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Evaluating the efficiency and effectiveness of environmental policies for global and local air pollutants

A dissertation submitted in partial satisfaction
of the requirements for the degree

Doctor of Philosophy
in
Environmental Science and Management

by

Vincent Thivierge

Committee in charge:

Professor Kyle C. Meng, Co-Chair
Professor Olivier Deschênes, Co-Chair
Professor Christopher Costello

June 2023

The Dissertation of Vincent Thivierge is approved.

Professor Christopher Costello

Professor Olivier Deschênes, Committee Co-Chair

Professor Kyle C. Meng, Committee Co-Chair

June 2023

Evaluating the efficiency and effectiveness of environmental policies for global and local air pollutants

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by

Vincent Thivierge

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Curriculum Vitæ
Vincent Thivierge

Education

2023	Ph.D. in Environmental Science and Management, University of California, Santa Barbara.
2013	M.A. in Economics, University of British Columbia.
2012	B.B.A. in Applied Economics, HEC Montréal.

Publications

"Equitable low-carbon transition pathways for California's oil extraction." with Deshmukh, Weber, Deschenes, Lea, Hernandez-Cortes, Kordell, Lee, Malloy, Mangin, Meng, Sum, Uppal, and Meng. (2023). ***Nature Energy***, 1-13.

"Carbon Pricing and Competitiveness Pressures: The Case of Cement Trade." (2020). in ***Canadian Public Policy*** 46(1) : 45-58.

"Refining the Evidence: British Columbia's Carbon Tax and Household Gasoline Consumption." with Chad Lawley. (2018). ***The Energy Journal*** 39(2) : 147-171.

Working papers

"Do Environmental Markets Improve Allocative Efficiency? Evidence from U.S. Air Pollution." with Kyle C. Meng. (2023).

"Do Carbon Tariffs Reduce Carbon Leakage? Evidence from Trade Tariffs." (2023).

Presentations

2023 : Université de Montréal, Université d'Ottawa, Georgia Tech, RFF, 20th Occasional Workshop in Environmental and Resource Economics

2022 : AERE 2022 Summer Conference

2021 : 9th Mannheim Conference on Energy and the Environment, AERE 2021 Summer Conference, 4th Annual LSE Workshop on Environmental Economics, 1st Federal Reserve Bank of San Francisco Climate Risk Workshop.

2017 : Canadian Economics Association (Antigonish).

2016 : Financial Accountability Office of Ontario, Ontario Ministry of Finance.

2015 : Université Paris-Dauphine, AERE 2015 Summer Conference (San Diego), Université d'Ottawa.

Grants, Honors, and Awards

Dissertation Fellow, UC Institute on Global Conflict and Cooperation (2022-2023)
National Nominee, SSHRC, Lindau Nobel Meeting (2022)

Doctoral Fellowship, SSHRC (2018-2022)
Regents Fellowship, UCSB (2017-2018)
Bren Fellowship, Bren School UCSB (2019-2021)
Academic Excellence Scholarship, HEC Montréal (2010)
Leadership Scholarship, HEC Montréal (2009)

Research employment

2019 – 2023	Graduate Student Researcher, emLab, UCSB
2017	Researcher, C.D. Howe Institute
2014 – 2016	Research Associate, Canada's Ecofiscal Commission
2014 – 2016	Research Associate, Smart Prosperity Institute, University of Ottawa

Teaching assistant

Graduate courses

Environmental Policy Evaluation (UCSB, 2022, 2023)
Economics of Environmental Management (UCSB, 2021)

Undergraduate courses

Introduction to Environmental Science (UCSB, 2021)
Principles of Economics (UCSB, 2019)
Intermediate Microeconomics (UCSB, 2019)
Principles of Macroeconomics (UBC, 2013)
Principles of Microeconomics (UBC, 2012)

Service

Refereeing: *Canadian Public Policy*, *Energy Economics*, *Resource and Energy Economics*, *The Energy Journal*

Other: Bren Ph.D. Student Anti-Racism Caucus (2021-2023), Bren School Ph.D. Program Committee (2020-2021), Bren School Seminar Committee (2019-2020), UCSB Economics Department Diversity Committee (2020)

Other information

Professional designations: Special Sworn Status, U.S. Census Bureau (2021-); Deemed Employee Status, Statistics Canada (2014-)

Languages: French (native), English (fluent)

Citizenship: Canadian

Software: R, Stata, LaTeX, Matlab

Permissions and Attributions

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Abstract

Evaluating the efficiency and effectiveness of environmental policies for global and local air pollutants

by

Vincent Thivierge

Unregulated global and local air pollutants impose high costs on society. For more than half a century, economists have argued that the introduction of market instruments such as pollution taxes or cap-and-trade markets can cut aggregate emissions at the lowest cost. Market instruments have been implemented by some countries to reduce local pollutants, such as nitrogen oxides, and global pollutants, such as greenhouse gas (GHG) emissions. One problem with this context is the lack of evidence on the cost savings of market-based policies relative to other policies. Particularly for global pollutants, a second problem with the patchwork of policies is carbon leakage, where emission reductions from regulated countries are offset by emission increases in unregulated countries. This dissertation seeks to explore these two problems. The first chapter based on joint work with Kyle C. Meng, *Do environmental markets improve allocative efficiency? Evidence from U.S. air pollution*, develops a framework to test the allocative efficiency changes of introducing cap-and-trade markets. The framework is applied to landmark U.S. air pollution markets using manufacturing data. The chapter finds evidence of allocative efficiency gains for some markets. The second chapter, *Carbon pricing and competitiveness pressures: The case of cement trade*, provides empirical evidence of decreased net exports of a carbon-intensive product, cement, in British Columbia, Canada following the introduction of their carbon tax. The third chapter, *Do carbon tariffs reduce carbon leakage? Evidence from trade tariffs*, combines theory and data to study the effects of proposed carbon tariffs that price the carbon content of imports on foreign GHG emission changes. The chapter finds evidence of reduced GHG emissions from targeted industries and an unintended emission offset effect

from downstream industries. Together, these chapters provide evidence on the efficiency and effectiveness of policies promoted to mitigate harmful air pollutants.

Contents

Chapter 1

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

1.1 Introduction

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

[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).

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

Our framework starts with the observation that allocative efficiency for any input occurs when its marginal product is equalized across producers. Distortions drive wedges between producers' marginal products, leading to misallocation. This insight is widely used in the misallocation literature in which dispersion of appropriately-weighted input prices quantifies the aggregate productivity consequences of capital or labor misallocation (Restuccia and Rogerson, 2008; Hsieh and Klenow, 2009; Restuccia and Rogerson, 2013). Such an approach, however, is not directly applicable when input prices are missing (or merely "shadow"), as in the case of pollution.

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

Our framework has three additional advantages. First, our theory accommodates policies with any arbitrary allocation of inputs, regardless of institutional context. This flexibility allows us to study a wide-range of settings in which the pre-market policy can take on any form and does not pre-specify that a market-based policy necessarily achieves allocative efficiency. Crucially, this means our main statistical test is two-sided: a market-based policy can either decrease or increase allocative inefficiency, as allowed by second-best theory. Second, we allow policies to have different total levels of an input, accommodating the fact that in practice, many market-based environmental policies stipulate a drop in total pollution (i.e., the "cap" in cap-and-trade) in addition to reallocation in pollution. Third, our framework uses a quasi-experimental approach to account for several common concerns in the misallocation literature, including 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 Regional Clean Air Incentives Market (RECLAIM) and the eastern U.S. NO_x Budget Program (NBP). These markets are notable both because of their scale, covering nearly all major polluting facilities within their jurisdiction, and for their reputation as precedent-setting pollution markets. The average emissions effects of these programs have also been extensively studied (Fowlie, Holland and Mansur, 2012; Deschenes, Greenstone and Shapiro, 2017), allowing us to build on established research designs to examine changes in allocative efficiency. Within each program, we focus on manufacturing facilities, in part because our framework may not apply to vertically-integrated electric utilities. For both programs, we build a new linking algorithm to merge facility-by-year NO_x emissions data from state and/or federal environmental agencies with restricted-use revenue data from the U.S. Census of Manufacturer (CM) and the Annual Survey of Manufacturing (ASM).

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

We contribute to a rich literature quantifying the total abatement cost of market-based environmental policies. In theory, a polluter's marginal abatement cost is the difference in optimized profit between no abatement and the specified abatement level. In practice, much of the empirical literature has relied on the cost minimizing dual of this problem whereby a particular cost function is assumed and then estimated in a cross-section of polluters.[2] As with any cost function estimation, these studies must argue that all relevant inputs and their prices are observed and vary exogenously. For the estimated cost function to be valid for counterfactual policies, this approach must also assume that polluters do not alter output in the counterfactual, restricting a potentially important abatement option. Additionally, prior approaches often assume that a market-based policy necessarily leads to allocative efficiency gains, leaving researchers with determining by just how much.[3] Our approach starts with

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

[3]In ex-ante studies, a cost minimizing algorithm is often assumed to characterize the counterfactual market-

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

Finally, we contribute to a recent misallocation literature in macroeconomics and development economics. Input misallocation within an economy has been shown to be a strong determinant of aggregate productivity differences across economies (i.e., the indirect approach) (Restuccia and Rogerson, 2008; Hsieh and Klenow, 2009; Restuccia and Rogerson, 2013). More recently, researchers have turned to 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, 2022; Sraer and Thesmar, 2023). As with Bau and Matray (2022), we argue that a quasi-experimental estimator can address potential concerns about measurement error (Bils, Klenow and Ruane, 2021) and misspecification

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.

(Haltiwanger, Kulick and Syverson, 2018). However, in contrast to Bau and Matray (2022) 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 observed input prices. Not needing input prices is particularly useful for studying market introductions: by definition, input prices are not observed prior when markets are absent. 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 function, we do not analyze the specific abatement technologies adopted following a market-based policy, which may shed light on the abatement decisions that alter misallocation costs (Linn, 2008; Fowlie, 2010; Chan et al., 2018). Finally, we rely on parametric assumptions in our theory to facilitate a mapping between our quasi-experimental estimator and the change in allocative efficiency. While these assumptions are employed elsewhere in the misallocation literature, they are also untestable. Our alternative semi-parametric approach trades off these assumptions with other limitations.

The rest of the paper has the following structure. Section 1.2 provides background on market-based policies in the U.S. Section 1.3 presents our conceptual framework, linking theory with our empirical research design. Section 1.4 discusses our data. Section 1.5 presents our results. Section 1.6 concludes the paper. Appendix A.1, A.2, A.3, and A.4 offer additional theoretical proofs, data, figures, and tables.

1.2 Background

1.2.1 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).[4] Today, cost-effectiveness serves as the central appeal behind the modern environmental market, sometimes called "cap-and-trade". In such programs, a regulator establishes a limit (or cap) on total emissions by issuing a fixed supply of emission permits. Regulated facilities are then either given, or must purchase through auction or trade with other facilities, permits to cover their emissions. Cost-effectiveness has motivated the adoption of environmental markets in nearly every environmental domains: today, pricing policies cover 30% of global fisheries (Costello et al., 2016), account for over $36 billion in global ecosystem service payments (Salzman et al., 2018), govern 20% of global greenhouse gas (GHG) emissions (World Bank, 2021), and underlie many major air pollution policies.

This promise of cost-effectiveness has also been subjected to criticism, both theoretically and empirically. Indeed, a second-best theoretical literature emerged shortly after the cost-effectiveness was established in a first-best setting. This literature considered both existing distortions such as market power in output markets (Malueg, 1990), complementary policies

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

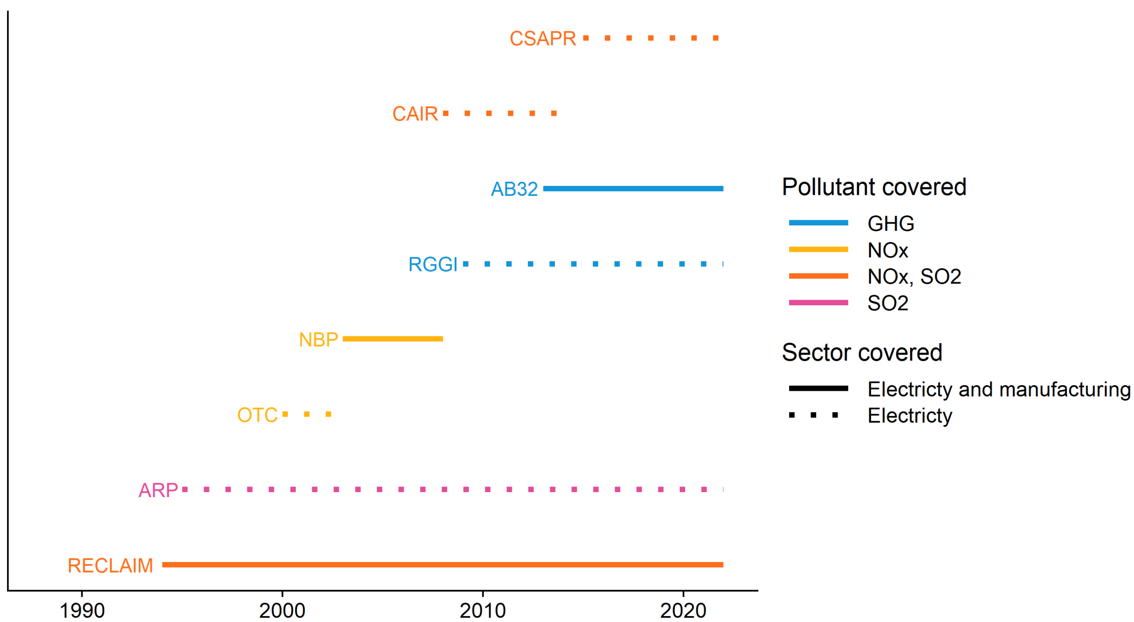
(Bohi and Burtraw, 1992; Fowlie, 2010), and income taxation (Goulder et al., 1999; Fullerton and Metcalf, 2001), and distortions that come with the environmental market itself in the form of market power in the permit market (Hahn, 1984), transaction costs (Stavins, 1995), and non-compliance (Malik,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

The empirical critique of cost-effectiveness is of a more epistemic nature. Many early pioneers of environmental markets had worked on the theory of optimal environmental policy, which at the time was hitting practical limitations: setting optimal policy requires regulators to know, among other things, the marginal abatement cost curves of every polluter, objects that are unobserved. The impracticality of this informational requirement pivoted attention away from optimal policy towards the design of instruments that can achieve environmental and economic objectives with minimum regulatory information. An environmental market satisfies this criteria: in (first-best) theory, an economy-wide environmental objective can be met at minimum cost without the regulator needing to know every polluter's marginal abatement cost curve. But within this lies an inherent tension with empirical validation: if environmental markets are appealing because it does not require a regulator to know marginal abatement cost curves, is it reasonable to assume that researchers can estimate such curves when attempting to establish the allocative efficiency of environmental markets? We return to this point in Section (1.3.1) when discussing prevailing approaches to estimating allocative efficiency changes.

1.2.2 U.S. air pollution markets

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

Figure 1.1: Major air pollution cap-and-trade market programs in the U.S.



Notes: Figure 1.1 show the timeline of major global or local air pollution cap-and-trade markets in the U.S. from 1990 to 2020. The length of the line represents the start to end dates for each markets. The different SO_2 and NO_x markets under CAIR and CSARP are bundled together for visual ease. Colors represent the pollutant covered, and the line type indicate whether the market covered electricity facilities, or electricity and manufacturing facilities. The acrononyms stand for: Cross-State Air Pollution Rule (CSAPR), Clean Air Interstate Rule (CAIR), Assembly Bill 32 (AB32), Regional Greenhouse Gas Initiative (RGGI), NO_x Budget Program (NBP), Ozone Transport Commission (OTC), Acid Rain Program (ARP), and Regional Clean Air Incentives Market (RECLAIM).

Figure 1.1 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 manufactur-

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

ing and/or electricity facilities. We employ two criteria in selecting the markets we study, both necessitated by our framework in Section 1.3. First, because we assume profit-maximizing facilities, we cannot study electricity generators that were part of vertically-integrated utilities. This rules out the SO_2 Acid Rain Program (ARP), which covers only electricity generators and was introduced when the electricity sector was composed largely of vertically-integrated utilities.[6] This requirement also complicates the study of electricity generators in later pollution markets when deregulation of electric utilities may have coincided with the introduction of pollution markets (Cicala, 2022), such as with the Regional Greenhouse Gas Initiative (RGGI). To avoid these complications, we focus on manufacturing facilities that participate in pollution markets. Second, because our framework is static, we omit cap-and-trade programs that allow dynamic banking and borrowing of permits such as California's AB32 greenhouse gas program. These restrictions leave us with two eligible air pollution markets, both for nitrogen oxides (NO_x): southern California's Regional Clean Air Incentives Market (RECLAIM) and the eastern U.S. NO_x Budget Program (NBP).

1.2.3 RECLAIM

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

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

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

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

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

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

1.2.4 NO_x Budget Program

The NO_x Budget Program (NBP) was a NO_x emission cap-and-trade market operated by the U.S. EPA that ran from 2003 to 2008. The NBP covered NO_x emissions of over 700 large emitting facilities across 20 eastern states.[8] The market was implemented to help states comply with ozone standards under the 1990 Clean Air Act Amendments. The U.S. EPA assigned each state a summertime NO_x emission budget for large point sources, and encouraged states to participate in the NBP market to provide compliance flexibly to their regulated sources (Burtraw and Szambelan, 2010). The U.S. EPA allowed states to determine how their allowance budget would be allocated across facilities. About 90% of NBP-regulated facilities were large power plants and about 100 facilities were manufacturing plants. For the manufacturing plants covered, more than 90% of the facilities are included in only four 4-digit North American Industry Classification System (NAICS) industries, namely pulp and paper manufacturing, chemical manufacturing, petroleum refineries, and primary metal manufacturing.

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

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

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

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

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

1.3 Conceptual framework

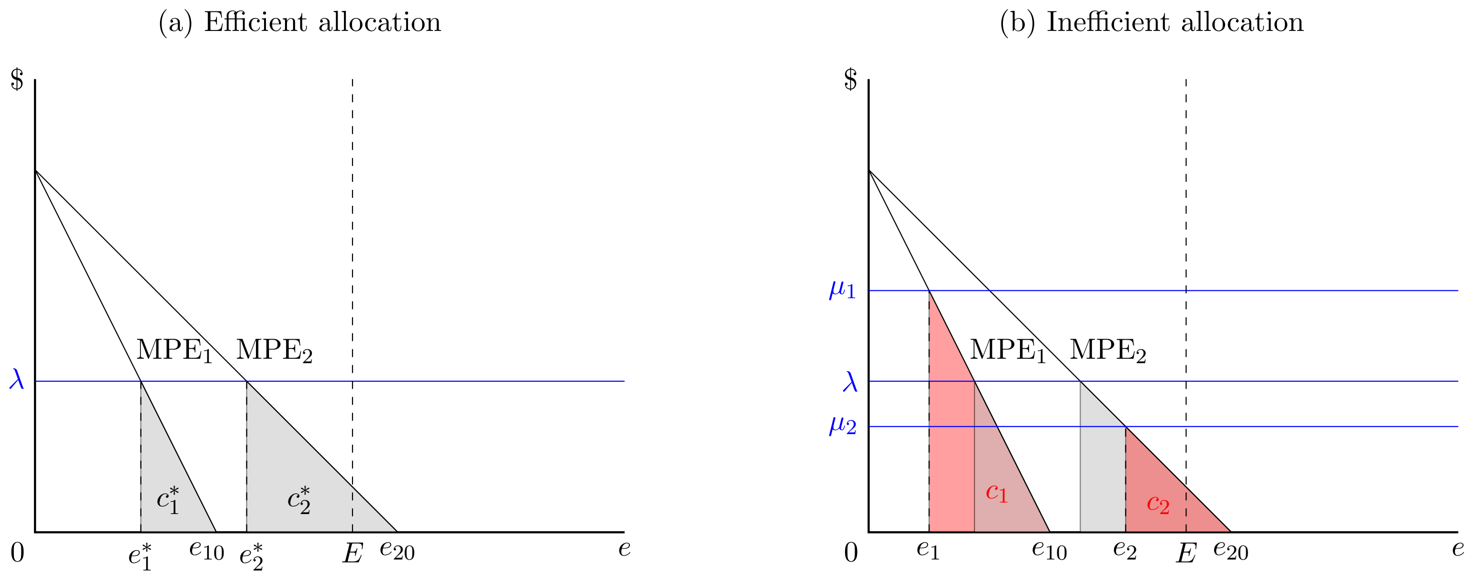
This section details our framework, linking theory and empirics, to estimate the change in allocative inefficiency following the introduction of a market-based policy. Section 1.3.1 begins with a stylized example to illustrate why this is empirically challenging. Section 1.3.2 presents a model of environmental policy and a definition of how allocative inefficiency in emissions changes across two arbitrary policies, which serves as our estimand. Section 1.3.3 introduce additional assumptions that enables this theoretical estimand to be recovered empirically, leading to our quasi-experimental estimator in Section 1.3.4. Section 1.3.5 explores various extensions to our framework that weakens some assumptions (while introducing others).

1.3.1 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 1.2 show emissions on the horizontal axis and its (shadow) price on the vertical axis. Facility 1 has a steeper marginal product of emissions curve than facility 2.[9] For a given allowable total emissions, E, there is a particular allocation of emissions that minimizes total cost, indicated by the sum of the shaded areas across the facilities. As panel (a) indicates, that efficient allocation occurs when the marginal product of emissions is equalized across facilities (i.e., the equimarginal principle is satisfied) at the economy-wide emissions price $\lambda(E)$ such that the more costly Facility 1 engages in less abatement while the less costly Facility 2 has more abatement.

[9]The horizontal axes in Figure 1.2 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 1.2: 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 1.2. 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).[10] Finally, there is an epistemic tension with trying to estimate facility-specific marginal product of emissions curves: if a key appeal of environmental markets over command-and-control policies is that it is unreasonable to expect a regulator to know such curves, how does one reasonably expect

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

researchers to be able to estimate them.

Panel (b) suggests an alternative approach. Rather than explicitly estimate each facility-level marginal product of emissions curve, perhaps something can be learned about allocative efficiency by looking at the dispersion in input prices, μ_i. This idea is leveraged by the misallocation literature, where the dispersion in appropriately-weighted input prices informs the aggregate productivity consequences of input misallocation (Restuccia and Rogerson, 2008; Hsieh and Klenow, 2009; Restuccia and Rogerson, 2013). We draw on this insight, but with one critical caveat: by definition, input prices are missing (or are "shadow") before a market-based policy and consistently missing for facilities in a control group. That is, one needs to adapt methods from the misallocation literature, designed for quantifying misallocation in existing markets, to the study of new markets. Furthermore, in contrast to the stylized example in Figure 1.2, 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.

1.3.2 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

$$\begin{aligned}
\Pi_i^* = \max_{e_i, z_i} \ & \sum_i p(q_i) q_i(e_i, z_i) - w z_i \\
s.t. \ & \sum_i e_i = E_s \\
= \max_{e_i, z_i} \ & \sum_i p(q_i) q_i(e_i, z_i) - w z_i - \lambda_s (\sum_i e_i - E_s) \qquad (1.1)
\end{aligned}$$

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

$$\begin{aligned}
\Delta \Pi_s^* &= (E_o - E_s) \frac{d\Pi_s}{dE_s}_{|E_s} + \mathcal{O}^2 \\
&\approx (E_o - E_s) \lambda_s \qquad (1.2)
\end{aligned}$$

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

$$\begin{aligned}
\pi_{is}(e_{is}) = \max_{e_i, z_i} \ & p(q_i) q_i(e_i, z_i) - w z_i \\
s.t. \ & e_i = e_{is} \\
= \max_{e_i, z_i} \ & p(q_i) q_i(e_i, z_i) - w z_i - \mu_{is}(e_i - e_{is}) \qquad (1.3)
\end{aligned}$$

where $\mu_{is}(e_{is})$ is the Lagrange multiplier on the emissions constraint, henceforth denote as μ_{is}. Observe that eq. 1.3 encompasses a wide range of regulatory environments. For example, under a command-and-control regulation, the regulator may set e_{is} directly. Under a market-based policy, facilities may face an emissions price leading to e_{is}, which may or may not be allocatively efficient.

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

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

$$\begin{aligned} \Delta\Pi_s &= \sum_i \Delta\pi_{is}(e_{is}) \\ &= \sum_i (e_{io} - e_{is}) \frac{d\pi_{is}}{de_{is}}_{|e_{is}} + \mathcal{O}^2 \\ &\approx \sum_i (e_{io} - e_{is}) \lambda_s \phi_{is} \end{aligned} \tag{1.4}$$

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. 1.2 and 1.4, this measure is

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

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

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

$$\theta = \frac{\sum_i a_{im}\phi_{im}}{\sum_i a_{ib}\phi_{ib}} \tag{1.5}$$

When $\theta = 1$, misallocation costs are unchanged. When $\theta < 1$, state m lowers misallocation costs (or improved allocative efficiency) compared with state b. Likewise, when $\theta > 1$, state m increased misallocation costs (or worsened allocative efficiency) compared with state b. $\theta - 1$ denotes the percent change in misallocation costs across policies. We rewrite eq. (1.5) as

$$\begin{aligned}
\theta =& \frac{N\left[(\frac{1}{N}\sum\phi_{im})(\frac{1}{N}\sum a_{im}) + \frac{1}{N}\sum_i(\phi_{im} - \frac{1}{N}\sum\phi_{im})(a_{im} - \frac{1}{N}\sum a_{im})\right]}{N\left[(\frac{1}{N}\sum\phi_{im})(\frac{1}{N}\sum a_{ib}) + \frac{1}{N}\sum_i(\phi_{ib} - \frac{1}{N}\sum\phi_{ib})(a_{ib} - \frac{1}{N}\sum a_{ib})\right]} \\
=& \frac{[\mu_m + N\rho_m]}{[\mu_b + N\rho_b]} \\
=& \frac{\mu_m}{\mu_b}\left[1 + N\left(\frac{\rho_m}{\mu_m} - \frac{\rho_b}{\mu_b}\right)\right] + \mathcal{O}^2 \\
\approx& \widetilde{\theta}\left[1 + N\left(\frac{\rho_m}{\mu_m} - \frac{\rho_b}{\mu_b}\right)\right]
\end{aligned} \tag{1.6}$$

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

1.3.3 From theory to empirics

The change in allocative efficiency, θ in equation (1.6), is constructed from facility-level abatement share a_{is} and distortion ϕ_{is} for each policy state. These elements are not directly observed in data.[11] To facilitate our empirical investigation, we turn to additional assumptions.

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

Appendix A.1.1 provides the proof. Here, we discuss some intuition behind the assumptions underlying Proposition 1. Note that assuming $\phi_{is} \sim \mathcal{LN}(0, \sigma_s^2)$ offers a link between $\widetilde{\theta}$ and the change in the variance of distortion across policies, an observation made elsewhere in the misallocation literature (Hsieh and Klenow, 2009). That is, $\widetilde{\theta} > 1$ when the variance of distortions increase following the policy change while $\widetilde{\theta} < 1$ when the variance of distortions decrease.[12] Second, it is natural to assume that abatement increases with distortions: as the shadow price of pollution increases with higher distortions a facility cuts more pollution. In Section 1.3.5, we consider an extension that relaxes this functional form assumption.

Proposition 1 point to $\widetilde{\theta}$ as our object of interest. But estimating $\widetilde{\theta}$ still requires facility-level distortions across policy states, which are also not directly observed. To over come this, we turn to the first order condition for the firm problem in eq. (1.3), equating the marginal cost of emissions with its marginal revenue

$$\lambda_s \phi_{is} = (1 + \xi_i)\kappa_i \frac{p_i q_{is}}{e_{is}} \tag{1.7}$$

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

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

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

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

where $\kappa_i = \frac{\partial q_i}{\partial e_i}\frac{e_i}{q_i} > 1$ is output elasticity and $\xi_i = \frac{\partial p_i}{\partial q_i}\frac{q_i}{p_i}$ is the inverse price elasticity[13], both of which may be heterogeneous across facilities. On the demand side, a growing literature documents heterogenous markups, and thus demand elasticities, across firms even within narrow sectoral definitions (Nevo, 2001; Hottman, Redding and Weinstein, 2016). 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. For notational simplicity, these firm-specific structural parameters are presented here as constants. But as Section 1.3.4 discusses, our quasi-experimental estimator in practice allows for these parameters to vary as a function of the distortions, addressing potential misspecification concerns. Rewriting eq. 1.7 as average revenue per emissions, $AR_{is} = \frac{p_i q_{is}}{e_{is}}$, yields

$$\ln AR_{is} = \ln(1/(1+\xi_i)) - \ln \kappa_i + \ln \lambda_s + \ln \phi_{is} \tag{1.8}$$

Eq. (1.8) suggests a possible regression specification. However, two additional considerations arise when bringing eq. (1.8) 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[14] It is also possible that macroeconomic conditions jointly alter the dispersion of distortions for treated and control facilities and that facilities differ by baseline distortions, regardless of policy, such that $\phi_{it} \sim \mathcal{LN}(0, \sigma_i^2 + \sigma_{st}^2)$. For estimation, these possibilities necessitate the use for a control group of facilities that are subject to the same macroeconomic changes but not the change in policy in a quasi-experimental framework.

Second, the first order condition in eq. (1.7) may be misspecified. For example, rather than being fixed, firm-specific demand and output elasticities may themselves be functions of

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

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

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.

1.3.4 Empirical specifications

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 (1.8)

$$\ln AR_{it} = \underbrace{\eta_i}_{\ln(\frac{1}{1+\xi_i})-\ln\kappa_i + \begin{cases}\ln\lambda_{b0} & \text{if } i\in\mathcal{B}\\ \ln\lambda_{m0} & \text{if } i\in\mathcal{M}\end{cases}} + \underbrace{\gamma_t}_{\ln\lambda_{bt}-\ln\lambda_{b0}} + \sum_{\substack{-\underline{\tau}\leq\tau\leq\overline{\tau}\\ \tau\neq 0}} \underbrace{\alpha^\tau D_i \times \mathbf{1}(\tau=t)}_{\substack{(\ln\lambda_{mt}-\ln\lambda_{bt})\\ -(\ln\lambda_{m0}-\ln\lambda_{b0})}} + \underbrace{\nu_{it}}_{\ln\phi_{it}+\zeta_{it}} \tag{1.9}$$

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. (1.9) 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. (1.9)

$$\ln AR_{it} = \eta_i + \gamma_t + \alpha D_i \times \mathbf{1}(\tau > 0) + \nu_{it} \tag{1.9'}$$

The residual ν_{it} in eq. (1.9) 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 $\widetilde{\theta}$, we square the predicted residuals $\widehat{\nu}_{it}$ after estimating eq. (1.9) and estimate a similar second-stage regression

$$\widehat{\nu}_{it}^2 = \underbrace{\psi_i}_{\substack{\sigma_i^2 \\ + \begin{cases} \sigma_{b0}^2 & \text{if } i \in \mathcal{B} \\ \sigma_{m0}^2 & \text{if } i \in \mathcal{M} \end{cases}}} + \underbrace{\upsilon_t}_{\sigma_{bt}^2 - \sigma_{b0}^2} + \sum_{\substack{-\underline{\tau} \le \tau \le \overline{\tau} \\ \tau \neq 0}} \underbrace{\beta^\tau D_i \times \mathbf{1}(\tau = t)}_{\substack{(\sigma_{mt}^2 - \sigma_{bt}^2) \\ -(\sigma_{m0}^2 - \sigma_{b0}^2)}} + \epsilon_{it} \tag{1.10}$$

where the facility-level fixed effect, ψ_i, captures any heteroscedasticity across facilities and any baseline difference in the dispersion of distortions between treated and treated facilities in the omitted year. The year fixed effect, υ_t captures annual changes in the dispersion of distortions for the control group relative to the omitted 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. When $\tau > 0$, β^τ estimates the difference in the dispersion of distortions between treated and control facilities due to the market-based policy, relative to the omitted year. The flexible function form of eq. (1.10) allows for the testing of pre-trends and time-varying policy change effects. 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. (1.7).

As with our first stage estimation, we also consider a difference-in-differences variant of eq. (1.10)

$$\widehat{\nu}_{it}^2 = \psi_i + \upsilon_t + \beta D_i \times \mathbf{1}(\tau > 0) + \epsilon_{it} \tag{1.10'}$$

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. (1.9), (1.9'), (1.10), and (1.10'), we cluster standard errors at a broader jurisdictional level (e.g., zip code or county depending on application) to account for arbitrary forms of spatial correlation and serial correlation in the residual within facilities of that jurisdiction.

1.3.5 Extension

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

Semi-parametric recovery of θ is still possible without the functional form assumption on $f()$, as detailed in Appendix A.1.2, provided we now employ the assumption that distortions ϕ_{is} are uncorrelated with emissions in the absence of policy, e_{io}. We have

$$\overline{\theta} \approx \widetilde{\theta} \left[1 - \frac{N(\delta - 1)}{E_b - E_m} \left(\frac{\varrho_m}{\delta \mu_m} - \frac{\varrho_b}{\mu_b} \right) \right] \tag{1.11}$$

where $\varrho_s = \frac{1}{N} \sum (\phi_{is} - \frac{1}{N} \sum \phi_{is})(e_{is} - \frac{1}{N} \sum e_{is})$ is the population covariance between distortions and emissions. Observe that each element in eq. (1.11) is either directly observed from data (e.g., e_{im}, e_{ib}, $E_m - E_b$) or can be estimated (e.g., ϕ_{im}, ϕ_{ib}). We use the difference-in-differences estimate of the policy-induced effect on emissions to obtain $\ln \delta$. except for $\delta = (E_o - E_m)/(E_o -$

E_b), the ratio of total abatement under policy m to that under policy b. We construct θ for a range of assumed δ values, including those that seem implausibly high.

1.4 Data

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

A contribution of this paper is the creation of a U.S. facility-level panel of economic and air pollution variables. Previous papers have matched panel of US plant-level pollution to a single year of ASM data (Shapiro and Walker, 2018) or used private plant-level data that proxy plant revenue.[16] We instead match facility level pollution data to restricted U.S. Census manufacturing economic variables over time. The following subsections detail the pollution data, the U.S. Census ASM and CM data, and how we link combine them.

CARB data

Yearly plant NO_x emissions and facility characteristics in California for 1990, 1993, and annually from 1995 to 2005 come from the California Air Resources Board (CARB). Emissions for the years 1991, 1992, and 1994 are not available. CARB collects criteria air pollution data under various state and federal mandates, and is aggregated from its thirty-five local air quality

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

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

districts (CARB, 2017). Under California mandates, facilities emitting above 10 tons of criteria pollution per year are required to report emissions annually.[17] This threshold is much higher at the federal level: the U.S. EPA's national emissions inventory covers only facilities with at least 100 tons per year of a criteria air pollutant. Since RECLAIM covers plants that emit as low as four tons of NO_x emissions per year, we follow previous studies in the literature by restricting our control plants those in the CARB data as it covers smaller emitting facilities than data from the U.S. EPA.

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

U.S. EPA data

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

Instead, we supplement the AMPD with data from the U.S EPA National Emissions Inventory (NEI). The NEI reports emissions of criteria pollutants for large point sources every three years. For treated plants without pre-treatment emissions, we use their U.S. EPA's Facility Registration Services (FRS) ID to obtain emissions in the NEI. For these facilities, we use their

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

NEI NO_x emissions for both the pre- and post-periods. NO_x emissions for control manufacturing plants are entirely from the NEI. Since the NEI only reports emissions every three year, we constrain the NBP sample to the years 1999, 2002, and 2005.[18] Facility-level characteristics in the combined AMPD and NEI used in our merge with the ASMCM (detailed below) include facility name, address, NAICS code, zip code, and county code.

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

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

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 A.2 provides further details on our matching procedure.

Since our outcome variable is a natural log transformation of a ratio, we drop plants who report either zero emissions or zero revenue. For RECLAIM, we match about 70% of the treated manufacturing plants to the ASMCM data, and about 40% of the control plants. One reason for the differential match rate is that the CARB data features smaller emitters that not included in the Annual Survey of Manufacturers. Indeed, the ASM probabilistically samples the smaller manufacturers. On the other hand, the average RECLAIM plant is a larger emitter than the average control plant in California, therefore making it more likely to be in the U.S. Census data. Similarly, we match nearly all of the 95 NBP manufacturing facilities to the ASMCM data, since the NBP covered very large emitters. However, we drop about 30 plants since they do not report pre-emissions in the AMPD data, and reported the same emissions for 2002 and 2005 in the NEI. Appendix A.4 presents further summary statistics for the unmatched and matched samples for RECLAIM and NBP.

1.5 Results

This section applies our empirical framework to the RECLAIM and NBP NO_x cap-and-trade markets. Using event-study and difference-in-differences models, we first establish that the introduction of the markets reduced NO_x emissions, consistent with results found elsewhere in the literature. We then report the first stage of our empirical procedure showing the effect of the pollution markets on average revenue of emissions by estimating equations (1.9) and (1.9').

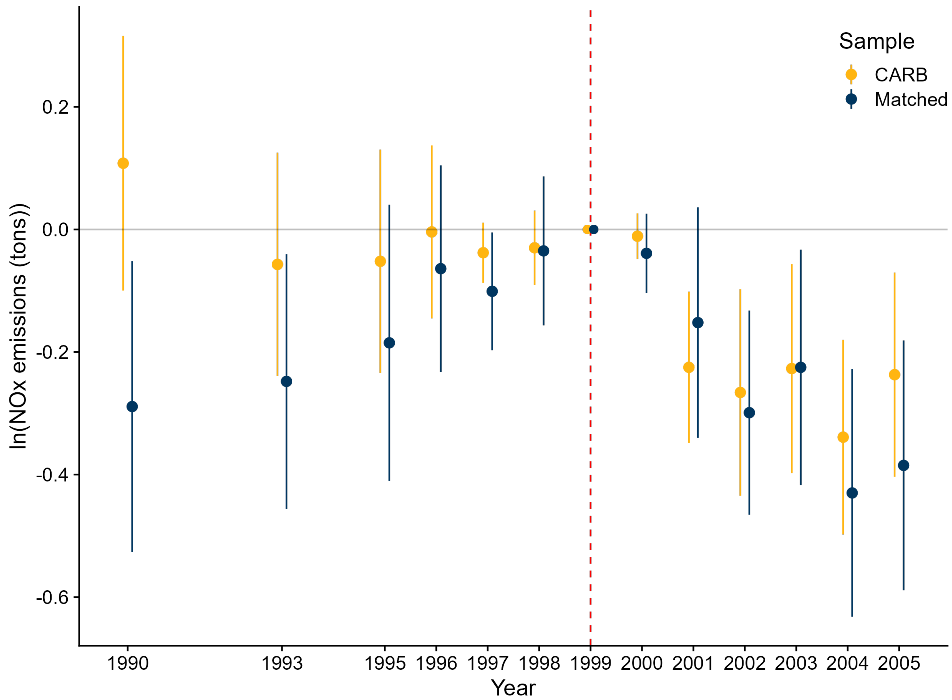
In our second stage, we take first-stage residuals and estimate the market-induced change in the variance of residuals using equations (1.10) and (1.10'). We then explore potential mechanisms driving differences in allocative efficiency gains and present several robustness checks. Section 1.5.1 presents results for RECLAIM program while Section 1.5.2 presents results for the NBP.

1.5.1 RECLAIM

We begin by estimating the effect of RECLAIM on NO_x emissions, the targeted pollutant. We do this both to quantify the emissions effect of RECLAIM for our sample of manufacturing facilities and to compare these effects with previous emissions effects reported in the literature using a similar research design. Figure 1.3 presents RECLAIM NO_x emissions estimates using the event-study model in equation 1.9 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 1.3: Event-study model of the effect of RECLAIM on NO_x emissions by sample

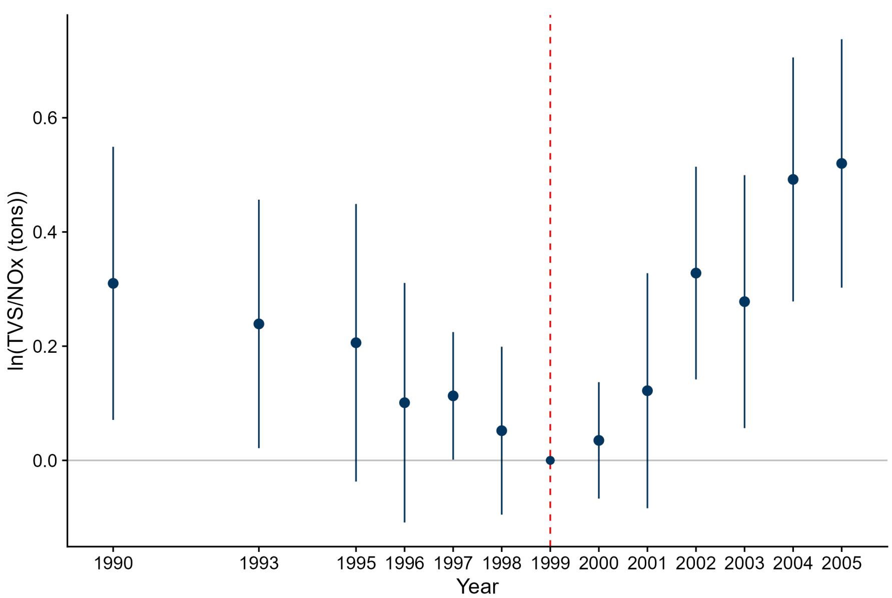


Notes: Point estimates and 95% confidence intervals of the yearly effect of RECLAIM on log NO_x emissions relative to 1999 using eq. (1.9). Estimates for the full sample of manufacturing facilities in CARB shown in gold. Estimates for the CARB-ASMCM matched sample shown in blue. Standard errors are clustered at the zip code level.

Columns (1) and (2) of Table 1.1 reports the average treatment effect of RECLAIM on NO_x emissions using the difference-in-differences specification in eq. (1.9') for CARB and matched CARB-ASMCM samples, respectively. The full sample suggests manufacturing plants covered by RECLAIM reduced their emissions by 18% compared to polluting facilities in the rest of California. While the average emissions effect for the CARB-ASMCM matched sample is a smaller -12%, it is not statistically different from the full CARB sample. This NO_x emission reduction effect is broadly consistent with emissions estimates from previous studies using similar research design, though these studies have not separately examined only manufacturing facilities (Fowlie, Holland and Mansur, 2012; Grainger and Ruangmas, 2018; Mansur and Sheriff, 2021). The reduction in total NO_x emissions after the introduction of RECLAIM highlights the importance of having a framework that allows total emissions target to change across policies, as considered in Section 1.3.

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 1.4 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 1.9. Consistent with the emission effect of the policy shown in Figure 1.3, 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 1.4 shows the shadow price of NO_x emissions trending downward prior to the cap binding for treated plants. RECLAIM reverses this trend.

Figure 1.4: Event-study model of the effect of RECLAIM on NO_x shadow price



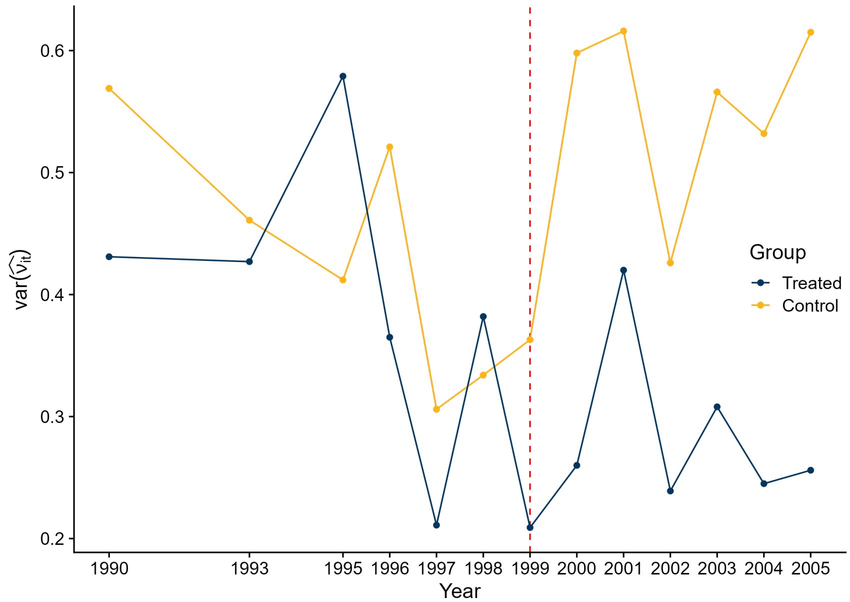
Notes: Point estimates and 95% confidence intervals of the yearly effect of RECLAIM on log revenue per emissions relative to 1999, or $\widehat{\alpha}^{\tau}$ using eq. (1.9). Standard errors are clustered at the zip code level.

Column (3) of Table 1.1 presents the average treatment effect of RECLAIM on log average revenue using equation (1.9'). The estimated coefficient represents the effect of RECLAIM on the aggregate NO_x shadow price. Consistent with the emissions effects detected in columns (1) and (2), RECLAIM increased the aggregate shadow price for NO_x emissions by 14%.

Before turning to our theory-informed estimate of the change in allocative efficiency under RECLAIM, we turn first to a more intuitive test of the policy-driven change in dispersion of distortions. Recall that allocative inefficiencies increase as the dispersion of distortions increase. As such, if a pollution market were to lower allocative inefficiencies, one should also see a drop in the annual cross-sectional variance of estimated residuals, $\widehat{\nu}_{it}$, from eq. (1.9), for treated facilities relative to control facilities after the market introduction. While the change in cross-sectional variance is directly linked to our theory, its intuitive connection with the dispersion in distortions can help build confidence in our eventual theory-based measure from Section 1.3.2.

This is shown in Figure 1.5. 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 1.5 hints at allocative efficiency gains from RECLAIM. Lower variance in residuals for treated facilities relative to control facilities suggest lower misallocation.

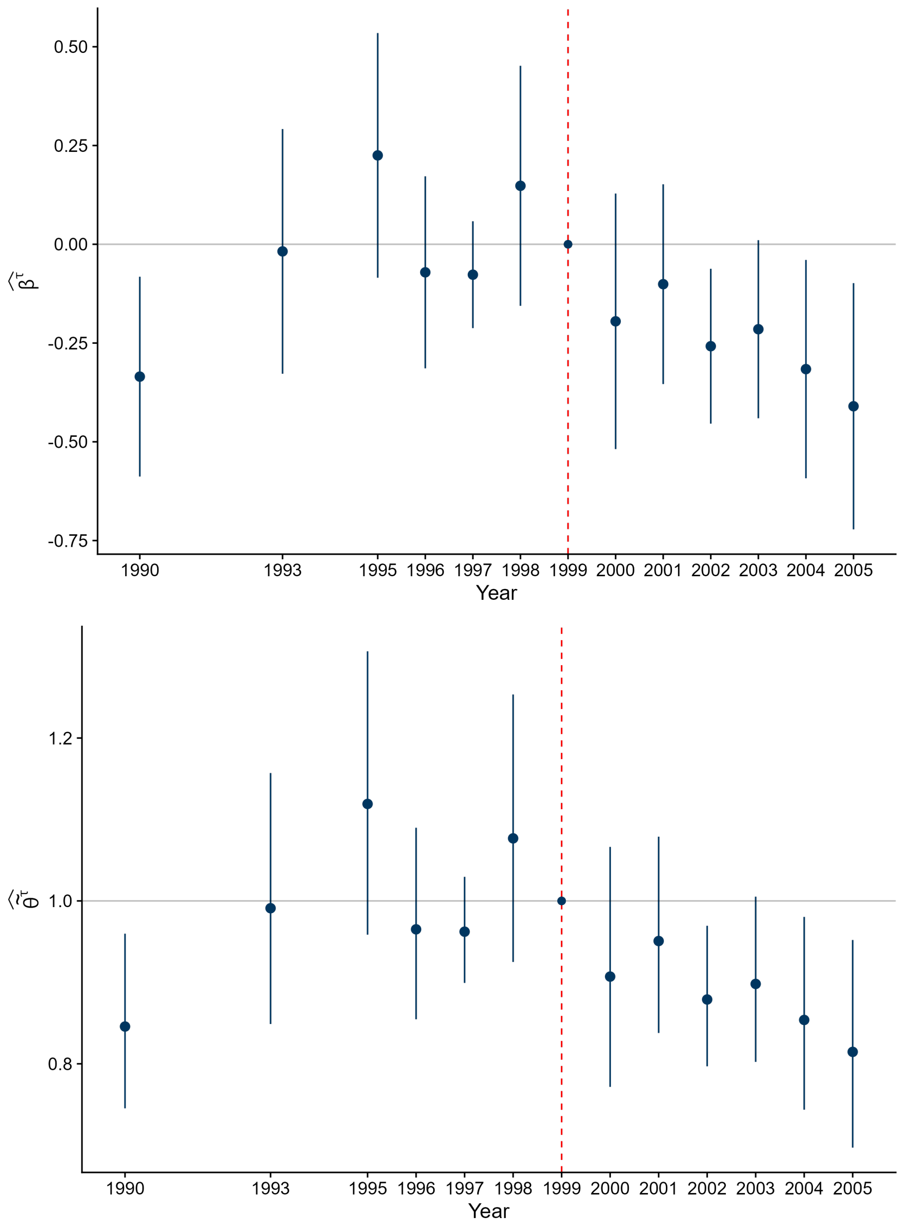
Figure 1.5: Difference in variance of distortions between RECLAIM treated and control plants



Notes: Blue and gold lines show the annual variance of the predicted residual, $\widehat{\nu}_{it}$, from equation 1.9 for treated and control plants, respectively.

The top panel of Figure 1.6 shows our main allocative efficiency effect for RECLAIM, plotting estimates $\widehat{\beta^{\tau}}$ from eq. (1.10). 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 1.6 presents the corresponding allocative efficiency measure, $\widehat{\widehat{\theta}} = e^{\frac{\widehat{\beta^{\tau}}}{2}}$. RECLAIM has lowered allocative inefficiency by about 10 percentage points. This effect increases in magnitude over time at an annual rate of roughly 2 percentage points.

Figure 1.6: 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. (1.10). Bottom panel shows for $\widehat{\widetilde{\theta}}^\tau = e^{\frac{\widehat{\beta}^\tau}{2}}$. Standard errors are clustered at the zip code level.

Column (4) of Table 1.1 presents the average treatment effect of RECLAIM on the dispersion of distortions or $\widehat{\beta}$ from equation (1.10'). Column (4) also shows the implied lower bound on the change in allocative efficiency, $\widehat{\widetilde{\theta}} = e^{\frac{\widehat{\beta}}{2}}$. A $\widehat{\widetilde{\theta}}$ of 0.898 indicates that the RECLAIM market led to allocative efficiency gains in NO_x emissions of 10 percentage points (i.e., $\widehat{\widetilde{\theta}}$-1). Under the parametric assumptions maintained in Proposition 1, $\widetilde{\theta}$ is a lower bound on, θ, the theory-based changed in allocative efficiency. Table 1.1 also presents a semi-parametric estimate of θ using equation 1.11 showing $\widehat{\widehat{\theta}} = 0.5$. Finding that $\widehat{\widehat{\theta}} < \widehat{\widetilde{\theta}}$ also reaffirms that $\widetilde{\theta}$ serve as a lower bound on the true allocative efficiency change.[19] Taken together, these estimates provides causal evidence that RECLAIM led to improvements in allocative efficiency in NO_x emissions.

Table 1.1: Average treatment effect of RECLAIM

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

Notes: Estimates of the average treatment effect of RECLAIM using a difference-in-difference model. All models include year- and facility-level fixed effects. Columns (1) and (2) examine log NO_x emissions as outcome using eq. (1.9). Column (3) models log average revenue per emissions as outcome using eq. (1.9'). Column (4) models the squared predicted residuals from eq. 1.9 as outcome using eq. (1.10). Column (1) uses the full CARB sample of manufacturing plants and the CARB facility identifier for facility fixed effects. Columns (2)-(4) uses the matched CARB-ASMCM sample and the LBD facility identifier for facility fixed effects. The lower bound on allocative efficiency change is $\widehat{\widetilde{\theta}} = e^{\frac{\hat{\beta}}{2}}$. The semi-parametric measure of allocative efficiency change is $\widehat{\widehat{\theta}}$ from eq. 1.11. Robust standard errors clustered at the zip code in parentheses.

Robustness checks We conduct several robustness checks of the average RECLAIM effects on NO_x emissions, average revenue of emissions (i.e., eq. 1.9'), and the dispersion of residuals (i.e., eq. 1.10'). Because of potential disclosure risks from increased information releases, the U.S. Census Bureau encourages the release of qualitative result for robustness checks, namely just the sign and statistical significance of the coefficients (U.S. Census Bureau, 2022). We

[19] When recovering $\widehat{\widehat{\theta}}$, we also consider varying the the ratio of total abatement under RECLAIM compared to the control group, or δ, as opposed to using the implied ratio from our estimates. Panel (a) of Figure A2 shows that for a reasonable range of δ, our estimates of $\widehat{\widehat{\theta}}$ for RECLAIM are always less than $\widehat{\widetilde{\theta}}$.

henceforth follow this guidance.

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

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

Table 1.2 estimates $\widehat{\widehat{\theta}}$ for each 2-digit SIC manufacturing sector by re-estimate eq. (1.10) in which the treatment variable is interacted with industry indicators. We find $\widehat{\widehat{\theta}} < 1$ 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 1.2: Allocative efficiency effect of RECLAIM by industry

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

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

To explore potential mechanisms, we turn to two dimensions. Table 1.3 examines heterogeneity in the RECLAIM effect on the squared residuals, $\widehat{\beta}$, by whether a plant is owned by a single- or multi-plant firm. One hypothesis is that firms with more than one plant might have more abatement reallocation options than firms that only operate a single plant. This logic is consistent with prior work that has shown increased production or abatement flexibility by multi-plant firms (Gibson, 2019; Cui and Moschini, 2020). Column (1) interacts the treatment variable with a dummy variable equal to one if it is operated by a multi plant firm. The uninteracted coefficient therefore represents the allocative efficiency gains from RECLAIM for single plant firms. The interacted coefficient suggests that there are imprecisely estimated small allocative efficiency gains for multi-plant firms relative to single plant firms from the market.

Table 1.3: Average treatment effect of RECLAIM by type of firm

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

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

Columns (2) and (3) analyze a separate sample whereby multi-facility firms that operate plants both inside and outside of RECLAIM are dropped. Column (2) replicates our main estimate without these firms that potentially violate the SUTVA assumption through reallocation of production or emissions to control plants. The overall allocative efficiency effect is statistically indistinguishable from our main estimate in column (4) of Table 1.1. However when dropping these multi region firms, column (3) shows larger effects on allocative efficiency gains between single- and multi-facility firms. In this sample, multi-facility firms are imprecisely estimated to have twice the efficiency gains versus single plant firms. Thus, Table 1.1 provides suggestive (and imprecise) evidence that firms with more abatement options experience larger allocative efficiency gains.

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

$$\ln AR^o_{it} = \eta^o_i + \gamma^o_t + \nu^o_{it} \tag{1.12}$$

where $o \in \{l, k\}$ for labor and capital, respectively. AR^o_{it} is now either average revenue per employee or dollar of capital expenditure. For each input and facility, we average the pre-period predicted residual from estimating eq. 1.12 and take the absolute value, denoted as $d^o_i \geq 0$. When $d^o_i = 0$, labor or capital distortions are zero on average for facility i before RECLAIM.

We interact the treatment variable from equation 1.10' separately with d^o_i for each input, and also jointly for both. Columns (1) and (2) of Table 1.4 separately interact the RECLAIM treatment variable with the capital and labor distortion measure, respectively. Column (3) jointly interacts both distortion measures. For facilities in which there are no distortions in other inputs, the uninteracted coefficient shows a drop in allocative inefficiency for NO_x emissions that are larger in magnitude than our full-sample results in column (4) of Table 1.1. Facilities with baseline labor and capital distortions experience smaller improvements in allocative efficiency, as indicated by the positive interaction coefficients. While statistically imprecise, the point estimate of the interacted effect is around one half of the average treatment effect. These heterogeneity results suggest that distortions in other inputs can prevent plants from achieving efficiency gains in the allocation of emissions following the introduction of a pollution market.

Table 1.4: Average treatment effect of RECLAIM by distortions in other inputs

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

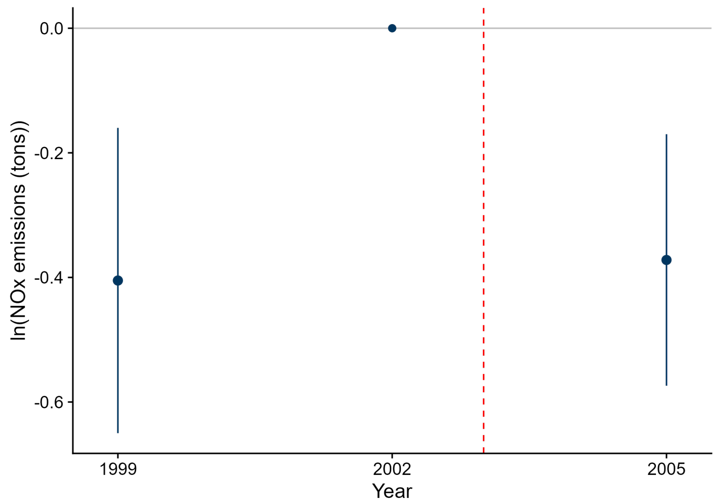
Notes: Estimates of the effect of RECLAIM on the dispersion of distortions. Column (1) interacts the RECLAIM treatment variable with absolute value in pre-policy capital distortions. Column (2) interacts the treatment variable with absolute value in pre-policy clabor distortions. Column (3) includes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

1.5.2 NO_x Budget Program

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

Figure 1.7 presents the NBP effects on NO_x emissions using an event-study model. The coefficients capture the difference in NO_x emission changes between treated and control manufacturing plants compared to the difference in 2002 before the introduction of the market in 2003. The 2005 coefficient shows that the program lowered NO_x emissions. While there are pre-policy differences in 1999, they are trending in the opposition direction of the post-policy trend. Column (1) of Table 1.5 presents the average treatment effect of the NBP on NO_x emissions. The NBP market reduced manufacturing facility emissions by about 18%. As a basis for comparison, using a triple-differences research design applied to a sample of power plants, Deschenes, Greenstone and Shapiro (2017) find that the NBP lowered seasonal NO_x emissions by 44%.

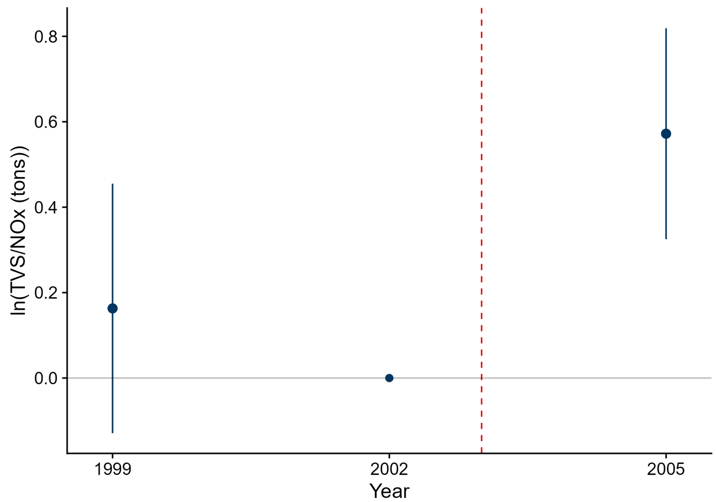
Figure 1.7: Event-study model of the effect of NBP on NO_x emissions



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. (1.9). Standard errors are clustered at the county level.

Figure 1.8 shows the effect of the NBP on average revenue per emissions using eq. (1.9'), which following Section 1.3.4 can be interpreted as the aggregate shadow-price of emissions under efficient allocation. Estimates indeed show an increase in the shadow price after the introduction of the NBP, consistent with the negative NBP emissions effect. The coefficient for 1999 does not indicate a precise 1999 difference between treated and control facilities. If anything, the 1999 effect trends in the opposite direction than the 2005 effect. Interestingly, these estimates of the NBP aggregate NO_x shadow price in Figure 1.8 mirrors closely the marginal pollution tax effect found in Shapiro and Walker (2018). Using a structural model, the authors find that the NBP increased the pollution tax of covered manufacturing plants by one log point in 2005, whereas our quasi-experimental estimate shows a 0.6 log point increase. Our 1999 effect also closely mirrors their estimate. Column (2) of 1.5 presents the average treatment effect of NBP on average revenue of emissions, or α from eq. 1.9. The estimate suggests an increase of 50% in aggregate NO_x price under efficient allocation for treated plants.

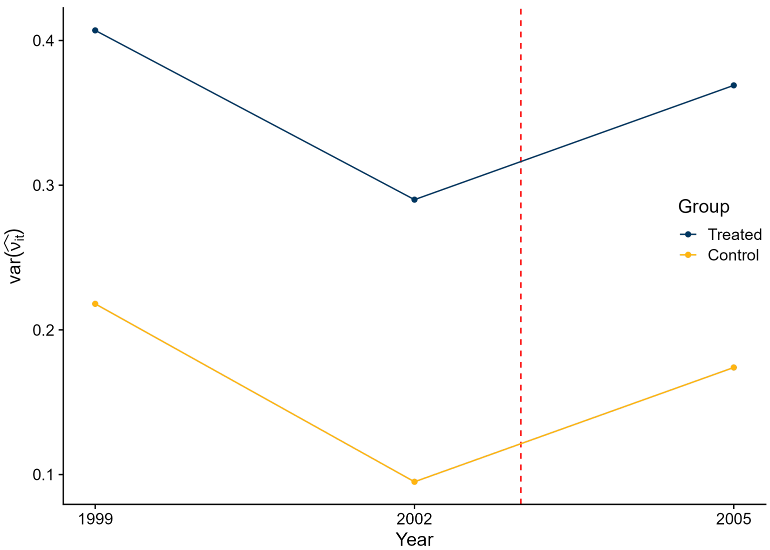
Figure 1.8: Event-study model of the effect of NBP on NO_x shadow price



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. (1.9). Standard errors are clustered at the county level

As with RECLAIM, we look at the annual cross-sectional variance in estimated residuals, $\widehat{\nu}_{it}$, separately for treated and control facilities, for an intuitive test for whether the program altered allocative efficiency. Unlike with RECLAIM, Figure 1.9 shows that no change in trend in the differential variance across treated and control facilities. The two time series exhibit similar gaps both before and after the introduction of NPB, suggesting that allocative efficiency changes may have been limited under the NBP.

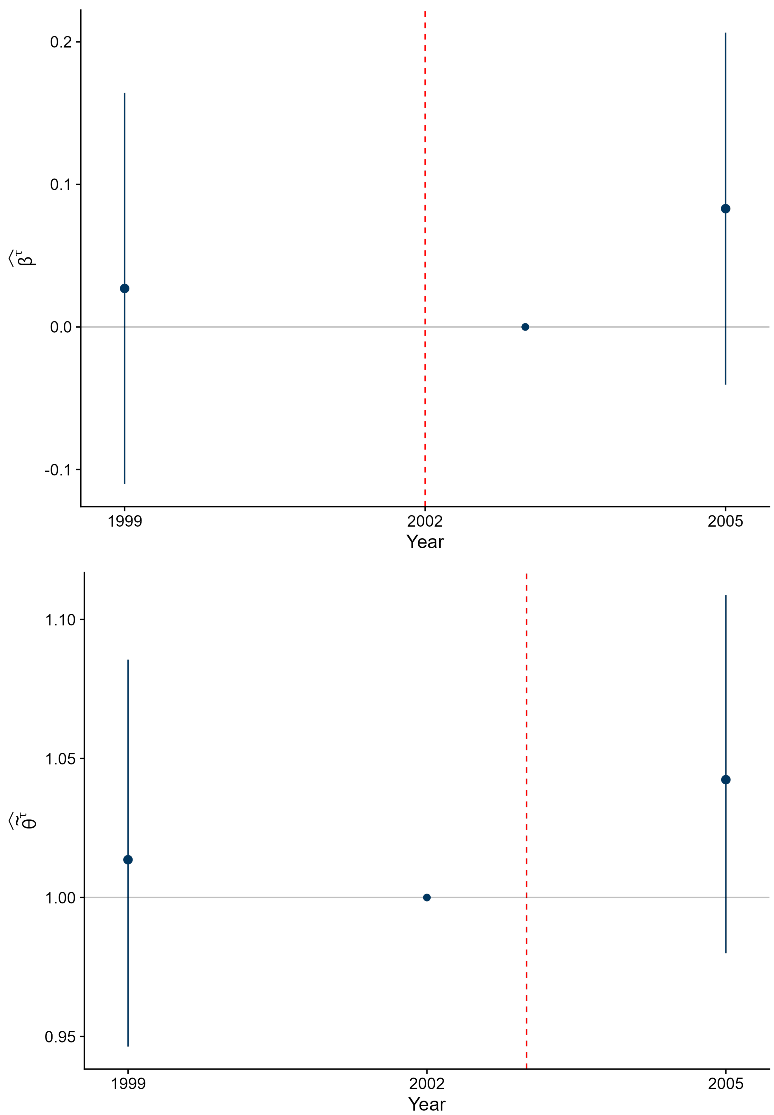
Figure 1.9: Difference in variance of plant level distortions between NBP treated and control plants



Notes: The blue and gold lines show the yearly variance of the predicted residual, $\widehat{\nu}_{it}$, from equation 1.9 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 1.10 plots estimates of β^{τ} from eq. (1.9') or the effect of NBP on the squared residual of average revenue per emissions. Coefficients before and after the NBP are not statistically significant. If anything, the positive post-treatment coefficient suggests a slight increase in misallocation from the policy. The bottom panel of Figure 1.10 presents the corresponding allocative efficiency measure, $\widehat{\widetilde{\theta}} = e^{\frac{\widehat{\beta^{\tau}}}{2}}$.

Figure 1.10: 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. (1.10). Bottom panel shows $\widehat{\widetilde{\theta}}^{\tau} = e^{\frac{\widehat{\beta}^{\tau}}{2}}$. Standard errors are clustered at the county code level.

The last column of Table 1.5 show the average treatment effect of NBP on squared residuals, or $\widehat{\beta}$ from eq. (1.10), and our related measure of allocative efficiency changes, $\widehat{\widetilde{\theta}} = e^{\frac{\widehat{\beta}}{2}}$. The table suggest an imprecise increase in allocative inefficiency as a result from the policy. The semi-parametric version of θ, or $\widehat{\overline{\theta}}$ from eq. 1.11, also suggests a small increase in allocative inefficiency.

Table 1.5: Average treatment effect of NBP

	$\ln \mathrm{NO}_x$ emissions	$\ln AR_{it}$	$\widehat{\nu}_{it}^2$
	(1)	(2)	(3)
NBP X Post	-0.176^{*}	0.494^{***}	0.070
	(0.105)	(0.133)	(0.056)
$\widehat{\widetilde{\theta}}$			1.036
			[0.98, 1.094]
$\widehat{\theta}$			1.083
Observations	16,500	16,500	16,500

Notes: Estimates of the average treatment effect of NBP using a difference-in-difference model. All models include year- and facility-level fixed effects. Columns (1) examines log NO_x emissions as outcome using eq. (1.9). Column (2) models log average revenue per emissions as outcome using eq. (1.9'). Column (3) models the squared predicted residuals from eq. 1.9 as outcome using eq. (1.10). The lower bound on allocative efficiency change is $\widehat{\widetilde{\theta}} = e^{\frac{\widehat{\beta}}{2}}$. The semi-parametric measure of allocative efficiency change is $\widehat{\widetilde{\theta}}$ from eq. 1.11. Robust standard errors clustered at the county level in parentheses.

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

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

We first consider a balanced panel. The sign and significance of the results are robust to that change. About half of the sample is maintained in the balanced panel. Since not all manufacturing facilities are covered in each NBP state, we also consider the subsample composed only of treated and control facilities in states with both sets of facilities. Again, our results are robust to this subset. Third, we restrict the control group to the same set of manufacturing industries as the treated group. Results are of the same sign and significance as in Table 1.5. Finally, to account for potential confounding changes at the state level and differential shocks at the industry level, we also run our main effects using state-by-year and industry-by-year fixed effects. Our results are again robust to those additions.

1.6 Conclusion

Market-based interventions hold the promise of improving allocative inefficiencies in settings where prices are otherwise missing. Pollution provides a classic example: the introduction of a market can in theory efficiently allocate emissions across heterogeneous polluters, lowering the total cost of meeting an aggregate pollution 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. Heterogeneity analyses suggest that facilities with less labor and capital distortions, and more abatement and output reshuffling options experienced greater efficiency gains.

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

Chapter 2

Carbon pricing and competitiveness pressures: The case of cement trade

2.1 Introduction

Concerns over the impact of carbon pricing on domestic industries are an important component of current climate policy debates in Canada. These concerns stem from the unilateral implementation of new or more rigorous climate policies by a jurisdiction compared to its trading partners. In Canada, as highlighted by Canada's Ecofiscal Commission (2015*a*), this situation could be problematic for provincial industries that are simultaneously carbon-and trade-exposed. While the carbon intensiveness of a province's electricity grid is an important determinant of the overall risk of a province, cement manufacturing is an industry that faces carbon competitiveness pressures despite its provincial location.

Because of its carbon-intensive production and exposure to international trade, cement manufacturing is a poster child for the competitiveness impacts of unilateral climate policy on domestic industries. In Canada, the historical patchwork of provincial carbon pricing policies has raised concerns by the industry. More precisely, British Columbia's (BC's) carbon tax is held responsible for the loss of local market share by British Columbian cement producers. In

February 2014, the Cement Association of Canada released a statement about its BC members: "local producers have lost nearly a third of the market share to imports since the inception of the carbon tax in 2008".

A competitiveness effect in this paper refers to a reduction in net exports due to BC's carbon tax. This definition is equivalent to the pollution haven effect (PHE) as defined in Levinson and Taylor (2008). As such, evidence of adverse competitiveness effects or a PHE on the cement industry from BC's carbon tax requires finding statistically significant reductions in net exports.

The objective of this paper is to empirically explore the impacts of BC's carbon tax on provincial cement manufacturing using cement trade and production data. This exercise is critical for two reasons. First, studying British Columbia's cement industry can shed light on the sensitivity of Canada's cement industry to carbon pricing. This is especially important as the stringency of carbon pricing policy is set to increase in Canada and because of the continued absence of national climate policy south of the border. Second, in the case of unilateral climate policy, competitiveness effect estimates are essential in determining whether and how much industry support the government should provide. In 2015, the BC government implemented a temporary subsidy for its cement industry seven years after implementing the carbon tax. In 2019, a permanent industry-wide emission intensity-based subsidy program was introduced by the province.

Several studies have empirically analyzed the impact of domestic carbon pricing or environmental regulation on international trade. Using pollution abatement expenditures as a proxy for U.S. regulatory stringency, Levinson and Taylor (2008) find modest impacts of environmental regulations on the average industry's trade position. They estimate that a 1 percent increase in pollution abatement costs for U.S. manufacturing industries decreased net exports by 0.064 percent as a share of value shipped. For the 20 industries for which pollution costs increased the most, this estimate implies that more than half of the total increase in trade volume is due to decreased net exports in response to increased pollution costs.

Using changes in energy prices as a proxy for carbon pricing, Aldy and Pizer (2015) find that a USD$30 per tonne carbon price in the U.S. would lead to a 10 percent production decline and a 2 percent decrease of net exports for energy-intensive sectors. This suggests that much of the decline in U.S. manufacturing production from changes in energy prices is primarily due to changes in domestic consumption as opposed to trade effects. Fowlie, Reguant and Ryan (2016*b*) use a US firm-level dataset to estimate the impact of changes in energy costs on imports, exports, and domestic production for different industries. They find that for an industry like cement, a hypothetical domestic carbon tax of USD$10 is associated with a 20 percent reduction in export volumes and increases in imports exceeding 10 percent.

Rivers and Schaufele (2015*a*) examine the impact of BC's carbon tax on the province's agricultural trade. Using provincial trade data of agricultural products, they find no evidence of reduced exports or increased imports as a response to the tax. As agriculture is a relatively less emission-intensive sector, this is not a surprising result and consistent with the above studies.

A related literature on the competitiveness effects of unilateral climate policies relies instead on multi-sector and multi-region computable general equilibrium (CGE) models. Fischer and Fox (2012) explore the effectiveness of different policies at addressing competitiveness impacts and the related carbon leakage stemming from domestic carbon pricing policies. In most cases, they find that full border carbon adjustments are the most effective policy to address adverse competitiveness impacts. Carbone et al. (2018) provide a comparison of CGE modelling results and econometric estimates for the employment responses to BC's carbon tax. They find responses of similar magnitudes and signs using both methods.

This paper builds on the previous econometric studies in two important ways. First, similarly to Rivers and Schaufele (2015*a*), I examine the impact of carbon pricing on the trade position of an industry using a stand-alone carbon tax within the province of BC, as opposed to relying on proxy variables. Second, I look at the impact of the policy on one of the most emission-intensive and trade-exposed industry in the province.

However, as opposed to the agricultural sector, the assumption of a competitive industry for cement manufacturing is less plausible. This is highlighted by the small number of cement plants in Canada and the parallel concerns in the American cement industry Fowlie, Reguant and Ryan (2016*a*) Miller, Osborne and Sheu (2017). A simple theoretical framework is used in the paper to argue that the concentration of the industry does not pose an issue for the main estimates. This result is primarily driven by the fact that BC can be considered a small open economy and that traded cement is assumed to be a perfect substitute to domestic cement.

The main results suggest that BC's CAD$30 per tonne carbon tax led to reduced net exports by 13 to 18 percent as a share of production. These results provide evidence of competitiveness impacts or a PHE of BC's carbon tax on its cement industry. Further empirical investigation suggests that reductions in domestic production as a response to the tax were driven by the trade effect as opposed to reduction in domestic consumption. These results provide justification of BC's output-based tax rebate policy to support its cement industry.

The remainder of the paper is separated into four sections. Section 1 provides an overview of the Canadian cement manufacturing industry and how it relates to carbon pricing policy. Section 2 presents the data and discusses the econometric models and results. Section 3 discusses the important factors governments should consider when designing support policies. The final section provides concluding remarks.

2.2 The cement industry and carbon pricing

Manufacturing cement is an energy- and greenhouse gas (GHG) emission-intensive process. Coupled with the fact that cement is increasingly shipped internationally, the cement industry is one of the most exposed to carbon competitiveness risks Miller, Osborne and Sheu (2017). Furthermore, the concentration in the cement industry adds a layer of complexity. This section explores the links between the industry's characteristics and carbon pricing.

Overview of the Canadian cement industry

Cement is the primary ingredient of concrete, a key construction material. Concrete is used for the construction of residential and non-residential buildings, bridges, roads, and sewage pipes. To produce cement, plants first heat raw materials, mostly limestone, silica, alumina and iron, in kilns at temperatures up to 1500 degrees Celsius. This produces cement's main input: clinkers. Clinkers are then ground with other additives, such as gypsum and limestone, to create cement.

The kiln process makes cement manufacturing energy-intensive. The production of clinkers accounts for 90 percent of the cement industry's energy consumption. Heating up the kiln relies heavily on burning carbon-intensive fossil fuels, such as coal or petroleum coke. For the past two decades, fossil fuel burning accounted for over 75 percent of Canada's cement industry's energy use (CIEEDAC, 2016).[1]

For every tonne of cement produced in Canada, nearly a tonne of GHG is emitted. However, more than half of the industry's GHG emissions are process emissions. These emissions are a by-product of the chemical reaction of limestone turning into clinkers. The remainder—about 40 percent of GHG emissions— is from burning fossil fuels, also referred to as combustion emissions. Distinguishing process and combustion emissions is important since BC's carbon tax only applies to combustion emissions.

In 2013, there were 17 operational cement plants in Canada owned by seven firms: seven plants in Ontario, four in Quebec, three in BC, two in Alberta, and one in Nova Scotia.[2] Because of its low value-to-weight ratio, cement is a costly product to transport over roads. As such, the industry has often been characterized as serving regional markets. In the U.S., an estimated 80 percent to 90 percent of domestically produced cement is trucked less than 200 miles or approximately 321 kilometers (Miller, Osborne and Sheu, 2017).

Coastal cement plants can face substantially more international import competition compared to landlocked cement plants. A coastal regional market such as Seattle has an import

[1] The use of alternative fuels, such as waste and wood fuels, is limited.

[2] The number of current fi rms is now down to six since the 2015 merger of Holcim and Lafarge.

market share of 65 percent, whereas an inland market such as Denver's has a null import share (Fowlie, Reguant and Ryan, 2016*a*).

In Canada, while regional market data is not available, provincial-level import market shares provide a similar but less extreme picture. In 2011, BC's import market share was slightly below 30 percent, the Prairies and Ontario close to 20 percent, and Quebec's below 15 percent. Higher import shares in Ontario compared to Quebec could be explained by the fact that good inland navigable waterways, such as the Great Lakes, can also increase competition for local producers.

British Columbia's carbon tax

By putting a price on GHG emissions, carbon pricing has the potential to increase the costs of manufacturing cement, both in terms of combustion and process emissions. In the case of BC's carbon tax, the price only applies to the GHG content of fossil fuels. As such, the policy might increase the costs of the cement industry's energy inputs, such as coal, petroleum coke and natural gas.

The first three rows of Table 1 provide the GHG emission intensity of Canada's cement industry.[3] Multiplying row one with the share of combustion emission gives the combustion-based emission intensity of cement. The last two rows provide different measures of the stringency of BC's carbon tax, namely the marginal tax rate per tonne of GHG and the carbon cost per tonne of cement produced. This last row is obtained by multiplying the combustion-based emission intensity with the per tonne of GHG carbon tax.

Table 2.1: Emission intensity of the cement industry and BC's carbon tax

Year	2007	2008	2009	2010	2011	2012
GHG/output	0.897	0.916	0.964	0.921	0.933	0.867
Share of combustion emissions	42.4%	43.5%	46.3%	43.8%	44.6%	40.3%
Combustion based GHG/output	0.380	0.399	0.447	0.404	0.416	0.350
BC carbon tax ($/tonne of GHG)	-	$10.00	$15.00	$20.00	$25.00	$30.00
BC carbon tax ($/tonne of cement)	-	$3.99	$6.70	$8.07	$10.40	$10.49

Source: CIEEDAC (2016)

Given an average price of cement of $100 per tonne (Miller, Osborne and Sheu, 2017),

[3] Data on the share of combustion emissions are only publicly available at the national industry level.

BC's 2012 carbon costs translate to about 10 percent of the product's price.[4] These costs are a significant portion of the finished product's price. For comparison, BC's carbon tax is equivalent to slightly less than 7 percent of gasoline's price at the pump (Lawley and Thivierge, 2018).

While this analysis focuses on BC's carbon tax, Quebec and Alberta have also had carbon policies in place since 2007. However, for the period considered in this paper, the tax rates were much lower, at about \$3 and \$1.80 per tonne, respectively, and were intended as revenue generating tools (Canada's Ecofiscal Commission, 2015*b*). Like other papers studying the impact of BC's carbon tax, the main focus of this analysis is on BC's pricing policy, as opposed to Quebec's and Alberta's (Rivers and Schaufele, 2015*a*,*b*; Yamakazi, 2017). As such, I exclude Quebec's and Alberta's policies from the main analysis. While not reported in the paper, robustness checks confirm that the results are not sensitive to the inclusion of all provincial policies.

Cement manufacturing, carbon pricing, and competitiveness pressures

It is important to situate the study within the broad concept of competitiveness. Dechezleprêtre and Sato (2017) define competitiveness as the ability of a firm, industry or sector to sell (measured in terms of market shares or net exports), to earn profits, and to attract investments. In this study, competitiveness effects refer to changes in the ability of the cement industry to sell products domestically and in international markets as measured by net exports.

Given the relative stringency of BC's carbon tax compared to its international cement trading partners, such as Asian countries or the U.S., the policy could reduce net exports. Finding such a reduction in net exports as a response to BC's carbon tax would constitute an adverse competitiveness impact (Aldy and Pizer, 2015). This is also referred to in the literature as a pollution haven effect (Levinson and Taylor, 2008). The pollution haven effect is defined as a reduction in net exports or foreign direct investment due to the tightening of domestic

[4] Monetary terms are expressed as Canadian dollars throughout.

environmental policy.

The reduction of net exports through increased imports or decreased exports can cause emission leakage. Emission leakage is defined as the increase in foreign emissions as a result of changes in domestic policy (Fowlie and Reguant, 2018). The consequence of leakage is that the relocation of economic activity might not change global GHG emissions, as they are simply emitted elsewhere. Since I do not observe foreign production, the study of leakage effects is beyond the scope this paper.

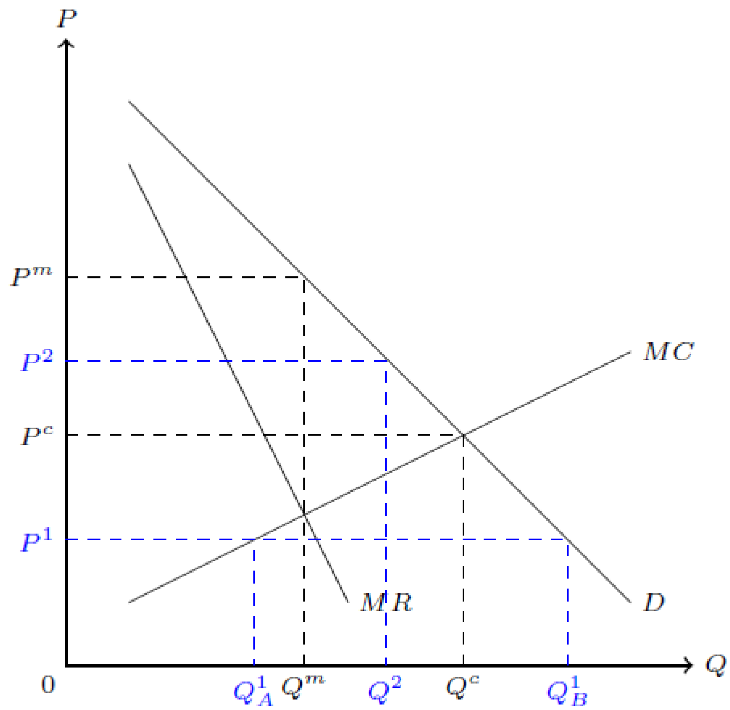
Market concentration in the Canadian cement industry

The small number of cement plants, and even smaller number of firms, makes cement manufacturing a concentrated industry (Competition Bureau, 2015). The prospect of market power and strategic behavior by firms poses potential issues with the interpretation of estimates of the impact of a carbon tax. This is evident in a static model of a monopolist in a closed economy. Because the marginal revenue curve is steeper than the demand curve, the change in quantity resulting from a carbon tax is smaller under a domestic monopolist than under a competitive industry. However, this prediction does not necessarily hold in an open economy context in which the three BC cement plants operate.

This section therefore explores the implications of imposing a carbon tax on a domestic industry that exerts local market power in an open economy. I consider the simplifying case of a domestic monopolist. The exercise draws inspiration from Stegemann (1984), who proposes a simple model to study how domestic monopolists behave in an open economy.

Figure 1 presents a diagram of an import-competing domestic monopolist. MC represents the marginal cost curve of the domestic monopolist, or the supply curve if it were a competitive industry. D represents the domestic demand curve, and MR the marginal revenue curve. In this model, I assume that the domestic country is a small open economy and that the domestic and imported goods are perfect substitutes. Before imposing a carbon tax on the domestic industry, we can think of two cases where equilibrium outcomes could be different depending on whether the domestic industry is a monopoly or a competitive market.

Figure 2.1: Import-competing domestic monopolist



Source: Author

Case (1): World price (P^1) is below the domestic competitive price (P^c) that would occur if the economy were closed. In this case, whether the domestic industry is a monopolist or a competitive market, the domestic industry would produce the same amount, i.e. at the intersection of the domestic supply curve and the world price (Q_A^1). The remaining demand ($Q_B^1 - Q_A^1$) is covered by imports. No monopoly burden exists.

Case (2): World price (P^2) is above the domestic competitive price (P^c) that would occur if the economy were closed. The domestic industry supplies the whole domestic demand, but the monopolist can restrict the quantity to Q^2, where the price is equal to the world price P^2. Monopoly burden can exist, however, this is in a market where there are no imports.

As such, under the assumption of a small open country trading perfect substitutes, the existence of imports precludes the exercise of market power. If one instead assumes a large open economy or imperfect substitutes, the domestic monopolist can retain some market power. However, the assumptions that (1) BC is a small open economy, and (2) that domestic and foreign cement are perfect substitutes, are plausible. Given positive imports of cement before and after policy implementation, this model implies that the estimates of the carbon tax's impact are the same, independent of market structure. Only if the market went from no imports to positive imports due to the carbon tax would there be an estimation issue.

For an exporting industry, under the same simplifying assumptions, market power can be maintained only if domestic price exceeds the export price, which requires some form of price differentiation through domestic market protection measures. This is not a plausible assumption for BC's cement industry.

This simple theoretical approach suggests that the equilibrium outcomes of imposing a carbon tax on BC's cement industry would be the same whether the domestic industry is a monopoly or a competitive industry. This result stems once again from the observation that BC traded cement before and after the policy implementation, and from the assumptions that BC is a small open economy, and that domestic and foreign cement are perfect substitutes. As such, the small number of plants in BC should not pose issues for my econometric estimates.

2.3 Empirical analysis of BC's carbon tax on cement trade

Determining whether a unilateral carbon tax led to changes in trade is an empirical question. BC's carbon tax lends itself well to this challenge. Indeed, the carbon tax can be treated as exogenous, since the province implemented a plausibly unanticipated policy (Rivers and Schaufele, 2015*b*). Assuming policy exogeneity, researchers can compare provincial outcomes before and after the carbon tax and across provinces to identify the impact of the policy separately from other economic trends.

This approach of exploiting BC's carbon tax as a quasi-natural experiment has been used by economists to look at the impact of the policy on multiple outcomes: aggregate fuel use

Elgie and Mcclay (2013), gasoline consumption (Rivers and Schaufele, 2015*b*; Antweiler and Gulati, 2016; Erutku and Hildebrand, 2018; Lawley and Thivierge, 2018), vehicle purchases (Antweiler and Gulati, 2016), natural gas consumption (Xiang and Lawley, 2019), employment (Yamakazi, 2017; Yip, 2018), and agricultural trade (Rivers and Schaufele, 2015*a*).

My analysis is restricted to the four largest cement producing provinces: BC, Alberta, Ontario and Quebec. Ideally my sample would also include the Atlantic provinces, which had cement producing capacity during the sample. Newfoundland had a cement plant which closed in 2000, and Nova Scotia has a single plant. However, both plants mostly served the domestic markets. For most of the sample, there are years where no cement was either exported or imported. Without consistent international trading in these markets, they are left out of the sample. This omission is a limitation of the study, as changes in industry dynamics in the Atlantic provinces are ignored in the analysis.

Data

The main outcome variables, the quantity in tonnes of provincial cement imports and exports, are pulled from Statistics Canada's Canadian International Trade Database. The variables' time frame spans 2002 to 2013. They are at the quarterly frequency. Net exports are calculated as the difference between exports and imports. Yearly provincial cement production data is provided by the Cement Association of Canada for the same years. Scaling trade data by production accounts for different sizes in provincial cement industries (Levinson and Taylor, 2008).

The econometric models include provincial and quarterly varying control variables to account for differential rates of demand for cement in the four provinces, namely provincial quarterly unemployment rates and the number of residential building starts. The expected impact is that increased local demand in cement should increase imports, reduce exports and therefore also reduce net exports. The inclusion of unemployment rates and residential construction building starts follows the covariates employed in the import supply equation in Fowlie, Reguant and Ryan (2016*a*).

Table 2.2: Summary statistics, 2002 to 2013

	BC		**Alberta**		**Ontario**		**Quebec**	
	Mean	S.D.	Mean	S.D.	Mean	S.D.	Mean	S.D.
Imports	176	103	320	84	348	131	186	67
Exports	1056	333	221	40	2684	498	547	203
Net exports	880	401	-99	97	2336	593	361	230
Production	2195	390	2230	211	5624	640	2093	264
Residential starts	29	7	35	9	72	11	48	5
Unemployment	6.6%	1.4%	4.8%	1.1%	7.3%	0.9%	8.1%	0.6%

Source: Author's calculations, Statistics Canada and Cement Association of Canada
Notes: All trade measures are international and omit interprovincial trade. Imports, exports, net exports and productions are in thousands of tonnes. S.D. stands for "standard deviation".

Table 2 provides summary statistics of the cement industries in the four provinces. All provinces but Alberta are net exporters of cement. BC and Ontario export on average at least six times the quantity of cement imported. This is especially important for BC, as a large increase in imports need not change its net exports significantly. In terms of domestic production, Ontario has by far the largest cement industry. Alberta is second, followed by BC and Quebec

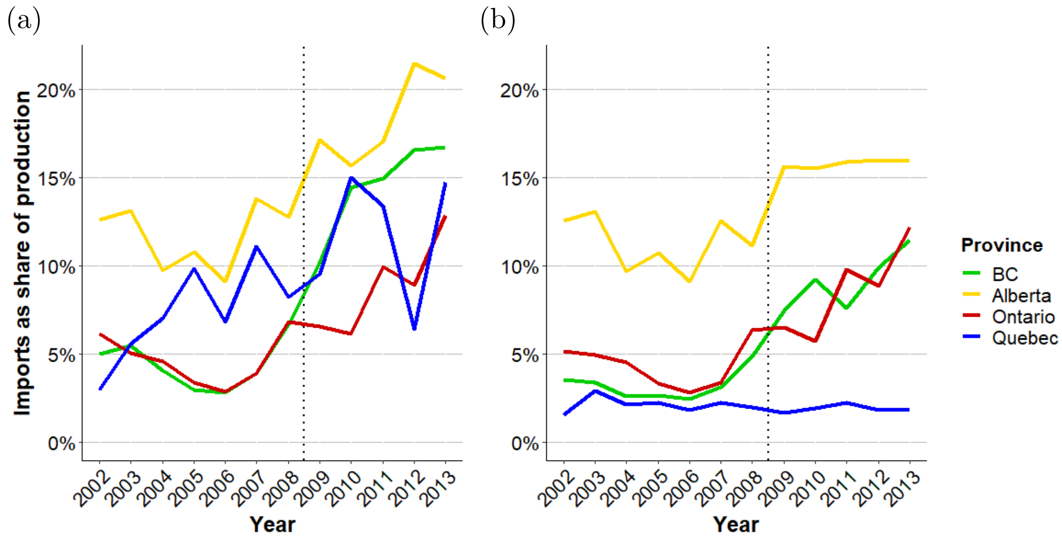
Descriptive analysis

This section provides a descriptive summary of the changes in trade in cement producing provinces before and after the carbon tax implementation. The following sections proceed with the formal empirical analysis of the policy's impact.

Figure 2 plots yearly imports of cement scaled by production in the four provinces from 2002 to 2013. The left panel presents total imports from all countries, while the right panel provides the subset of imports from the U.S. The blue dotted line shows the implementation date of BC's carbon tax. It is evident from both panels that most imports are from the U.S. for all four provinces but Quebec.

From both panels, imports in BC have increased following the implementation of the policy. However, two points suggest caution in assigning this impact to the carbon tax. First, imports were also increasing in the other three provinces. This could be explained by the global recession of 2008, which happened around the same time as the implementation of the tax, making the identification of the tax's impact separately from the recession graphically difficult.

Figure 2.2: Yearly International Imports of Cement by Province: (a) Imports from the World and (b) Imports from the United States

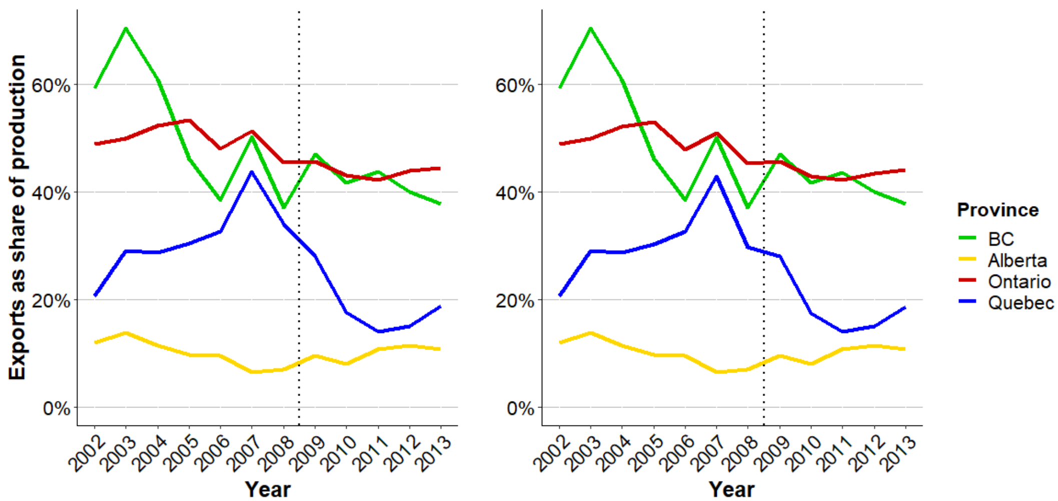


Source: Canadian International Trade Merchandise Database and Cement Association of Canada
Notes: The dotted line shows the implementation date of British Columbia's carbon tax.

Figure 3 presents yearly exports scaled by production of cement for the four provinces. Panel A includes yearly provincial exports to the world and panel B shows yearly provincial exports to the U.S. As evident from the similarity of both panels. nearly all provincial international exports are destined to the U.S. For most provinces, exports were falling prior to the tax. However, this was only the case for Quebec from 2006 onwards. After the tax, exports began rebounding by 2011 for all provinces but BC.

Figure 2.3: Yearly International Exports of Cement by Province: (a) Exports to the World and (b) Exports to the United States

(a) (b)



Source: Canadian International Trade Merchandise Database and Cement Association of Canada
Notes: The dotted line shows the implementation date of British Columbia's carbon tax.

While graphical analysis can help elucidate the impact of the carbon tax, the above figures do not provide strong evidence of an effect. More understanding can be gained on the relationship between the tax implementation and trade using rigorous statistical analysis. As such, the next sections present econometric models and results.

Econometric modelling

To more formally assess the impact of BC's carbon tax on cement trade, this section relies on econometric modelling. The models look at changes in trade data in BC and other major cement producing provinces while controlling for important economic factors. Specifically, I estimate the impact of the carbon tax using two models.

The first is the log-linear model used in Rivers and Schaufele (2015*a*):

$$\log\left(Y_{p,y,q}\right) = \beta_1 \tau^{BC}_{p,y,q} + X_{p,y,q}\Theta_1 + \delta_{1p} + \gamma_{1p} + \zeta_{1q} + \varepsilon_{p,y,q} \tag{1}$$

The second is an adaptation of the specification used in Levinson and Taylor (2008):

$$\left(\frac{Y_{p,y,q}}{Q_{p,y}}\right) = \beta_2 \tau^{BC}_{p,y,q} + X_{p,y,q}\Theta_2 + \delta_{2p} + \gamma_{2p} + \zeta_{2q} + \omega_{p,y,q} \tag{2}$$

where $Y_{p,y,q}$ is the quantity in tonnes of cement exports, imports or net exports in province p, year $y$and quarter q; $Q_{p,y}$ is the yearly provincial cement production in tonnes; $\tau^{BC}_{p,y,q}$ is the per tonne of cement carbon tax if the cement industry is in BC; $X_{p,y,q}$ is a vector of provincial economic and construction variables; δ_p, γ_y, ζ_q are province, year and quarter fixed effects, respectively; and $\varepsilon_{p,y,q}$ and $\omega_{p,y,q}$ error terms.

Because the sample only includes four provinces, clustering of the error terms by province would be inappropriate. As discussed in Cameron, Gelbach and Miller (2008), problems arise when conducting statistical inference with a small number of clusters. The calculated standard errors will be mechanically too small as they lack the required asymptotic properties. When dealing with a small number of clusters, one strategy is to use the wild cluster bootstrap method. However, in practice at least five clusters are required for this approach. As such, in order to allow both cross-sectional and time-series correlation structures in my standard errors, I use the Newey-West autocorrelation-robust approach (Newey and West, 1987).[5]

From models (1) and (2), the coefficients of interest are β_1 and β_2, which estimate the impact of the carbon tax. β_1 represents the percentage change in imports, exports, or net exports for a one dollar increase in carbon costs. Because model (1) takes the natural logarithm of the dependent variable and there are quarters with negative net exports, I cannot directly estimate model (1) for net exports. However, using the computed coefficients and standard errors on logged imports and exports, it is possible to calculate the implied net export response to the carbon tax.[6] In model (2), for imports, exports, and net exports, β_2 represents the percent change of trade as a share of cement production from a one dollar increase in carbon costs.

[5]I choose lags of four quarters of autocorrelation for the Newey–West standard errors. When using robust or clustered standard errors, the signifi cance of the results does not qualitatively change.

[6]The net export response to the carbon tax in Model (1) is $\beta_1^N = \frac{d\ln(N)}{d\tau} = \frac{1}{N}\frac{dN}{d\tau}$, where N is net exports. Because $N = E - I$, where E is exports and I is imports, $\frac{1}{N}\frac{dN}{d\tau} = \frac{1}{N}\frac{d(E-I)}{d\tau} = \frac{1}{N}\left(\frac{dE}{d\tau} - \frac{dI}{d\tau}\right) = \frac{E}{N}\frac{d\ln E}{d\tau} - \frac{I}{N}\frac{dlnI}{d\tau}$. Therefore, I can rewrite $\beta_1^N = \frac{E}{N}\beta_1^E - \frac{I}{N}\beta_1^I$ and calculate it using the estimated regression coefficients and the mean values of net exports, exports, and imports for British Columbia, that is, $\hat{\beta}_1^N = \frac{\bar{E}}{\bar{N}}\hat{\beta}_1^E - \frac{\bar{I}}{\bar{N}}\hat{\beta}_1^I$. To calculate the standard errors, I take the square root of sum of the weighted estimated variances from the export and imports models in Table 3 and of the estimated covariance between the two coefficients by bootstrapping the sample, that is, $se(\hat{\beta}_1^N) = \sqrt{(\frac{\bar{E}}{\bar{N}})^2\widehat{var}(\hat{\beta}_1^N) + (\frac{\bar{I}}{\bar{N}})^2\widehat{var}(\hat{\beta}_1^N) - 2\frac{\bar{E}\bar{I}}{\bar{N}^2}\widehat{cov}(\hat{\beta}_1^E, \hat{\beta}_1^I)}$. To calculate the estimated covariance between the import and export coefficients, I create 1,000 bootstrap samples in which each bootstrap draw includes all the variables for estimating Model (1) with exports or imports as dependent variables. Once the bootstrap samples constructed, I estimate the coefficients and estimate their covariance.

Similarly, coefficients for the covariates for model (1) can be interpreted as the effect of a one unit change in the variable leading to a Θ_1 percent change in trade. For model (2), Θ_2 represent the percent change of trade as a share of cement production from a one unit increase in the covariate.

While not the main outcome of interest in this paper, provincial production and consumption of cement are also considered as left-hand side variables. Such regressions intend to provide a better picture of the changes in the cement industry from the carbon tax.

Province and time fixed effects are included in the models to account for observed and unobserved differences affecting provincial cement industries. Province fixed effects account for constant differences between province cement industries, such as market access, industry costs and structure. For example, it accounts for BC cement plants' higher trade exposure to Asian cement production. The quarter fixed effects control for seasonality in cement trade. The year fixed effects account for factors such as changes in the general economic context in Canada, such as recessions, exchange rates, commodity prices, and federal trade policy. These time fixed effects account for common changes that affect the whole Canadian cement industry.

By omitting input and output prices, both models assume that provincial cement industries pay the same input prices and receive the same output price up to a time invariant difference. Since cement can be thought of as a commodity, and because energy markets are integrated, this fixed difference assumption is reasonable. The additional control variables included in the models account for factors ignored by the fixed effects that might influence cement trade, such as changes in provincial economic and construction activity.[7]

Econometric results

I present and discuss the result of several specifications where the trade variables are either logged or expressed as a share of production. All econometric specifications include province,

[7] A potential weakness of the empirical model is the omission of local US data on unemployment and housing starts. For example, housing starts in Washington State are likely to influence cement export patterns in British Columbia.

quarter, and year fixed effects. The objective of all models is to assess whether BC's carbon tax led to significant changes in the province's cement industry's trade position.

To address the importance of the rise in overseas cement imports, I analyze trade separately with the U.S. and the world. World imports might be more sensitive to carbon costs than U.S. imports if indeed the policy is leading to increased overseas imports.

The first two regression tables present the effect of the carbon tax for six trade outcome variables: U.S. imports, world imports, U.S. exports, world exports, U.S. net exports, and world net exports.[8] Using coefficients for models (1) and (2), Table 5 provides estimates of the impact of BC's \$30 per tonne carbon tax on imports and net exports. Table 6 then investigates whether the tax led to changes in domestic production and consumption of cement.

Table 3 reports results from model (1) accounting for all fixed differences between provincial cement industries, yearly changes to the Canadian industry, and two provincial time varying control variables: quarterly unemployment rate and residential construction starts. Since net exports are negative for many quarters, this prevents estimation of model (1) with net exports as the dependent variable. The last two columns present the calculated net export response to the carbon tax using the estimated coefficients from imports and exports.

All the coefficients for the carbon tax have the expected sign, i.e. increase in imports, and reduction in exports and net exports. All coefficients are also statistically different than zero. A one-dollar increase in the per cement tonne carbon cost is associated with a 6 percent increase in the quantity of U.S. and of world imports. The carbon tax coefficients for exports and net exports to the U.S. and the world respectively imply reductions of 2.5 and 4.2 percent from a one dollar in increase in the carbon cost per tonne of cement. The estimated coefficients on imports and exports are of similar magnitudes to the coefficients reported in Fowlie, Reguant and Ryan (2016*b*).

[8] A better approach to test the sensitivity of the effect of carbon tax on trade by trading partner would be to look at either the United States or the world minus the United States. However, as shown in Figures 2 and 3, most of cement provincial trade is with the United States. As such, world trade minus the United States includes quarterly null values that cannot be used in these models.

Table 2.3: Estimated effects of BC's carbon tax and other factors on trade

	(1) log(Imports from U.S.)	(2) log(Imports from world)	(3) log(Exports to U.S.)	(4) log(Exports to world)	(5) log(Net exports to U.S.)	(6) log(Net exports to world)
BC carbon tax	0.062∗∗∗	0.060∗∗∗	-0.025∗	-0.025∗	-0.042∗∗	-0.042∗∗
	(0.015)	(0.015)	(0.011)	(0.011)	(0.014)	(0.014)
Residential starts (thousands)	0.054∗∗∗	0.054∗∗∗	0.039∗	0.039∗	N.A.	N.A.
	(0.013)	(0.015)	(0.015)	(0.015)		
Unemployment rate	0.131∗∗∗	-0.05	0.154∗∗∗	0.158∗∗∗	N.A.	N.A.
	(0.032)	(0.047)	(0.036)	(0.037)		
Number of observations	192	192	192	192	N.A.	N.A.
R^2	0.67	0.63	0.73	0.72	N.A.	N.A.

Notes: Columns (1) to (4) report results from regressions including province, year and quarter fixed effects. The carbon tax coefficients in column (5) and (6) are calculated from $\frac{E}{N}\beta_1^E - \frac{I}{N}\beta_1^I$, where N, E and I are respectively net exports, exports and imports, using the mean values in Table 2 and the estimates in columns (1) to (4). See endnote 8 for the standard error calculations for column (5) and (6). N.A. stands for "not available". Newey-West standard errors are in parentheses. ∗p<0.1; ∗∗p<0.05; ∗∗∗p<0.01.

The coefficients for residential starts make intuitive sense for imports, but not exports. For imports, coefficients are positive and significant. Estimates suggest that, for both U.S. and world imports, an additional 1000 residential starts result in about 5 percent increase in cement imports. Counter to economic intuition, the same increase in residential starts is also significantly associated with increased exports of 4 percent.[9] The opposite is true for the effect of unemployment on trade. The sign of the coefficients on exports follow economic intuition, but not for imports. A one percentage point increase in unemployment is linked to increases in U.S. imports and exports of more than 10 percent. The effect is of similar magnitude and sign only for world exports. However, most importantly, the sign of the carbon tax coefficients do not change when removing housing starts and unemployment rate.

Table 4 presents the results from model (2), which normalizes the trade variables by domestic cement production. Similar to the results from model (1), these estimates suggest that the carbon tax led to statistically significant increases in imports and decreases in exports and net exports. More specifically, a one-dollar increase in carbon cost is linked to a 0.3 percent and 0.6 percent increase of U.S. and world imports as a share of production respectively. A one-dollar increase in costs reduces exports to the U.S. and the world as share of production by 0.9 percent. Both of these effects translate to respective reductions of 1.2 and 1.5 percent

[9]As suggested in Note 7, the omission of data on local US economic conditions could account for this sign.

in net exports to the U.S. and world from a one dollar increase in carbon costs.

Table 2.4: Estimated effects of BC's carbon tax and other factors on trade as a share of production

	(1) Imports from U.S. (%)	(2) Imports from world (%)	(3) Exports to U.S. (%)	(4) Exports to world (%)	(5) Net exports to U.S. (%)	(6) Net exports to world (%)
BC carbon tax	0.003∗∗	0.006∗∗∗	-0.009∗	-0.009∗	-0.012∗∗	-0.015∗∗∗
	(0.001)	(0.001)	(0.003)	(0.003)	(0.004)	(0.003)
Residential starts	0.0003	0.0003	0.015∗∗∗	0.015∗∗∗	0.015∗∗∗	0.015∗∗∗
(thousands)	(0.001)	(0.001)	(0.004)	(0.004)	(0.004)	(0.004)
Unemployment rate	0.011∗∗∗	-0.003	0.062∗∗∗	0.064∗∗∗	0.052∗∗∗	0.062∗∗∗
	(0.002)	(0.005)	(0.011)	(0.011)	(0.012)	(0.012)
Number of observations	192	192	192	192	192	192
R^2	0.60	0.66	0.69	0658	0.62	0.62

Notes: All regressions include province, year and quarter fixed effects. Newey-West standard errors are in parentheses. ∗p<0.1; ∗∗p<0.05; ∗∗∗p<0.01.

Results in Tables 3 and 4 provide evidence that BC's carbon tax decreased international net exports for the province's cement industry. These results are also of similar magnitude for world and U.S. trade. As such, these findings provide some evidence of a pollution haven effect for the cement industry.

Because of the transformations of the outcome variables, the coefficients from Tables 3 and 4 are not directly comparable. As such, using the coefficient of the carbon tax from each table, Table 5 provides estimates of the impact of BC's carbon tax on the cement industry for imports, exports and net exports as a share of the industry's production.

Table 2.5: Summary of the estimated effects of BC's carbon tax on trade as a share of production

	(1) Imports from U.S.	(2) Imports from world	(3) Exports to U.S.	(4) Exports to world	(5) Net exports to U.S.	(6) Net exports to world
Model (1)	5.2%	5.0%	-12.6%	-12.6%	-17.8%	-17.7%
Model (2)	3.1%	6.3%	-9.4%	-9.4%	-12.6%	-15.7%

Notes: For the model (1), the impact is estimated as the related carbon tax coefficient from Table 3, multiplied by the total carbon costs per tonne of cement, multiplied by the average volume of trade over the average cement production. For model (2), it is the related carbon tax coefficient from Table 4 multiplied by the total carbon cost per tonne of cement.

Models (1) and (2) suggest that the $30 per tonne carbon tax increased U.S. imports by 3.1 to 5.2 percent as a share of average BC cement production. Estimates for world imports indicate they increased by 5 to 6.3 percent as a share of production following the policy. Coefficient estimates translate to reduced exports to the U.S. and world of 9.4 to 12.6 percent. The similarity of the estimates between U.S. and world exports stems from the fact that most provincial cement exports is destined for the U.S. For net exports to the U.S., the estimates range from a 12.6 to 17.8 percent reduction as a share of production as a response to the carbon tax. For net exports to the world, results range from a 15.7 to 17.7 percent reduction as a result of the province's carbon pricing policy.

The potential reduction in net exports as a response to the carbon tax provides suggestive evidence of an adverse competitiveness impact. This competitiveness effect could reflect substitution of domestic cement production with imported cement. To assess the importance of domestic cement substitution, I explore whether domestic consumption of cement in BC responded to the tax. If domestic consumption did not respond to the policy, this would suggest a one-for-one substitution of imports for domestic production (Aldy and Pizer, 2015).[10] However, if domestic consumption declined, then domestic production would have decreased by more than the changes in imports. This could reflect more efficient use of cement by BC industries.

Separating the international competitiveness effect from changes in domestic consumption can inform the proper policy response. Indeed, trade-related support policies might help the industry if the main change in production is not driven by reductions in domestic consumption.

Table 6 looks at the effect of BC's carbon tax on logged domestic production and cement consumption in column (1) and (2). Results suggest that the carbon tax did indeed lead to reductions in domestic cement production, but no reductions in domestic consumption. Rather, the estimate suggests domestic consumption of cement in BC increased following the carbon tax. This implies net imports increased more than the fall in production. This provides evidence that reductions in production are driven by trade-related competitiveness pressures.

[10]Domestic consumption is defi ned as domestic production minus exports and plus imports.

Table 2.6: Estimated effects of BC's carbon tax and other factors on cement production and domestic consumption

	(1) log(Production)	(2) log(Domestic consumption)
BC carbon tax	-0.014∗	0.020∗
	(0.006)	(0.009)
Residential starts (thousands)	0.001	-0.020∗
	(0.004)	(0.009)
Unemployment rate	-0.020	-0.132∗∗∗
	(0.012)	(0.025)
Number of observations	192	192
R^2	0.68	0.52

Notes: All regressions include province, year and quarter fixed effects. Newey-West standard errors are in parentheses. ∗p<0.1; ∗∗p<0.05; ∗∗∗p<0.01.

While not included in the paper, robustness checks were implemented to complement the main results. Specifically, I re-estimated the models including all provincial carbon pricing policies, i.e. BC's carbon tax, Quebec's carbon tax, and Alberta's intensity-based standard. Omitting the carbon prices in Alberta and Quebec could confound the estimates. The results from including all policies are similar to the ones reported in the paper.

Another factor that could confound the estimates is the hosting of the 2010 Winter Olympics by Vancouver. The Olympics occurred while the carbon tax was increasing. This correlated event could bias the estimate since the hosting of the Olympics required infrastructure investments. Increases in cement imports and reductions in exports due to the Olympics prior to the carbon tax implementation would bias the estimate downward. However, the same changes in cement trade after the carbon tax would bias the effect of the policy upward. The models were re-estimated including the value of non-residential building permits by province and quarter. The results are again similar to the main results included in the paper.

For BC's cement industry, the trade effect driven by the carbon tax seems to be the main force impacting changes in domestic production of cement. This finding justifies government intervention to relieve this competitiveness impact. The next section explores the policy space for competitiveness support for the cement industry.

2.4 Design of competitiveness support policies

As one of the more trade-exposed and emission-intensive industries, the econometric modelling provides evidence that a $30 per tonne combustion-based carbon tax applied to the cement industry's inputs can lead to adverse trade-related competitiveness impacts.

For governments seeking to reduce domestic greenhouse gas emissions with carbon pricing, reducing competitiveness pressures for a sector or industry is an additional policy goal. The Tinbergen Rule states separate policy objectives should be met with separate policy instruments. In the case of BC's carbon tax, this implies introducing an additional policy for its cement plants as opposed to exempting the industry from its carbon tax. Exempting the cement industry would reduce the coverage of the policy and might prevent cost-effective emission reductions. Policy solutions to alleviate trade-related competitiveness pressures include output-based subsidies under a carbon tax, output-based free permit allocation for cap-and-trade systems, or border carbon adjustments (BCAs). By subsidizing production, output-based policies (OBPs) incentivize domestic firms to reduce emissions through other means than reductions in output. BCAs level the playing field of domestic and foreign producers by either imposing a tariff on the embedded carbon in imports, a subsidy on the embedded carbon content of domestic exports, or a full BCA which includes both components. Since BCAs might violate WTO agreements, the policy debate has focused on the use of OBPs (Böhringer, Carbone and Rutherford, 2018). As such, the remainder of the section will focus on OBPs.

Under a cap-and-trade system, free permits are allocated per unit of production based on an industry-specific emission intensity target. The equivalent rebate under a carbon tax would use revenue generated by the tax to provide a per unit-of-production subsidy based on the same industry-specific emission intensity target. By maintaining carbon pricing coverage, OBPs keep the marginal incentives to reduce GHG emissions, while limiting the incentive for firms to reduce GHGs through reduced production or relocation of plants. If a firm covered by the support policy wants to emit an additional tonne of GHG, it would either pay the tax or purchase a permit at the going carbon price. The support policy provides an implicit output subsidy for the firm to maintain or increase its production levels. The production incentive depends on the level of the firm's emission intensity compared to the emission intensity target.

The level of the industry-specific emission intensity target is key to determining which type of firm will be rewarded. If the level is too low, it will not provide incentives for more efficient production, as all firms will be rewarded with an output subsidy or free permits. Industry-specific emission intensity targets can be set at the "best available technology" or at the top quartile of performance within in an industry (Antweiler, 2016; Leach et al., 2015). Firms that meet or exceed that target are rewarded, whereas the laggards will have to pay more of the carbon tax or purchase additional permits. Also, periodic revisions of emissions intensity targets based on recent technological advances can provide incentives for continued innovations (Antweiler, 2016).

There are trade-offs to such government support. Namely, competitiveness support policies might result in smaller overall GHG reduction compared to a situation without them by keeping production higher in the compensated sectors. Also, governments providing such support to a sector run the risk of increased lobbying efforts by other sectors to receive compensation that might not be needed (Canada's Ecofiscal Commission, 2016).

In its 2015 Budget, BC adopted a temporary rebate program for its cement industry, the Cement Low Carbon Fuel Program. The program offers subsidies up to a total of $27 million to cement producers that meet or exceed emission intensity targets between 2015 to 2020. Having remained at $30 per tonne from 2012 to 2018, BC's carbon tax is set to increase by $5 per tonne a year to $50 per tonne in 2021. As such, BC's 2019 climate plan introduced a broader support policy for all industries, the CleanBC program. The program also subsidizes firms based on emissions intensities relative to "best available technology" benchmarks. While the specific details of both programs, such as the emission intensity benchmarks, are not publicly available, the principles of the programs appear to respect the above criteria.

2.5 Summary and concluding remarks

In this article, I investigate the impact of BC's carbon tax on cement trade. Results provide evidence that the policy reduced net exports. I find that the $30 per tonne carbon tax led to reductions of net exports between 13 to 18 percent as a share of production. Further results suggest that reductions in domestic production are driven by trade effects and not reductions in domestic consumption. This competitiveness effect provides some justification for a support policy by the BC government.

BC's government has put in place tax rebate policies for its cement sector; however key details, such as the level and path of its emission intensity benchmark are not known. The publication of such information is critical in determining their effects. Also, while the BC government is developing an industry-specific competitiveness support policy, it is still exempting process emissions for the cement industry and all sectors of its economy from its carbon tax. The implementation of its support policy to the cement industry and its recent extension to other industries that face substantial carbon competitiveness pressures is an opportunity for the province to include process emissions under its carbon pricing policy.

The limited availability of public data at the industry and provincial level was a challenge for this study. Further research attempting to uncover a pollution haven effect from carbon pricing in Canada would benefit from increased access to province-industry data on cost and energy use in addition to plant and firm-level micro data. Additionally, a related research agenda would be the empirical estimation of the effects of increased use of industry support policies by Canadian governments.

Chapter 3

Do carbon tariffs reduce carbon leakage? Evidence from trade tariffs

3.1 Introduction

After three decades of multilateral negotiations, countries have failed to deliver a binding and cooperative global solution for climate change mitigation. Global greenhouse gas (GHG) emissions continue to rise (NOAA, 2022), prompting some countries like those in the European Union to implement varying domestic policies to mitigate climate change, such as carbon trading markets. The UN Glasgow Climate Pact and the recent COP27 meeting are continuing to lay the foundations of an international carbon market to link domestic carbon markets and to ensure cost-effective emission reductions (Simmons et al., 2021).

One problem with this patchwork of country-level climate policies is carbon leakage, where emission reductions from regulated countries are offset by emission increases in unregulated countries. Carbon leakage undermines the global benefits of country-level or unilateral climate policies by countering their GHG reductions. As a response to potential carbon leakage, countries with prices on carbon are considering imposing carbon tariffs on countries without equivalent climate policies. Carbon tariffs price the carbon content of imports. Beyond reducing carbon leakage, Nordhaus (2015) demonstrates that targeted tariffs could also provide an incentive for unregulated countries to implement their own climate policy. This paper asks the following question: Do carbon tariffs reduce carbon leakage?

The implementation of country-level climate policies can lead to carbon leakage. Studies find that carbon emissions from unregulated countries can increase by 5 to 25% compared to the emissions reductions in the regulated country (Copeland, 2018). Aichele and Felbermayr (2015) empirically estimate that the Kyoto Protocol led to an 8% increase in imported carbon emissions. A proposed policy solution to reduce carbon leakage and incentivize climate policy adoption in unregulated countries is imposing carbon tariffs (Markusen, 1975; Drake, 2018; Nordhaus, 2015).

The European Union (EU) plans to implement carbon tariffs in 2026. In the US, carbon tariffs were part of the failed Waxman-Markey Bill, and President Biden has also considered pricing the embedded carbon in imports as part of his climate plan. However, carbon tariffs could have important unintended consequences. For example, a country's imposition of carbon tariffs on trading partners could further reshuffle carbon emissions. Proposed carbon tariffs also target upstream manufactured goods, such as steel, aluminum, cement, and paper. Therefore, these tariffs might also have unintended effects on the emissions from downstream polluting industries. Since no carbon tariffs are currently in place but are being proposed, the goal of this paper is to empirically estimate the effect of carbon tariffs on carbon leakage by looking at the emission change of facilities in industries facing increased export tariffs.

I first develop a two-country model to show that carbon tariffs can reduce carbon leakage. Since there are currently no carbon tariffs in place, I also use the conceptual model to show that trade tariffs from the recent trade war can be used to proxy the effect of carbon tariffs on carbon leakage. Even if current trade policy has been shown to be implicitly subsidizing the carbon content of traded goods (Shapiro, 2020), it is not evident that trade tariff variation can inform the emission effects of carbon tariffs.

Assuming that foreign and domestic goods are substitutes, the stylized model shows that trade tariffs provide a lower bound of the emission effect from a carbon tariff. This is because the carbon tariff reduces emission through both an emission intensity and scale effect, whereas the trade tariff only induces emission through a scale effect. In the case where carbon tariffs are imposed according to an industry-level average carbon intensity, the trade tariffs and carbon tariffs have the same emission reduction effect.

Empirically, I use increases in tariffs faced by US industrial facilities during the 2018-2019 trade war to proxy the effect of carbon tariffs on GHG emissions. I use difference-in-differences models to estimate the GHG emission response of US facilities to facing increases in export tariffs. Importantly, I account for the emission consequences of supply-chain linkages where emissions of downstream industrial facilities are affected by the use of the output of tariffed industries as an input. In terms of expected effects, export tariffs on the output of a facility restrict their foreign market access and hence should lead to emission reductions. The use of those products facing export tariffs as inputs will have a counteracting emission effect by lowering the cost of domestic inputs for those producers. Since US facilities were also simultaneously protected by import tariff measures applied to foreign trading partners, I also control for the emission effects of these import tariff increases.[1]

I use a difference-in-differences approach to estimate the effect of increases in export tariffs on US facility-level GHG emissions. I find evidence that export tariffs imposed on the output of US facilities reduced GHG emissions. I estimate that GHG emissions of US facility facing export tariff increases on their output fall by about 3% for each 1 percentage point (pp) increase in export tariffs. However, I also find that emissions increase for downstream facilities that use the tariffed exports. For each 1 pp increase in export tariff, facility emissions increase by 8%. Using these estimated semi-elasticities and the scale of upstream and downstream producers, I find that the emission reduction effect is more than offset by the emission increase from downstream users. Such results are consistent with other empirical studies on the 2018-2019 trade war and the North American Free Trade Agreement (NAFTA) finding evidence of large effect of tariffs

[1]By reducing import competition, import tariffs should increase emissions. Tariffs on the imports or exports of intermediate goods have a counteracting effect by raising or lowering the cost of producing inputs.

on the downstream users of inputs (Cherniwchan, 2017; Flaaen and Pierce, 2022).

Proposed carbon tariffs by the EU are incomplete as they are restricted to emissions from upstream products (Titievskaia, Simões and Dobreva, 2022). Results in this paper highlight the importance of considering the downstream emission effects of incomplete carbon tariffs. In the case of the set of industries covered by the 2018-2019 trade war, emission rebounds from downstream producers offset the emission reductions from producers targeted by the export tariffs.

I also conduct several robustness checks and estimate event-study models to test for differences in pre-trends. In order to explore potential heterogeneity driving this main result, I interact the tariff changes with industry measures of trade and GHG intensity. I find some evidence that facilities in trade-intensive industries reduced their emissions more in response to export tariffs, and that trade-intensive downstream users increased their emissions by less. This result is consistent with less trade-intensive downstream users benefiting more from the domestic reductions in the price of affected products. I also find some evidence of more important emission changes for upstream and downstream facilities in GHG-intensive industries.

I build on the empirical trade literature which uses tariff changes as quasi-experimental variation to study numerous outcomes. Researchers have estimated the effect of the 2018-2019 trade war tariff changes on US consumption, elections, employment, prices, and output (Amiti, Redding and Weinstein, 2019; Blanchard, Bown and Chor, 2019; Goswami, 2019; Waugh, 2019; Fajgelbaum et al., 2020; Flaaen and Pierce, 2022). More closely related to this study, studies have also leveraged tariff variation to empirically identify the effect of trade on the environment. Cherniwchan (2017) finds that trade liberalization under NAFTA lead to air pollution reduction in US manufacturing plants through access to cheaper inputs from Mexico. Bombardini and Li (2020) exploit changes in export tariffs faced by Chinese producers from 1982 to 2010 to study the effect of trade on pollution and mortality in China. I contribute to this literature by studying a new outcome of interest, namely greenhouse gas emissions.

There exists also a theoretical, numerical, and structural literature studying the effects of carbon tariffs. Studies have found that a large enough country (or group of countries) can reduce the foreign production of a polluting good through the use of an import tariff (Markusen, 1975; Fowlie, Reguant and Ryan, 2016*a*; Böhringer, Carbone and Rutherford, 2018; Hsiao, 2020)[2]. This study contributes to this literature chiefly by providing empirical evidence of the effect of carbon tariffs.

The rest of the paper has the following structure. Section 3.2 provides background on the 2018-2019 trade war and the proposed European carbon tariffs. Section 3.3 presents a conceptual framework to think about the emission effects of carbon tariffs and trade tariffs. Section 3.4 discusses the data. Section 3.5 presents the empirical framework. Section 3.6 presents and discusses the results. Section 3.7 concludes the paper. Appendix B.1, and B.2 offer additional figures and tables.

3.2 Background

The 2018-2019 trade war

Through 2018 and 2019, the US raised import tariffs and major trade partners retaliated with export tariffs. This trade war was characterized by new tariffs applied on thousands of traded goods and most increases ranged from 10 to 25 percentage point increases in ad valorem tariff rates (Fajgelbaum et al., 2020). Important industries affected by the trade war include the iron, steel, and aluminum manufacturing industries, as well as agriculture. Major retaliatory partners to the US import tariffs include China, the EU, Canada, Mexico, India, Russia, and Turkey. Retaliatory or export tariffs on US GHG reporting industries imposed by China, Canada, the EU, and Mexico affected respectively about 5%, 2%, and both 1% of the total value of US exports. Tariffs imposed by India, Russia, and Turkey account for less than 0.1% each. Tariffs between the US and NAFTA partners were lifted in 2019, and in 2021 for the EU. Import and export tariffs between the US and other trading partners remain in place.

[2] In the case of a group of countries, Hsiao (2020) highlight the importance of coordination and commitment between the countries' policy stringency and timeline to effectively reduce leakage.

The European Union's Carbon Border Adjustment Mechanism

In 2026, the EU will require importers (or exporters to the EU) to pay a carbon tariff for the embedded GHG emission content of their goods (Titievskaia, Simões and Dobreva, 2022). The policy is called the Carbon Border Adjustment Mechanism (CBAM). CBAM will take the form of importers purchasing GHG permits priced at the prevailing EU ETS price. CBAM will apply to a subset of upstream traded products: cement, electricity, fertilizers, iron, steel, and aluminum. The emissions of downstream products using these tariffed upstream products will not be covered. For example, the embedded GHG emissions in imported rolled steel would face a carbon tariff, but not the emissions embedded in the steel content of imported automobiles. To determine the carbon content of imports, the CBAM will have a hybrid structure. Importers can either report their verified emissions to pay the price of the actual embedded emissions in their products or pay based on a default emission intensity. Default values for each exporting country and each good will be set at an average emission intensity. I refer to the former type of carbon tariffs as facility-level carbon tariffs, and the latter as industry-level carbon tariffs.

Given that the 2018-2019 trade war covered many of the same industries targeted by the upcoming CBAM, namely the iron, steel, and aluminum manufacturing industries, learning the emission effect of the trade war on these industries could inform the effect of proposed carbon tariffs. However, trade tariffs should not have the same emission effect as carbon tariffs, since trade tariffs should not lead to emission abatement through emission intensity changes. As such, it is not obvious that one can use trade tariffs variation as a proxy for carbon tariff variation. The next section develops a stylized model to show that trade tariffs can provide a lower bound for the facility-level carbon tariff foreign emission effect and the same emission effect for industry-level carbon tariffs.

3.3 Theory

I turn to a simple two-country, two-good, partial equilibrium model to decompose the foreign emission effect from trade tariffs versus carbon tariffs. The model draws inspiration from Fischer and Fox (2012) and Böhringer, Fischer and Rosendahl (2014), who develop partial equilibrium models to study unilateral climate policies in international settings.

3.3.1 Set-up

Consider two countries, home and foreign, each with a perfectly competitive industry with a representative firm $i \in (H, F)$ that is a price taker in the input and output markets. Each firm produces a good with constant returns to scale and a unit cost function $c_i(r_i)$ where r_i is the reduction in emission intensity from their baseline intensity e_i^0. Production costs are rising in reductions in emission intensity. Each country's emissions are $E_i = (e_i^0 - r_i)Q_i$, where Q_i is output.

A representative consumer in each country determines home and foreign consumption for the two goods. Consumption of the good Q_H is composed of the home domestic consumption h and exports to foreign x, and consumption of the good Q_F is foreign domestic consumption f and home imports m. The consumer demand for each good is represented by a function of prices of the competing goods in each country: $h(p_h, p_m)$, $m(p_h, p_m)$, $x(p_x, p_f)$, and $f(p_x, p_f)$. I assume symmetric and constant elasticity of demand functions, for example, $h = \alpha p_h^{\eta_0} p_m^{\eta_1}$, where α is a demand shifter, $\eta_0 < 0$ an own-price elasticity, and $\eta_1 > 0$ a cross-price elasticity, meaning the goods are substitutes.

Market equilibrium is defined by $Q_H = h(p_h, p_m) + x(p_x, p_f)$ and $Q_F = f(p_x, p_f) + m(p_h, p_m)$. Also, let $r_i(\tau)$ reflect the cost-minimizing emission intensity at the carbon price τ. I assume that the home country's baseline emission rate e_H^0 includes a domestic carbon price. I plan to explicitly model the domestic carbon price in the near future.

I am interested in assessing changes in foreign emissions E_F under different trade policy measures, namely a facility-specific carbon tariff, an industry-level carbon tariff, and a trade tariff. Specifically, I want to compare the effect of the various policy interventions on the following changes in foreign emissions:

$$dE_f = \underbrace{-dr_F Q_F}_{\text{Technique effect}} + \underbrace{(e_F^0 - r_F)dQ_F}_{\text{Scale effect}} \tag{3.1}$$

Changes in foreign emissions in equation (3.1) can be split into a technique effect from changes in the emission intensity and a scale effect from changes in the production of the good.

3.3.2 Policy interventions

Below I compare changes in foreign emissions for different trade policy interventions. I compare foreign emission changes from carbon tariffs to the empirical context in my data, namely a trade tariff. I consider both the comparison of the effect of a trade tariff to a facility-level carbon tariff and an industry-level carbon tariff.

Trade tariff

Here I model the trade tariff as a specific tariff that directly taxes the foreign good imports at a rate of τ_t per unit. Home consumers of the foreign good now face price $p_m = c_F(e_F^0) + \tau_t$. In the absence of a change in the home carbon price, home prices are equal to the marginal production cost without additional emission intensity reductions, such that $p_h = p_x = c_H(e_H^0)$. Since there are also no incentives to cut foreign emission intensity, foreign consumers of the foreign good face price $p_f = c_F(e_F^0)$. I identify the trade tariff intervention with the superscript τ_t. The change in foreign emissions becomes:

$$\begin{aligned} dE_f^{\tau_t} &= e_F^0 dQ_F^{\tau_t} \\ &= e_F^0 \eta_0 \frac{dp_m}{p_m} < 0 \end{aligned} \tag{3.2}$$

where the first line comes from observing that $dr_F = 0$. The second line comes from substituting for $dQ_F = df + dm$ using the first-order approximations of the change in demand, which for example is $dm = \eta_0 \frac{dp_m}{pm} + \eta_1 \frac{dp_h}{p_h}$ for m. Since $e_F^0 > 0$, $\eta_0 < 0$ and $\frac{dp_m}{p_m} > 0$, the trade tariff reduces as expected foreign emissions, but only through a scale effect.

Facility-level carbon tariff

A facility-level carbon tariff τ on imports will give a direct incentive for the foreign producer to adjust their emission intensity. The CBAM carbon tariff proposed by the EU has a facility-level carbon tariff component. In the absence of a change in the home carbon price, home prices are equal to the marginal production cost without additional reductions, such that $p_h = p_x = c_H(e_H^0)$. Home consumers of the foreign good now face price $p_m = c_F(r_F^\tau) + \tau(e_F^0 - r_F^\tau)$. Home consumers of the foreign good pay for the embodied emissions and rising production costs. Because of the emission reduction incentives, the foreign goods producer sees its cost of production increase, $p_f = c_F(r_F^\tau)$. In evaluating the effect of the full policy change, I follow Fischer and Fox (2012) and assume $dr_F = r_F^\tau$. I identify this intervention with the superscript τ. The change in foreign emissions is:

$$\begin{aligned} dE_f^\tau &= -r_F^\tau Q_F^\tau + (e_F^0 - r_F^\tau) dQ_F^\tau \\ &= -r_F^\tau Q_F^\tau + (e_F^0 - r_F^\tau)\eta_0 \frac{dp_m}{p_m} < 0 \end{aligned} \tag{3.3}$$

where the change in foreign emissions is now driven by both a technique and a scale effect.

For the sake of comparison, if we assume the same unit cost of the trade and carbon tariff, such that $\tau_t = \tau(e_F^0 - r_F^\tau)$ such that the change in output is the same, $dQ_F^{\tau_t} = dQ_F^\tau$, then

$$dE_f^\tau - dE_f^{\tau_t} = -r_F^\tau(Q_F^\tau - dQ_F^\tau) \tag{3.4}$$

$$= -r_F^\tau(Q_F^\tau - \eta_0 \frac{dp_m}{p_m}) < 0 \tag{3.5}$$

where the change in emissions from a trade tariff of the same unit cost as a facility-level carbon tariff is a lower bound of the foreign emission reduction effect.

Industry-level carbon tariff

Industry-level carbon tariff taxes the emissions embodied in imports of the foreign good based on an industry average emission intensity denoted $\bar{e}_F$. The CBAM has an average emission intensity component, and other carbon tariffs are most likely going also have an industry-average component given the difficulty in attributing embodied emissions to individual importers. In the absence of a change in the home carbon price, home prices are equal to the marginal production cost without additional reductions, such that $p_h = p_x = c_H(e_H^0)$. Home consumers of the foreign good face price $p_m = c_F(e_F^0) + \tau\bar{e}_F$. Since there are no incentives to cut foreign emission intensity, foreign consumers of the foreign good face price $p_f = c_F(e_F^0)$. This also means that $r_F = dr_F = 0$. I identify the industry-level carbon tariff intervention with the superscript τ_I. The change in foreign emissions becomes:

$$dE_f^{\tau_I} = e_F^0 dQ_F^{\tau_I} < 0 \tag{3.6}$$

where foreign emission reduction follows directly from the trade tariff case.

For the sake of comparison, if we assume the same unit cost of the tariffs, namely $\tau_t = \bar{a}_F\tau_I$ such that $dQ_F^{\tau_t} = dQ_F^{\tau_I}$, then

$$dE_f^{\tau_t} - dE_f^{\tau_I} = 0 \tag{3.7}$$

and the foreign emission effect of the trade tariff has the same effect as the industry-level carbon tariff since both interventions only operate through a scale effect.

This stylized model highlights that the foreign emission effect of a trade tariff is either a lower bound of a facility-level carbon tariff or the same as an industry-level tariff. The model provides support for using the trade tariff increases during the trade war to learn about the emission effects of proposed carbon tariffs.

3.4 Data

Trade war tariff data

Commodity or product tariffs are used in the analysis to construct industry-level trade tariffs protected or faced by US industrial plants. While the variable of interest is the export tariffs, I still need to control for the increases in import tariffs. The yearly average ad-valorem trade war tariff increases for 2018 and 2019 between the US and retaliatory trading partners are taken from Fajgelbaum et al. (2020) and Fajgelbaum et al. (2021).

These data include the US tariffs waves on iron and steel, aluminum, China varieties, washing machines, and solar panel, and the retaliatory tariffs from China, Europe, Canada, Mexico, India, Russia, and Turkey. Non-manufacturing sectors, such as mining and agricultural production, were also targeted by the trade war. Similarly to other empirical papers studying the 2018-2019 trade war, I only consider trade war tariff increases as opposed to increases in the trade war tariffs on top of the baseline tariffs.

Trade data

Trade data are used to trade-weight the construction of the industry-level tariffs. I use 2010 to 2021 US Census trade data collected by Schott (2008) and Fajgelbaum et al. (2020). I only use the 2010 to 2017 data to construct the industry-level tariffs to avoid the impact of the change in tariffs may have had on industry-level trade.

Greenhouse gas data

The outcome variable of interest is industrial plant-level greenhouse gas emissions. I use US plant data to estimate the emission effect of tariff changes. Yearly plant-level GHG data measured in CO_2 equivalent (CO_2e) are obtained from the US EPA mandatory greenhouse gas reporting program (GHGRP). Variables provided include yearly CO_2e emissions, the six-digit North American Industrial Classification System (NAICS-6) code, geographic information, and parent firm information from 2010 to 2021. The US EPA mandates all facilities that emit more than 25,000 tonnes of CO_2e per year to report their emissions. The sample considered in this analysis includes about 5,500 reporting facilities per year, half of which are in the manufacturing sector.

Industry-level tariffs

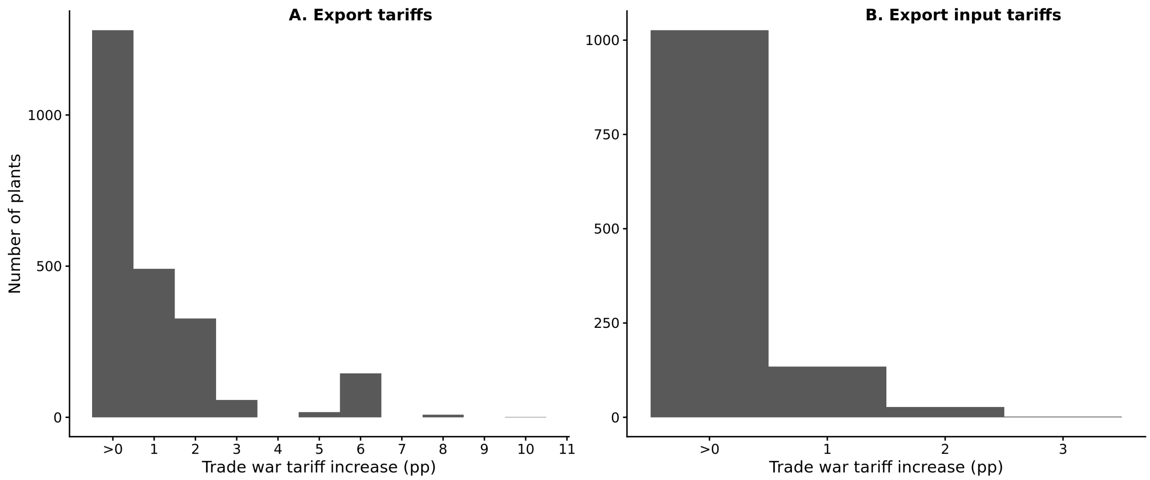
I construct industry-level tariffs using the commodity tariff data, and the trade data. Export tariffs are my main variable of interest, and I control for import tariffs. Ad-valorem tariffs and trade value between the US and its trading partners are first assigned to NAICS-6 codes using the concordance tables created byPierce and Schott (2012). I then aggregate the tariff data to the NAICS-6 level by taking a trade-weighted average of the tariffs using the average 2010-2017 trade values as weights. This procedure yields an export or import tariff at the NAICS-6 level, τ_{it}, for NAICS-6 i and year t. I then assign these tariff measures to each US manufacturing plant, p, based on the plants' NAICS-6 code i. There are more than 220 unique manufacturing NAICS-6 industries in the GHGRP. About three-quarters of them were targeted by the trade war.

Industry-level input tariffs

Since tariffs can affect plants not only through their output but their inputs, I follow several papers in the empirical trade literature and build input tariffs from the export and import tariffs (Cherniwchan, 2017; Goswami, 2019; Flaaen and Pierce, 2022). I construct industry-level input tariffs using the industry-level export and import tariffs calculated above and the 2012 US input-output (IO) table from the US Bureau of Economic Analysis. The *use* table of IO tables provides a dollar value of output use from industry j as input in industry i at the NAICS-6 level. For each NAICS code, I calculate the cost share of output use from other NAICS-6 industries. I then multiply the industry-level export and import tariffs of the use industry by the cost share and aggregate the input tariff to the NAICS-6 level.

Figure 3.1 shows the distribution of the number of facilities facing increases in new industry-level export tariffs between 2018 and 2019. Most GHG emitting plants facing tariff increases faced increases of less than 1 pp. A few hundred plants faced increases in output export tariffs of 2 pp or more, whereas a smaller number of facilities faced larger export tariff increases on their inputs. I exploit this intensity of tariff increases in my empirical strategy.[3]

Figure 3.1: Distribution of NAICS-6 level export tariff increases facing U.S. GHG emitting facilities



Notes: Figure 3.1 shows the distribution of GHG emitting facilities facing increases in export tariffs on their output or input during the 2018-2019 trade war.

Table 3.1 shows various summary statistics of NAICS-6 level export tariffs increase and facility GHG emissions at the NAICS-2 level. The table highlights the downstream impacts of tariffs for industries not directly targeted by the trade war export tariffs. Table Tables A1 and A2 respectively show the export and import tariffs and further decomposed summary statistics of the NAICS-6 level industry tariffs at the NAICS-3 level for the manufacturing sector. They show that most facilities within the same industries often face both export and import tariff increases on their output or inputs, and hence the importance to account for all tariff changes.

[3] Figure A1 in Appendix A.3 shows the distribution of GHG emitting US manufacturing plants for the increases in output and input import tariffs.

Table 3.1: NAICS-2 industry variation in trade war export tariffs increases and greenhouse gas emissions

		Tariff increases (pp)						
		Export		Export input		CO2e (kt)		
Sector	NAICS-2	Mean	Std. dev.	Mean	Std. dev.	Mean	Std. dev.	# plants
Agriculture	11	1.4	3.08	0.03	0.08	48	17	7
Mining	21	0.7	1.56	0.03	0.05	175	331	1275
Water and sewage	22	0	0	0.01	0.01	114	337	260
Food and textile	31	0.7	1.36	0.09	0.31	107	330	370
Petroleum, chemical and wood	32	0.48	0.9	0.08	0.12	459	868	1454
Primary and secondary metal	33	0.58	0.96	0.3	0.5	205	806	468
Wholesale	42	0	0	0.01	0.01	22	24	4
Warehousing	49	0	0	0.02	0.02	52	65	8
Buidlings	53	0	0	0.02	0.02	66	31	5
Research and development	54	0	0	0.01	0.01	38	23	19

Notes: pp = percentage point. kt = kiloton. Std. dev. = Standard deviation.

3.5 Empirical framework

To estimate the effect of export tariff increases on changes in US industrial plant emissions, I use difference-in-differences (DiD) models and event-study (ES) models. The unit of observation is at the facility-industry-year level, the outcome variable yearly greenhouse gas emissions, and the treatment variable NAICS-6 industry-level tariff increases from the 2018-2019 trade war. In order to compare facilities in treated industries to facilities in comparable control industries, samples are restricted to treated NAICS-3 industries or to the manufacturing sector.

Before studying the effect of export tariffs on facility-level GHG emissions, I first look into the effect of the constructed NAICS-6 industry-level tariffs on industry-level net export value. For example, industry-level export tariff increases should be correlated to decreases in the corresponding net export values through restricted foreign market access. In contrast, downstream exposure to export tariff increases should be related to increased net exports through lowered input costs.

I run the following DiD model:

$$y_{it} = \beta_1 \Delta\tau_i^O \times Post_t + \beta_2 \Delta\tau_i^I \times Post_t + X_{it}\theta + \mu_i + \eta_t + \phi_{nt} + \epsilon_{it} \tag{3.8}$$

where y_{it} is net exports in millions of US dollars at the NAICS-6 industry i and year t level. $\Delta\tau_i^O$ is the averaged 2018-2019 export tariff increase faced by NAICS-6 industry i on its output, and $\Delta\tau_i^I$ is the exposure to the export tariff increase through its input use. $Post_t$ is equal to one after 2017 once the trade war began. β_1 and β_2 are respectively semi-elasticities that identify the percent change in net export value from a 1 pp increase in either the output or input export tariff. X_{it} are control variables for the industry exposure to output and input import tariff increases during the trade war. Not controlling for the output and input import tariff increases could introduce omitted variable bias, as they are correlated with the export tariff increases as evident from Table A1. NAICS-6 fixed effects are accounted for by μ_i, and η_t are year-fixed effects. To account for industrial sector shocks, ϕ_{nt} are two-digit NAICS-by-year, nt, fixed effects. Standard errors are clustered at the NAICS-6 digit level. There are over 500 different NAICS-6 treated industries in the industry-level trade value sample.

After estimating the effect of the industry-level tariff increases on industry-level trade, the main estimating strategy explores the effect of the export tariff increases on facility-level GHG emission changes. Specifically, I estimate the following DiD model:

$$\ln(\mathrm{CO_2e}_{pit}) = \delta_1 \Delta\tau_i^O \times Post_t + \delta_2 \Delta\tau_i^I \times Post_t + X_{it}\Theta + \psi_p + \omega_{st} + \rho_{nt} + \varepsilon_{pit} \tag{3.9}$$

where $\mathrm{CO_2e}_{pit}$ are greenhouse gas emissions measured in $\mathrm{CO_2e}$ for a US industrial plant p, in NAICS-6 industry i and year t. $\Delta\tau_i^O$ is the averaged 2018-2019 export tariff increase faced by NAICS-6 industry i on its output, and $\Delta\tau_i^I$ is the exposure to the export tariff increase through its input use. δ_1 and δ_2 are semi-elasticities interpreted as the percentage change in GHG emissions from a 1 pp increase in output or input export tariff exposure. X_{it} are control variables for the industry exposure to output and input import tariff increases during the trade war. ψ_p, ω_{st}, and ρ_{nt} are respectively plant and state-by-year, st, and two-digit NAICS-by-year, nt, fixed effects; and ε_{pit} an error term. $\mathbf{1}(\Delta\tau_{it} > 0)$ is a cross-sectional variable that is equal to one if the NAICS-6 industry was ever targeted by an export or import tariff increase during 2018-2019. Standard errors are clustered at the level of treatment variation, namely at the NAICS-6 level. There are over 225 different NAICS-6 treated industries in the facility-level GHG sample.

The plant-fixed effects account for time-invariant differences between industrial facilities, such as emission intensity, and size. The state-by-year fixed effect control for important changes in state-level environmental policy changes, such as California's cap-and-trade system, and differences COVID-19 stay-at-home policies. The varying composition of NAICS-6 industry plants within each state allows me to use state-by-year fixed effects. The two-digit NAICS-by-year fixed effects capture yearly changes in the broad categories of industrial sectors, such as the food and textile manufacturing sector (NAICS-2 31), the wood, chemical, and non-metallic manufacturing sector (NAICS-2 32), and the automotive, machinery, and metals manufacturing sector (NAICS-2 33).

More formally, in order to interpret δ_1 and δ_2 as causal, I test for differences in pre-trends by running an event-study version of equation (3.9). Specifically, I interact the tariff increases with year dummies. I omit the year 2017, in order to interpret the interacted coefficients as differences in emission changes relative to the year before the trade war began.

3.6 Results

This section presents estimates of the effect of export tariff increases on industry-level trade and on US industrial plant GHG emissions using equations (3.8) and (3.9). I first discuss the overall difference-and-differences estimates. I then test for common pre-trends and finally interact my export tariff variable with measures of emission and trade intensity to explore potential heterogeneity of response.

3.6.1 Export tariff effects on industry-level net exports

Table 3.2 presents the estimates of the effect of the output and input export tariffs exposure on industry-level net exports. The first two columns restrict the control group to facilities in the same NAICS-3 digit industries that include NAICS-6 treated industries. The last two columns restrict the sample instead to the manufacturing sector. For each sample, the first columns only include industry and year-fixed effects, and the second column further includes NAICS-2 by year fixed effects to account for sector-specific shocks. The estimates show that net exports decrease for industries for which their output was targeted by export tariff increases. This reduction in net exports is consistent with these industries facing restrictions in foreign market access. Specifically, a 1 pp increase in export tariff on output is related to a nearly 300 million reduction in net exports for the average manufacturing industry. The effect is smaller and imprecise for the set of treated NAICS-3 industries.

Net exports increase for industries exposed to export tariff increases on their inputs. This effect is consistent with the notion that domestic reductions in prices of affected output by export tariffs can be beneficial for US buyers of these products. Estimates suggest that a 1 pp increase in export tariffs on domestic products used for inputs increases net export value for industrial sectors by more than 1 billion. These estimates suggest that downstream industries are more responsive to upstream export tariff increases than the targeted upstream industries.

Table 3.2 provides evidence of the relevance of the NAICS-6 industry-level tariffs. These estimates also suggest that downstream industries are more responsive to upstream export tariff increases than the targeted upstream industries. Table A3 presents a consistent story for net export effects for industries exposed to the output or input import tariff increases.

Table 3.2: Semi-elasticity of the trade war export tariffs on industry-level US net exports

	Net exports (mil $)			
	(1)	(2)	(3)	(4)
Δ Export tariff X Post	−11.654	−85.059	−274.231*	−291.702**
	(74.210)	(74.234)	(143.325)	(142.274)
Δ Export input tariff X Post	1,600.084*	1,462.659*	2,462.056**	1,993.156**
	(952.152)	(858.666)	(991.488)	(916.822)
Adj. R2	0.89	0.89	0.94	0.94
Sample	Treated NAICS-3	Treated NAICS-3	Manufacturing	Manufacturing
NAICS-2 X Year	×	✓	×	✓
Observations	4,715	4,715	4,168	4,168

Notes: Estimates of the emission semi-elasticity of the trade war export tariffs on NAICS-6 level net exports in millions of USD. All models include year fixed effects and NAICS-6 fixed effects. Facilities are restricted to the treated NAICS-3 industries or the manufacturing sector. Column 1 and Column 3 are the baseline models for each sample. Column 2 and 4 further include NAICS-2 by year fixed effects. Robust standard errors clustered at NAICS-6 level in parentheses.

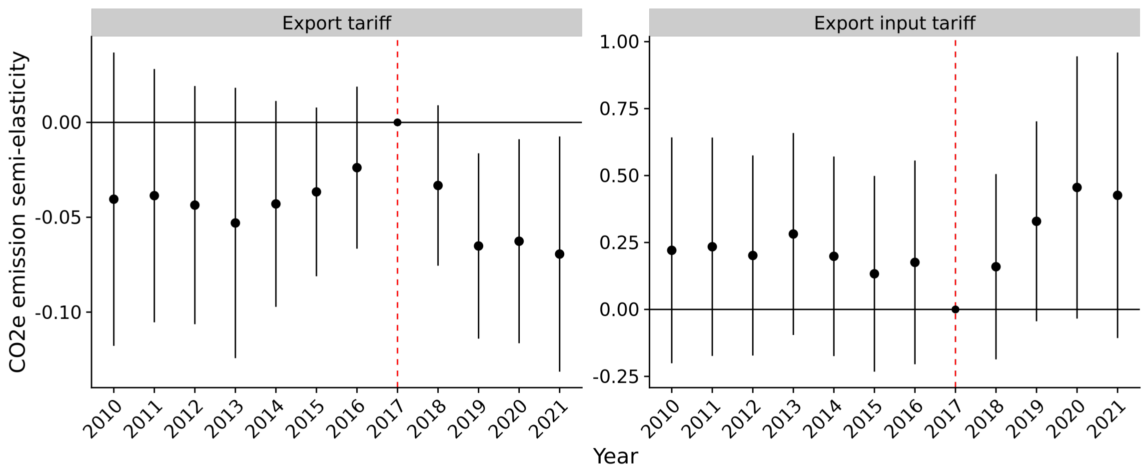
3.6.2 Export tariff effects on facility-level GHG emissions

I now turn to my main estimating strategy, namely the effect of the export tariff increase on industrial facility-level GHG emission changes. I first estimate the event-study version of equation (3.9) to compare changes in GHG emissions for facilities in NAICS-6 industries that faced export tariff increases to similar facilities in NAICS-6 industries that were not targeted by the trade war tariff changes. This allows me to visually test for pre-trends and the effect of the export tariff increases.

We would expect manufacturing facilities facing increases in export tariffs on their output to reduce their emissions, and facilities that use these outputs for their production to increase their emissions. These effects are consistent with the economic intuition of reduction in output from reduced access to foreign markets and reduced domestic prices for the use of those output as inputs.

Figure 3.2 shows the percentage change in emissions for facilities facing increases in export tariffs on their outputs or on their inputs during the trade war. Relative to 2017, the coefficients for years before the trade war for both panels are not statistically distinguishable from zero. For facilities facing export tariffs increase on their output, there is a decrease in emissions after the export tariff increases. For facilities exposed to export tariffs on their inputs, their emission increased after the beginning of the trade war. The event-study graphs for facilities facing output and input import tariffs are shown in Figure A2. Results are qualitatively the same for the sample restricted to the manufacturing sector as shown in Figure A3.

Figure 3.2: Event-study model of the effect of trade war export tariffs on CO2e emissions



Notes: Point estimates and 95% confidence intervals of the semi-elasticity effect of output and input export tariffs on log CO_2e emissions relative to 2017 using an event study version of equation (3.9). Estimates for the sample restricted to NAICS-3 treated industries are shown. Standard errors are clustered at the NAICS-6 level.

Figure 3.2 provides evidence of a decrease in GHG emissions for facilities facing an increase in export tariffs, but however offers a cautionary tale of a potential rebound of emissions from input users downstream. Table 3.3 presents the semi-elasticities of the export tariffs across different sets of fixed effects using equation (3.9). The first column only includes plant and year fixed effects, columns 2 and 3 respectively add state-by-year, and two-digit NAICS-by-year fixed effects to account for more unobserved shocks correlated with the trade war. Estimates from equation (3.9) in column 3 suggest that a 1 pp increase in export tariff on output reduces industrial facility emissions by 2%. A 1 pp increase in inputs used for production downstream is estimated to increase emissions by 16%. However, in the sample considered, a 1 pp increase in export tariffs in output corresponds to a 0.4 pp increase in exposure for downstream industries. Therefore, it is more reasonable to compare the upstream effect to a 1 pp export tariff increase targeted output, which leads to a 6% increase in downstream emission. This larger responsiveness of downstream producers is consistent with previous estimates shown above, and other studies that have found larger responses for tariff exposures on inputs (Cherniwchan, 2017; Flaaen and Pierce, 2022). Qualitatively similar results are found if instead the dependent variable is in levels, or if the sample is restricted to the manufacturing sector as shown respectively in Table A5 and A6. Table A4 also shows a greater responsiveness of downstream input exposure for import tariff increases relative to output import tariff exposure.

Table 3.3: Semi-elasticity of the trade war export tariffs on facility-level CO2e emissions

	ln(CO2e)		
	(1)	(2)	(3)
Δ Export tariff	-0.008	-0.019	-0.023^{*}
	(0.018)	(0.012)	(0.012)
Δ Export input tariff	0.154	0.191^{*}	0.163
	(0.110)	(0.104)	(0.103)
Adj. R2	0.85	0.85	0.85
State X Year	×	✓	✓
NAICS-2 X Year	×	×	✓
Observations	46,875	46,875	46,875

Notes: Estimates of the emission semi-elasticity of the trade war export tariffs. All models include year fixed effects and plant fixed effects. The control group is restricted to facilities in the same NAICS-3 industries as the treated facilities. Column 1 is the baseline model. Column 2 further includes state by year fixed effects. Column 3 additional controls for NAICS-2 by year fixed effects. Robust standard errors clustered at NAICS-6 level in parentheses.

Net emission effect of export tariffs

While the estimates from equation (3.9) in Table 3.3 show a greater responsiveness of downstream industries to upstream carbon tariffs, the estimates do not directly say whether their emission increase offset the emission decrease of the targeted industries. In order to determine the net emission effect from a 1 pp export tariff increase, three other ingredients are needed besides the semi-elasticity estimates from Table A4, namely the size of GHG emissions in upstream and downstream emissions, and how much a 1 pp increase in export tariffs on output translated to downstream tariff exposure through input-output linkages. In the context of the 2018-2019 trade war, the respective GHG emission size of upstream industries and downstream industries affected by the export tariffs on outputs is of 808 and 584 Mt. For 1 pp increase in export tariffs on outputs, the average exposure for downstream input users was of 0.42 pp. Combining all these numbers, a 1 pp increase in export tariff reduced upstream emissions by 19 Mt, but however increase downstream emissions by 20 Mt. The net effect in this case is an emission increase of 22 Mt.

This back-of-the-envelope calculation highlights a potential issue with incomplete carbon tariffs applied to upstream products, such as the proposed EU CBAM policy. If downstream emission rebound offsets the direct reductions in upstream emissions, then carbon tariffs could increase foreign GHG emissions. A limitation of this exercise is that the export tariffs facing US industrial facilities during the 2018-2019 trade war are not fully representative of the covered sectors under CBAM.

Heterogeneity

I now turn to heterogeneity analyses of the export tariff effects on two dimensions: industry-level trade intensity and GHG emission intensity. We expect trade and carbon tariffs to affect more tradable industries. While we also expect more carbon-intensive industries to respond more to carbon tariffs, it is unclear whether they should respond more or less to trade tariffs.

To measure trade intensity, I calculate for each NAICS-6 industry the ratio of total trade value (import + export) to total value of sales from the NBER-CES manufacturing database (Becker, Gray and Marvakov, 2021). I average the share of trade over the pre-trade war period of 2010-2017. Low values of the ratio implies low trade intensity, whereas trade-intensive industries have a higher value of the ratio. To explore the effect of trade intensity, I interact the export tariff variables in equation (3.9) with a dummy variable equal to 1 if the NAICS-6 treated industries has a higher share of trade than the median NAICS-6 treated industry. A value of 1 for the trade intensity dummy indicates a trade-intensive industry.

Table 3.4 presents estimates of equation (3.9) by trade intensity. While noisy, the results show that emissions for facilities in more trade intensive industries reduce more as a response to export tariffs affecting their output. The opposite is true for trade-intensive industries exposed to export tariffs on their inputs, they increase their emissions less. One explanation is that the trade-intensive downstream sectors benefit less from the reduction in the domestic price of the input than the less trade-intensive downstream industries more reliant on domestic inputs.

Table 3.4: Semi-elasticity of the trade war tariffs on facility-level CO2e emissions by trade intensity

	ln(CO2e)		
	(1)	(2)	(3)
Δ Export tariff	0.012	−0.008	−0.007
	(0.022)	(0.020)	(0.021)
Δ Import tariff	−0.011	0.002	0.011
	(0.012)	(0.013)	(0.015)
Δ Export input tariff	0.140	0.225*	0.253*
	(0.138)	(0.134)	(0.139)
Δ Import input tariff	0.110	0.022	−0.038
	(0.246)	(0.243)	(0.243)
Δ Export tariff X Trade intensity	−0.044*	−0.021	−0.020
	(0.026)	(0.023)	(0.024)
Δ Import tariff X Trade intensity	0.016	0.004	−0.003
	(0.014)	(0.015)	(0.017)
Δ Export input tariff X Trade intensity	−0.038	−0.155	−0.224
	(0.152)	(0.146)	(0.155)
Δ Import input tariff X Trade intensity	−0.182	−0.084	0.018
	(0.244)	(0.239)	(0.244)
Adj. R2	0.87	0.87	0.87
State X Year	×	✓	✓
NAICS-2 X Year	×	×	✓
Observations	27,514	27,514	27,514

Notes: Estimates of the emission semi-elasticity of the trade war tariffs by trade intensity. Trade intensity is a dummy variable equal to 1 if the NAICS-6 industries trade value as a share of sales is greater than the median value. All models include year fixed effects and plant fixed effects. Facilities are restricted to the manufacturing sector. Column 1 is the baseline model. Column 2 further includes state by year fixed effects. Column 3 additional controls for NAICS-2 by year fixed effects. Robust standard errors clustered at NAICS-6 level in parentheses.

The GHG emission intensity measure is the average 2010-2017 NAICS-6 industry total US emission from the GHGRP over total value of sales for the same industry from the NBER-CES manufacturing database (Becker, Gray and Marvakov, 2021). Similar to the trade intensity estimates, I interact the export tariff variables in equation (3.9) with a GHG intensity dummy variable equal to 1 if the NAICS-6 treated industries' GHG emission per sales is greater than the median value for treated industries.

Table 3.5 shows the estimates by GHG emission intensity. The interacted results suggest that the emissions from plants in more emission-intensive industries generally respond more stronlgy to export tariff increases on their inputs or outputs. While mostly imprecise, the interacted coefficients on the preferred specification in column 3 show greater increase for facilities in industries targetted by export tariffs on their output, and greater increases in emissions from downstream facilities.

Table 3.5: Semi-elasticity of the trade war tariffs on facility-level CO2e emissions by GHG intensity

	ln(CO2e)		
	(1)	(2)	(3)
Δ Export tariff	−0.016	−0.016	−0.007
	(0.021)	(0.017)	(0.018)
Δ Import tariff	0.002	0.007	0.014
	(0.010)	(0.009)	(0.010)
Δ Export input tariff	0.161	0.159	0.107
	(0.109)	(0.105)	(0.112)
Δ Import input tariff	−0.070	−0.064	0.010
	(0.063)	(0.065)	(0.077)
Δ Export tariff X GHG intensity	0.014	−0.013	−0.030
	(0.038)	(0.024)	(0.024)
Δ Import tariff X GHG intensity	0.033	0.036	0.028
	(0.039)	(0.039)	(0.038)
Δ Export input tariff X GHG intensity	0.827	0.757	0.840*
	(0.578)	(0.513)	(0.447)
Δ Import input tariff X GHG intensity	−0.749	−0.672	−0.693
	(0.461)	(0.468)	(0.440)
Adj. R2	0.87	0.87	0.87
State X Year	×	✓	✓
NAICS-2 X Year	×	×	✓
Observations	27,514	27,514	27,514

Notes: Estimates of the emission semi-elasticity of the trade war tariffs by greenhouse gas (GHG) intensity. GHG intensity is a dummy variable equal to 1 if the NAICS-6 industries GHG emissions per sales is greater than the median value. All models include year fixed effects and plant fixed effects. Facilities are restricted to the manufacturing sector. Column 1 is the baseline model. Column 2 further includes state by year fixed effects. Column 3 additional controls for NAICS-2 by year fixed effects. Robust standard errors clustered at NAICS-6 level in parentheses.

Interacting the main DiD model with measures of trade and GHG intensity suggests that the emission response from the trade war export tariff increases have different effects. For trade intensity, the relative effect of emission changes for facilities in upstream and downstream industries goes in the opposite effect, whereas they move in the same direction for facilities in GHG-intensive industries. Since many important emitters were exempt from the trade war, such as products from pulp and paper mills and cement manufacturers, the importance of their trade and GHG-intensity would affect the importance of the offsetting of emissions from downstream sectors.

3.7 Conclusion

In this paper, I attempt to predict the effect of proposed carbon tariffs on foreign emission changes for industrial GHG emitting facilities. Using a two-country partial equilibrium model, I first show that observable trade tariffs can be used to estimate the upstream effect of unobservable carbon tariffs on foreign GHGs. Assuming that foreign and domestic goods are substitutes, variation in trade tariffs provides a lower bound of the emission effect of carbon tariffs.

Empirically, I exploit changes in trade tariffs during the 2018-2019 trade war to estimate the net emission effect from export tariff increases for US industrial facilities. While controlling for other tariff changes, I find evidence that US emitting facilities respond to export tariff increases targeting their output by reducing their emissions. However, results also highlight that downstream facilities that use the targeted output as input respond by increasing their emissions. This rebound effect from downstream emissions offsets the upstream emission reductions in the case of the 2018-2019 trade war.

The offsetting emission effect from downstream facilities highlights a potential issue with incomplete carbon tariffs applied to upstream products, such as the proposed EU CBAM policy. The EU CBAM will only cover five product categories: cement, iron and steel, aluminum, fertilizers, and electricity. This paper highlights the importance of considering input-output linkages for the net emission effect of incomplete carbon tariffs. Specifically, if uncovered downstream producers are large emitters, then their emission increases as a response to upstream carbon tariffs could offset the upstream emission reductions. As discussed in Titievskaia, Simões and Dobreva (2022), the EU Commission is aware of the potential emission reshuffling risks of downstream producers not currently considered under their CBAM, and is planning to re-evaluate in the coming years the inclusion of downstream products. Results in this paper suggest focusing on covering products of downstream producers that are large emitters.

A limitation of this paper is that the export tariffs facing US industrial facilities during the 2018-2019 trade war are not fully representative of the covered sectors under CBAM. The trade war tariff increases mostly affected the steel, iron, and aluminum sectors. Further research should study the net emission response to export tariffs of other covered sectors by the EU CBAM, namely the cement, fertilizers, and electricity sectors.

Appendix A

Appendix to Chapter 1

A.1 Theory appendix

A.1.1 Proposition 1

This appendix section proves Proposition 1. Recall eq. (1.6)

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

To establish Proposition 1, it is sufficient to demonstrate that with $Z_s = \frac{\rho_s}{\mu_s}$, that $dZ_s/d\mu_s > 0$. When $\widetilde{\theta} = \frac{\mu_m}{\mu_b} < 1$ or $\mu_m - \mu_b < 0$, having $Z_m - Z_b < 0$ implies $\theta < \widetilde{\theta}$ and thus $\widetilde{\theta}$ is a lower bound on the true allocative efficiency gain θ. Conversely, when $\widetilde{\theta} > 1$ or $\mu_m - \mu_b > 0$, having $Z_m - Z_b > 0$ implies $\theta > \widetilde{\theta}$ and so $\widetilde{\theta}$ is a lower bound on the true allocative efficiency loss θ.

Below, we establish $dZ_s/d\mu_s > 0$ for two functional forms $f()$ relating distortion ϕ_{is} to abatement share a_{is} under the assumption that $\phi_{is} \sim \mathcal{LN}(0, \sigma_s^2)$, with population mean $\mu_s = e^{\frac{\sigma_s^2}{2}} > 1$.

Linear function Let $a_{is} = \alpha\phi_{is}$, where α is the linear multiplier parameter. Under $\phi_{is} \sim \mathcal{LN}(0, \sigma_s^2)$, we have

$$\frac{dZ_s}{d\mu_s} = \alpha(3\mu_s^2 - 1)$$

Since $\mu_s > 1$, $\frac{dZ}{d\mu_s} > 0$ when $\alpha > 0$, or when $f()$ is an increasing linear function of ϕ_{is}.

Power function Let $a_{is} = \phi_{is}^p$, where p is the power parameter. Under $\phi_{is} \sim \mathcal{LN}(0, \sigma_s^2)$, we have

$$\frac{dZ_s}{d\mu_s} = p^2\mu_s^{(p^2-1)}\left[\mu_s^{2p} - 1\right] + 2p\mu_s^{(p+1)^2-2}$$

Since $\mu_s > 1$, $\frac{dZ}{d\mu_s} > 0$ if $p > 0$, or when $f()$ is an increasing power function of ϕ_{is}.

A.1.2 Semi-parametric recovery of θ

To see how θ can be recovered with data without the parametric assumptions on $f()$ in Proposition 1, we first expand eq. (1.6)

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

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

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

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

A.2 Data appendix

A.2.1 Record linkage procedure

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

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

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

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

Table A.1 and A.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 A.1: Potential match candidate from hypothetical data 1

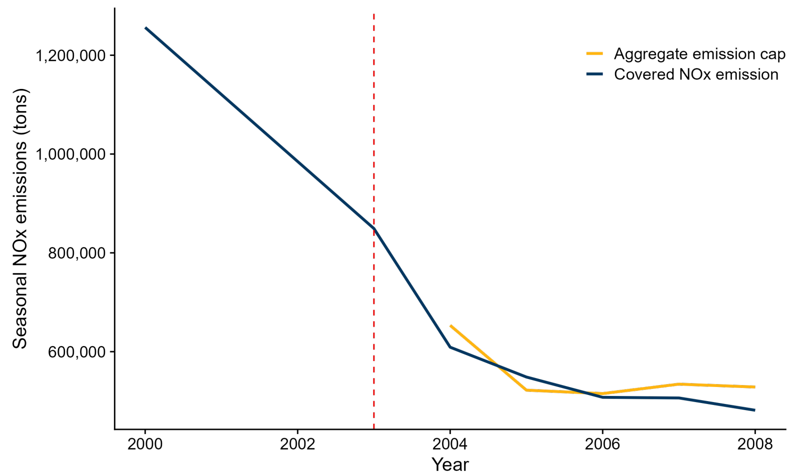
unique ID data 1	Year	Plant name	NAICS (3-digit)
plant_1	2000	GOLETA REFINERY	324
plant_1	2001	GOLETA REFINERY	
plant_1	2002	COASTAL PETROLEUM	324

Table A.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

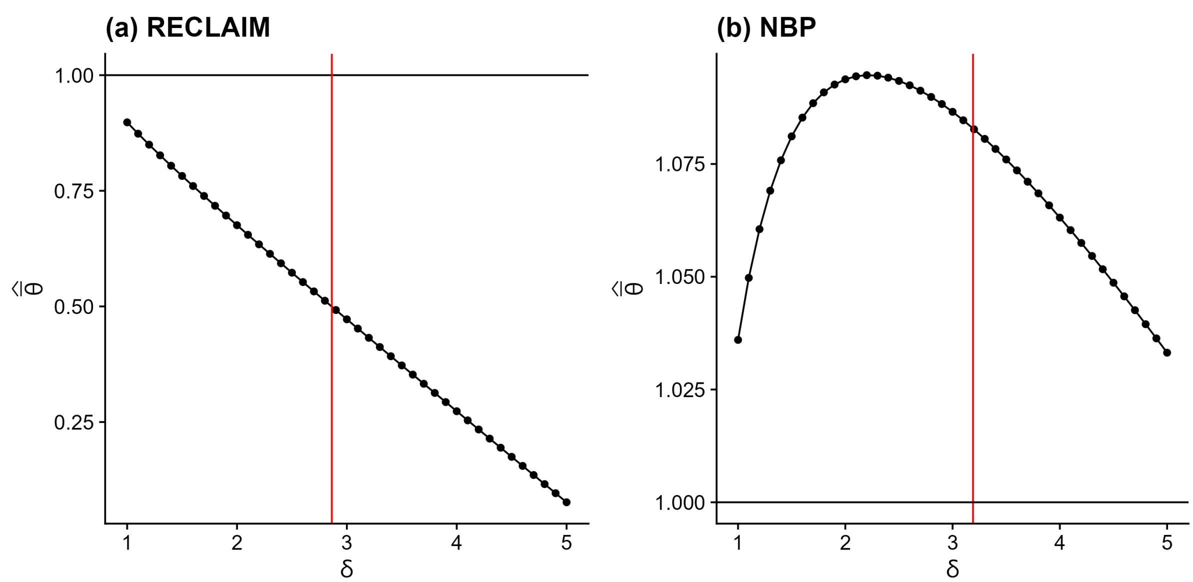
A.3 Figure appendix

Figure A1: NBP NO_x emissions and cap



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

Figure A2: Semi-parametric allocative efficiency effects by policy



Notes: Semi-parametric measure of allocative efficiency change $\widehat{\overline{\theta}}$ from eq. 1.11 by policy and range of abatement ratio across policies (δ).

A.4 Table appendix

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

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

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

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

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

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

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

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

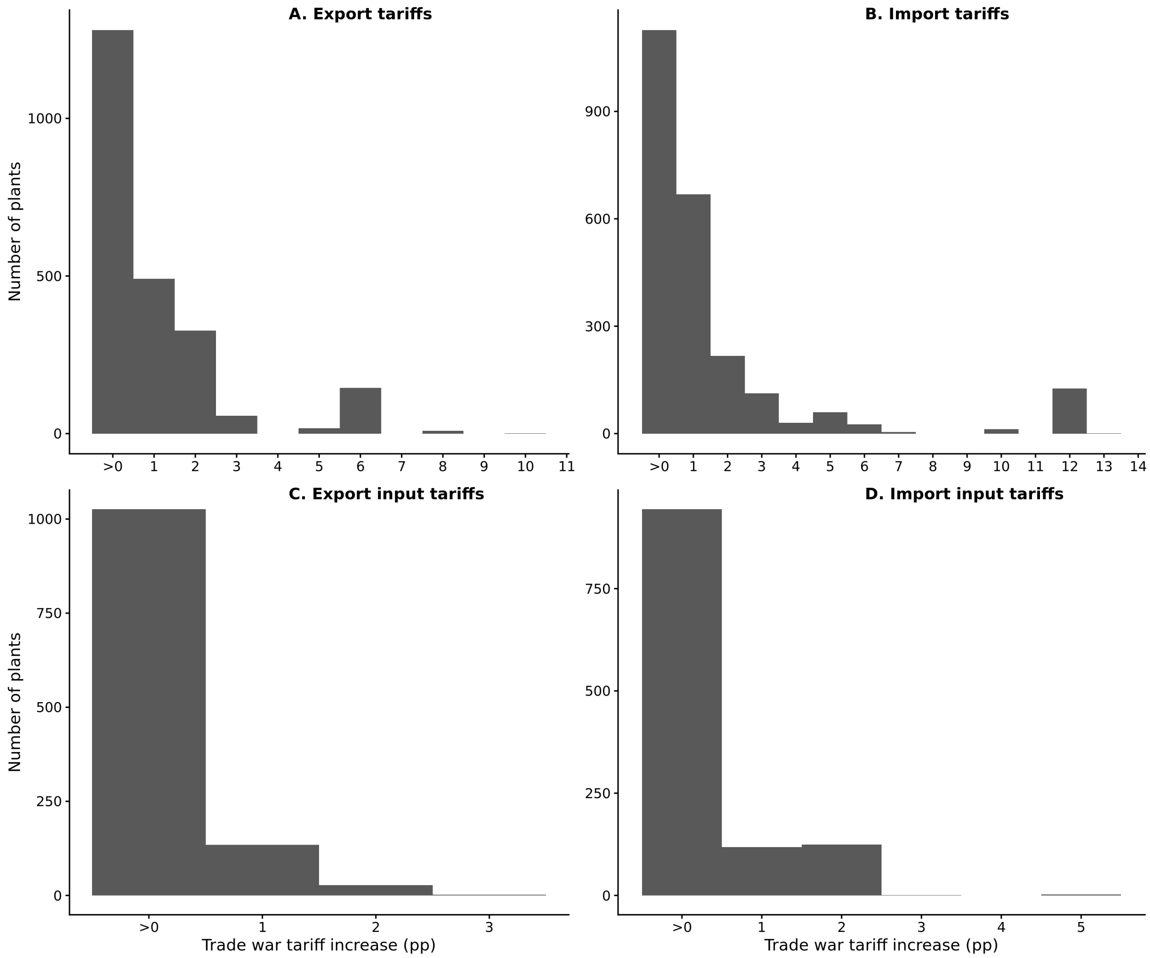
Notes: Post is a dummy variable equal to one for the years after 2002, NBP is a dummy equal to one if a U.S. manufacturing plant is covered by NBP. California plants are excluded because of the confounding of RECLAIM. Numbers are rounded based on the U.S. Census Bureau's rounding rules (U.S. Census Bureau,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

Appendix B

Appendix to Chapter 3

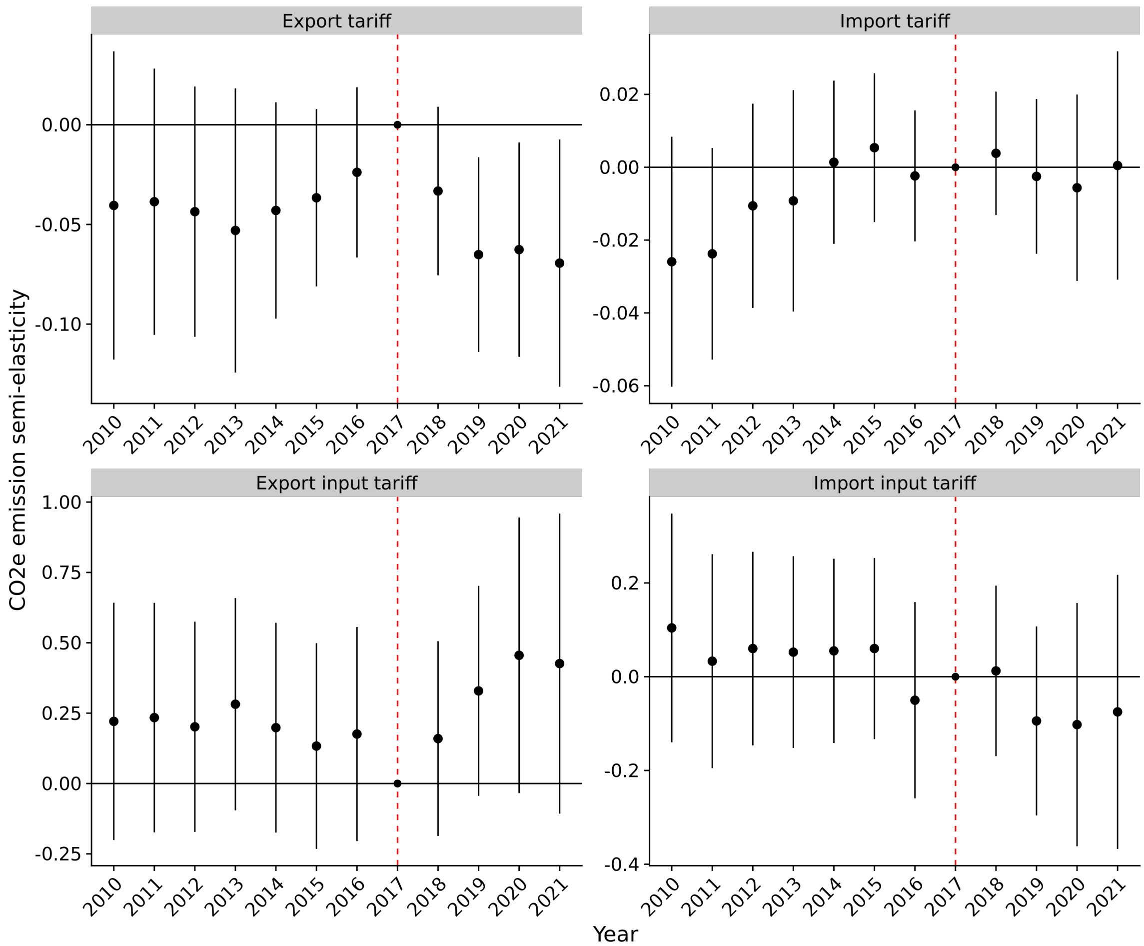
B.1 Figure appendix

Figure A1: Distribution of NAICS-6 level trade-war tariff increases facing U.S. GHG emitting facilities



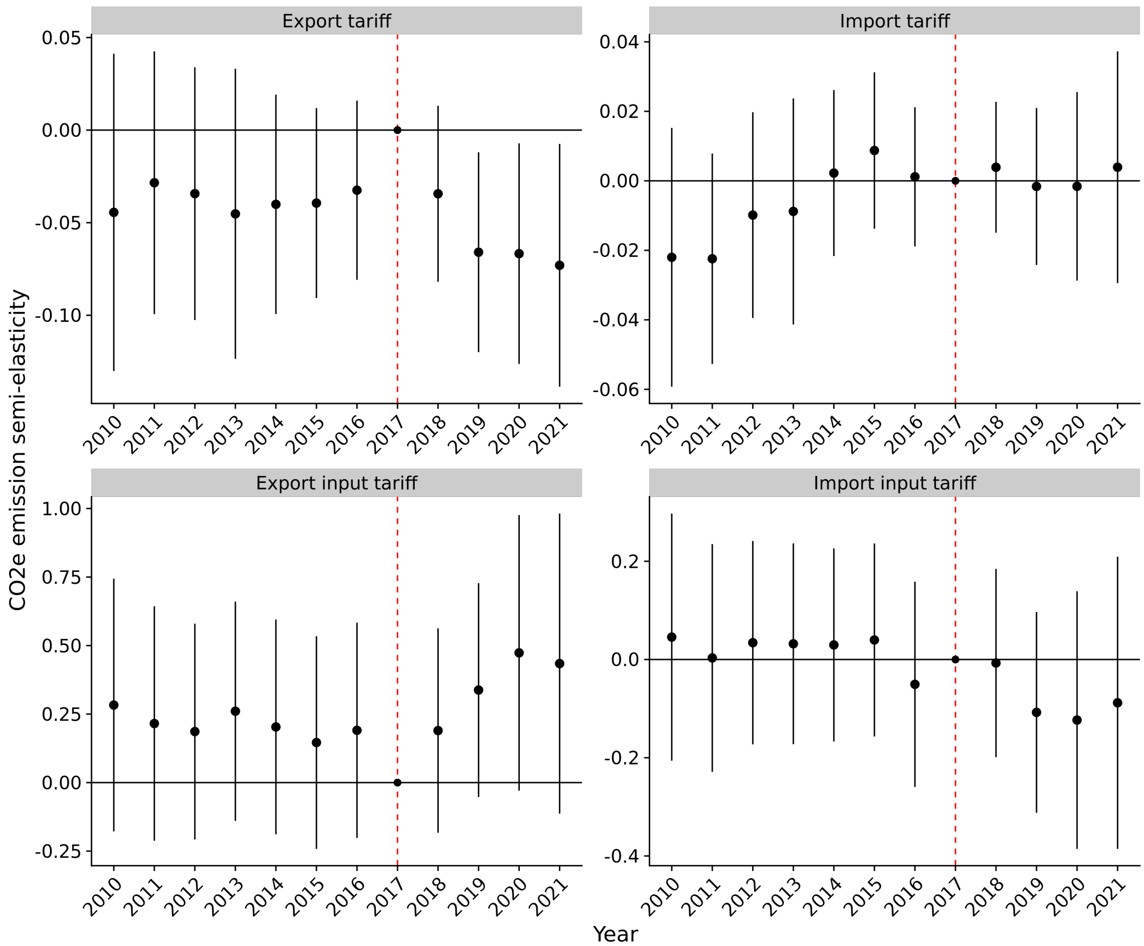
Notes: Figure A1 shows the distribution of GHG emitting facilities facing increases in export or import tariffs on their output or input during the 2018-2019 trade war.

Figure A2: Event-study model of the effect of trade war tariffs on CO2e emissions



Notes: Point estimates and 95% confidence intervals of the semi-elasticity effect of output and input trade war tariffs on log CO_2e emissions relative to 2017 using an event study version of equation (3.9). Estimates for the sample restricted to NAICS-3 treated industries are shown. Standard errors are clustered at the NAICS-6 level.

Figure A3: Event-study model of the effect of trade war tariffs on CO2e emissions



Notes: Point estimates and 95% confidence intervals of the semi-elasticity effect of output and input trade war tariffs on log CO_2e emissions relative to 2017 using an event study version of equation (3.9). Estimates for the sample restricted to the manufacturing sector are shown. Standard errors are clustered at the NAICS-6 level.

B.2 Table appendix

Table A1: NAICS-2 industry variation in trade war tariffs increases and greenhouse gas emissions

		Tariff increases (pp)									
		Export		Import		Export input		Import input		CO2e (kt)	
Sector	NAICS-2	Mean	Std. dev.	Mean	Std. dev.	Mean	Std. dev.	Mean	Std. dev.	Mean	Std. dev.
Agriculture	11	1.4	3.08	0.2	0.57	0.03	0.08	0.05	0.16	48	17
Mining	21	0.7	1.56	0.45	1.59	0.03	0.05	0.05	0.1	175	331
Water and sewage	22	0	0	0	0	0.01	0.01	0.02	0.02	114	337
Food and textile	31	0.7	1.36	0.96	1.78	0.09	0.31	0.07	0.17	107	330
Petroleum, chemical and wood	32	0.48	0.9	1.21	1.91	0.08	0.12	0.13	0.22	459	868
Primary and secondary metal	33	0.58	0.96	2.22	3.07	0.3	0.5	0.67	0.98	205	806
Wholesale	42	0	0	0	0	0.01	0.01	0.04	0.03	22	24
Warehousing	49	0	0	0	0	0.02	0.02	0.04	0.04	52	65
Buidlings	53	0	0	0	0	0.02	0.02	0.05	0.07	66	31
Research and development	54	0	0	0	0	0.01	0.01	0.07	0.11	38	23

Notes: pp = percentage point. kt = kiloton. Std. dev. = Standard deviation.

Table A2: NAICS-3 manufacturing variation in trade war tariffs increases and greenhouse gas emissions

		Tariff increases (pp)									
		Export		Import		Export input		Import input		CO2e (kt)	
Sector	NAICS-3	Mean	Std. dev.	Mean	Std. dev.	Mean	Std. dev.	Mean	Std. dev.	Mean	Std. dev.
Food	311	0.69	1.1	0.55	1.11	0.11	0.29	0.07	0.16	114	348
Beverage and tobacco products	312	1.75	3.07	0.3	0.79	0.25	0.66	0.18	0.32	48	30
Textile mills	313	0.24	0.27	1.73	2.07	0.02	0.05	0.04	0.08	70	47
Textile product mills	314	0.32	0.48	1.93	2.31	0.03	0.03	0.07	0.08	46	22
Wood products	321	0.34	0.92	1.31	1.65	0.02	0.05	0.05	0.16	119	124
Paper	322	0.45	0.61	2.18	3.12	0.1	0.11	0.14	0.18	671	747
Printing and related activities	323	0.29	0.42	0.64	1.22	0.04	0.05	0.14	0.18	31	7
Petroleum and coal products	324	0.4	0.9	0.09	0.13	0.08	0.09	0.09	0.12	1, 205	1, 583
Chemical	325	0.66	0.8	0.71	1	0.07	0.12	0.15	0.25	306	725
Plastics and rubber products	326	0.24	0.45	1.44	1.89	0.13	0.18	0.16	0.24	39	19
Nonmetallic mineral products	327	0.6	1.4	1.51	2.27	0.06	0.08	0.16	0.22	304	412
Primary metal	331	1.11	1.73	3.75	4.61	0.26	0.58	0.63	1.13	304	1, 041
Fabricated metal products	332	0.44	0.65	1.65	2.5	0.52	0.8	1.01	1.5	40	31
Machinery	333	0.44	0.47	2	1.75	0.29	0.31	0.7	0.72	38	21
Computer and electronic products	334	0.59	0.88	2.29	2.45	0.09	0.09	0.27	0.28	126	146
Electrical equipment and appliances	335	0.78	0.76	3.41	4.2	0.32	0.32	0.74	0.79	26	18
Transportation equipment	336	0.51	1.34	1.11	2.06	0.37	0.56	0.85	1.02	46	26
Furniture and related products	337	0.61	1.14	3.93	4.61	0.22	0.36	0.51	0.74	20	NA
Miscellaneous	339	0.39	0.39	1.17	2.07	0.13	0.13	0.22	0.26	62	25

Notes: pp = percentage point. kt = kiloton. Std. dev. = Standard deviation.

Table A3: Semi-elasticity of the trade war tariffs on industry-level US net exports

	Net exports (mil $)			
	(1)	(2)	(3)	(4)
Δ Export tariff X Post	−11.654	−85.059	−274.231*	−291.702**
	(74.210)	(74.234)	(143.325)	(142.274)
Δ Import tariff X Post	75.624	189.873**	159.131**	204.924***
	(56.952)	(75.554)	(69.195)	(72.922)
Δ Export input tariff X Post	1,600.084*	1,462.659*	2,462.056**	1,993.156**
	(952.152)	(858.666)	(991.488)	(916.822)
Δ Import input tariff X Post	−1,337.802**	−997.678**	−1,665.316***	−1,240.315**
	(562.710)	(490.021)	(575.652)	(510.695)
Adj. R2	0.89	0.89	0.94	0.94
Sample	Treated NAICS-3	Treated NAICS-3	Manufacturing	Manufacturing
NAICS-2 X Year	×	✓	×	✓
Observations	4,715	4,715	4,168	4,168

Notes: Estimates of the emission semi-elasticity of the trade war tariffs on NAICS-6 level net exports in millions of USD. All models include year fixed effects and NAICS-6 fixed effects. Industries are restricted to the treated NAICS-3 industries or the manufacturing sector. Column 1 and Column 3 are the baseline models for each sample. Column 2 and 4 further include NAICS-2 by year fixed effects. Robust standard errors clustered at NAICS-6 level in parentheses.

Table A4: Semi-elasticity of the trade war tariffs on facility-level CO2e emissions

	ln(CO2e)		
	(1)	(2)	(3)
Δ Export tariff	−0.008	−0.019	−0.023*
	(0.018)	(0.012)	(0.012)
Δ Import tariff	0.001	0.008	0.007
	(0.009)	(0.008)	(0.008)
Δ Export input tariff	0.154	0.191*	0.163
	(0.110)	(0.104)	(0.103)
Δ Import input tariff	−0.121**	−0.139**	−0.103
	(0.062)	(0.062)	(0.067)
Adj. R2	0.85	0.85	0.85
State X Year	×	✓	✓
NAICS-2 X Year	×	×	✓
Observations	46,875	46,875	46,875

Notes: Estimates of the emission semi-elasticity of the trade war tariffs. All models include year fixed effects and plant fixed effects. The control group is restricted to facilities in the same NAICS-3 industries as the treated facilities. Column 1 is the baseline model. Column 2 further includes state by year fixed effects. Column 3 additional controls for NAICS-2 by year fixed effects. Robust standard errors clustered at NAICS-6 level in parentheses.

Table A5: Facility-level CO2e emission effect of the trade war tariffs

	CO2e (kt)		
	(1)	(2)	(3)
Δ Export tariff	-6.640^{*}	-6.420^{*}	-3.540
	(3.700)	(3.430)	(2.530)
Δ Import tariff	-0.680	-0.570	-4.300^{**}
	(1.880)	(2.230)	(2.160)
Δ Export input tariff	41.980^{**}	51.840^{***}	36.870^{**}
	(17.450)	(17.830)	(18.090)
Δ Import input tariff	-35.510^{***}	-40.330^{***}	-28.580^{***}
	(9.530)	(11.080)	(9.470)
Adj. R2	0.95	0.96	0.96
State X Year	×	✓	✓
NAICS-2 X Year	×	×	✓
Observations	41,366	41,366	41,366

Notes: Estimates of the emission effect of the trade war tariffs. All models include year fixed effects and plant fixed effects. The control group is restricted to facilities in the same NAICS-3 industries as the treated facilities. Column 1 is the baseline model. Column 2 further includes state by year fixed effects. Column 3 additional controls for NAICS-2 by year fixed effects. Robust standard errors clustered at NAICS-6 level in parentheses.

Table A6: Semi-elasticity of the trade war tariffs on facility-level CO2e emissions

	ln(CO2e)		
	(1)	(2)	(3)
Δ Export tariff	-0.019	-0.027^{**}	-0.027^{**}
	(0.017)	(0.013)	(0.013)
Δ Import tariff	-0.002	0.004	0.007
	(0.009)	(0.008)	(0.008)
Δ Export input tariff	0.173^{*}	0.195^{*}	0.176^{*}
	(0.103)	(0.100)	(0.106)
Δ Import input tariff	-0.113^{**}	-0.124^{**}	-0.099
	(0.057)	(0.059)	(0.068)
Adj. R2	0.87	0.87	0.87
State X Year	×	✓	✓
NAICS-2 X Year	×	×	✓
Observations	27,514	27,514	27,514

Notes: Estimates of the emission semi-elasticity of the trade war tariffs. All models include year fixed effects and plant fixed effects. The control and treatment groups are restricted to facilities in the manufacturing sector. Column 1 is the baseline model. Column 2 further includes state by year fixed effects. Column 3 additional controls for NAICS-2 by year fixed effects. Robust standard errors clustered at NAICS-6 level in parentheses.

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