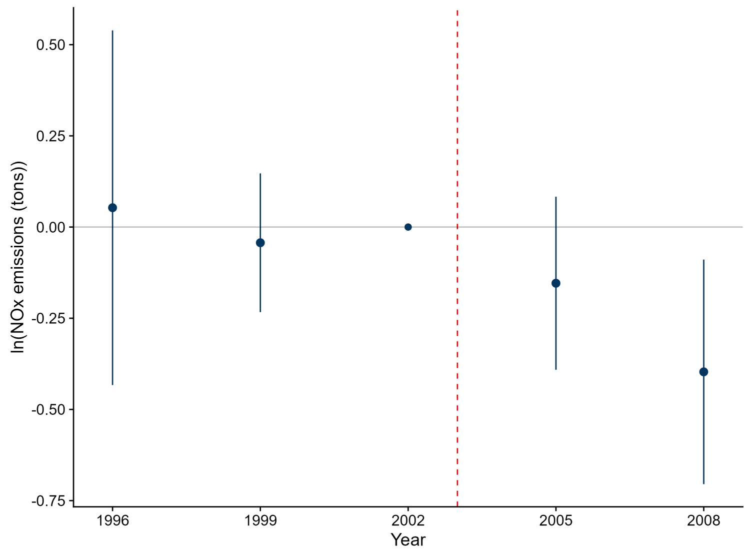
Notes: NO_x emissions are dashed, aggregate allocation of permits or the cap is solid, and the NO_x permit price are dotted. Reproduced from Fowlie, Holland and Mansur (2012). There is a typographic error in the original Fowlie, Holland and Mansur (2012) figure. It should read ”in 2001”, and not ”in 2011”.

Figure A2: NBP NO_x emissions and cap



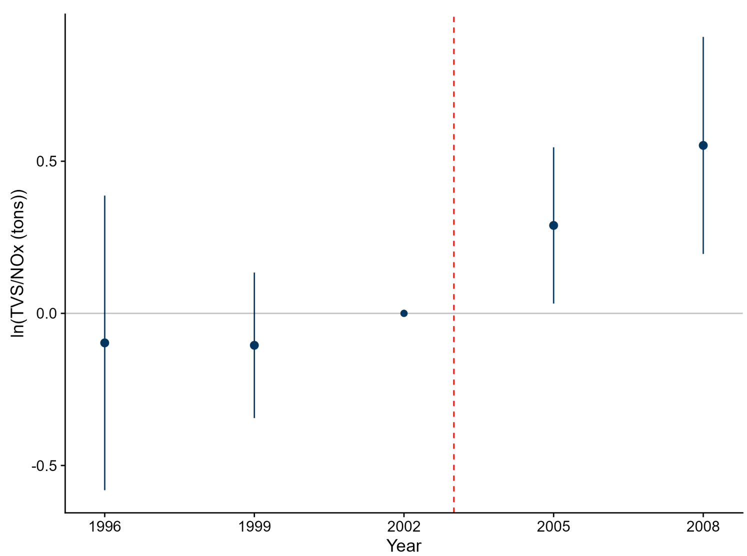
Notes: Seasonal NBP NO_x emission trends, and aggregate emission allowance budgets. The year 2003 cap is omitted from the graph since not all states had joined the NBP yet (U.S. Environmental Protection Agency, 2009)

Figure A3: Event-study model of the effect of NBP on NO_x emissions on trimmed sample



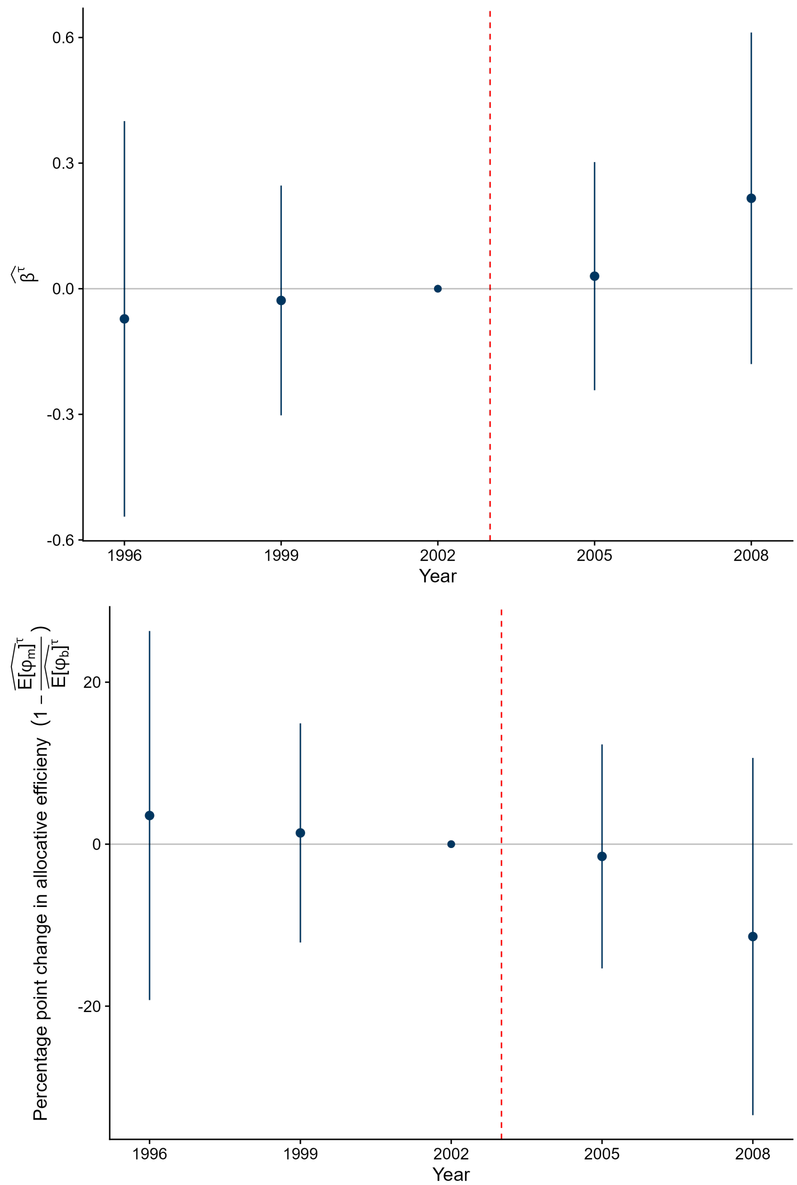
Notes: Point estimates and 95% confidence intervals of the yearly effect of NBP on log NO_x emissions relative to 2002, the year before the NBP was introduced, using eq. (9). Standard errors are clustered at the county level.

Figure A4: Event-study model of the effect of NBP on NO_x shadow price on trimmed sample



Notes: Point estimates and 95% confidence intervals of the yearly effect of NBP on log revenue per emissions relative to 2002, or $\widehat{\alpha}^{\tau}$ using eq. (9). Standard errors are clustered at the county level

Figure A5: Annual effects of NBP on allocative efficiency on trimmed sample



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 $\frac{\widehat{E[\phi_m]}^{\tau}}{\widehat{E[\phi_b]}^{\tau}} = e^{\frac{\widehat{\beta}^{\tau}}{2}}$. Standard errors are clustered at the county code level.

D Table appendix

Table A1: Trend-break model of RECLAIM

	$\ln \text{NO}_x$ emissions	$\ln AR_{it}$	$\widehat{\nu}_{it}^2$
	(1)	(2)	(3)
RECLAIM X Post	0.022	-0.070	-0.168
	(0.060)	(0.074)	(0.151)
RECLAIM X Trend	0.032^{**}	-0.034^{**}	0.033^{*}
	(0.014)	(0.014)	(0.017)
RECLAIM X Post X Trend	-0.106^{***}	0.133^{***}	-0.080^{*}
	(0.026)	(0.028)	(0.044)
RECLAIM effect when t = 2005	-0.420^{***}	0.522^{***}	-0.445^{***}
	(0.110)	(0.117)	(0.136)
$1 - \frac{\widehat{E[\phi_m]}}{\widehat{E[\phi_b]}}$			0.199
			[0.072, 0.31]
Observations	11,500	11,500	11,500

Notes: Estimates of differential pre-trend (RECLAIM X Trend), DiD effect (RECLAIM X Post), and differential post-trend break (RECLAIM X Post X Trend) from eq. XYZ for log NO_x, log average revenue per emissions, and predicted residuals from eq. 9 across columns. Average effect for 2005 shown. The lower bound on allocative efficiency change for 2005 is $1 - \frac{\widehat{E[\phi_m]}}{\widehat{E[\phi_b]}} = 1 - e^{\frac{\widehat{\beta}}{2}}$ All models include facility-specific and year-specific dummy variables. Standard errors clustered at the county-level in parentheses, and 95% confidence interval in brackets.

Table A2: Average treatment effect of RECLAIM on trimmed sample

	ln NO_x emissions	ln AR_{it}	$\widehat{\nu}^2_{it}$
	(1)	(2)	(3)
RECLAIM X Post	-0.146^{**}	0.166^{**}	-0.121^{*}
	(0.059)	(0.070)	(0.066)
$1 - \frac{\widehat{E[\phi_m]}}{\widehat{E[\phi_b]}}$			0.059
			[-0.004, 0.118]
Observations	11,500	11,500	11,500

Notes: Estimates of the average treatment effect of RECLAIM using a difference-in-difference model on the trimmed sample. Facilities for which changes in emissions or revenue exceed 100 times the 99th percentile changes or less than 100 times smaller than the 1th percentile of changes are dropped. 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 $1 - \frac{\widehat{E[\phi_m]}}{\widehat{E[\phi_b]}} = 1 - e^{\frac{\widehat{\beta}}{2}}$. Robust standard errors clustered at the zip code in parentheses, and 95% confidence interval in brackets.

Table A3: Average treatment effect of RECLAIM by type of firm

	$\widehat{\nu}^2_{it}$	$\widehat{\nu}^2_{it}$	$\widehat{\nu}^2_{it}$
	(1)	(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.