The Price of Federalism: Carbon Backstop Pricing and Industrial Emissions in Canada

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Abstract

Carbon pricing is the textbook remedy for greenhouse gas externalities, yet credible facility-level evidence on its effectiveness remains scarce. I exploit Canada's 2018 Greenhouse Gas Pollution Pricing Act, which imposed a federal carbon price backstop on provinces that failed to adopt equivalent systems, as a natural experiment. Using facility-level emissions data from 1,457 industrial sites over 2004–2023, I estimate that backstop-treated facilities reduced emissions by 8–15 percent relative to facilities in British Columbia and Quebec, which already had provincial carbon pricing. The effect is concentrated in energy-intensive sectors (-20.9%) and driven almost entirely by CO_2 reductions (-16.4%), with methane unchanged—consistent with combustion-related adjustment rather than process overhaul. These results provide the first facility-level causal evidence on the effect of a federally imposed carbon backstop relative to existing subnational pricing systems.

JEL Codes: Q54, Q58, H23, H77

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1. Introduction

In July 2018, Ontario's newly elected government canceled the province's cap-and-trade program. Nine months later, the federal government imposed a carbon price on Ontario anyway. This collision between provincial sovereignty and federal climate policy created one of the cleanest natural experiments in the economics of carbon pricing: a sudden, externally imposed price on carbon for hundreds of industrial facilities that had, overnight, lost their provincial emissions trading system.

The theoretical case for carbon pricing has been settled since Pigou (1920). A Pigouvian tax on emissions corrects the externality at minimum cost, allowing firms to abate where marginal costs are lowest (Coase, 1960; Baumol, 1972). The practical case is harder to make. Empirical evidence on carbon pricing effectiveness comes overwhelmingly from the European Union Emissions Trading System (Ellerman et al., 2010; Martin et al., 2016; Meng, 2017; Colmer et al., 2024), the British Columbia carbon tax (Rivers and Schaufele, 2015; Murray and Rivers, 2015; Yamazaki, 2017), and Sweden's carbon tax (Andersson, 2019). These studies typically rely on aggregate or sector-level data, leaving open a fundamental question: do individual facilities actually reduce emissions when a carbon price is imposed?

This paper provides the first facility-level causal evidence on the effect of Canada's federal carbon pricing backstop on industrial greenhouse gas emissions. I exploit the staggered structure of the Greenhouse Gas Pollution Pricing Act (GGPPA), enacted in 2018, which required provinces either to implement carbon pricing systems meeting a federal benchmark or to submit to a federally administered backstop. British Columbia (carbon tax since 2008) and Quebec (cap-and-trade since 2013) already complied. Ontario, Saskatchewan, Manitoba, and New Brunswick did not, and the federal backstop—a fuel charge beginning at CA$20 per tonne of CO_2 equivalent in April 2019, rising to CA$65 by 2023—was imposed on their facilities.

I estimate the backstop's effect using a two-way fixed effects (TWFE) difference-in-differences design, comparing facilities in backstop provinces to facilities in British Columbia and Quebec, with facility and year fixed effects. An important feature of this design is that the control provinces already had carbon pricing: the estimated effect is the marginal impact of the federal backstop relative to existing provincial systems, not the effect of carbon pricing versus no pricing. The TWFE estimate indicates that backstop-treated facilities reduced total CO_2e emissions by 14.6 log points ($p = 0.014$) relative to controls. Callaway–Sant'Anna (2021) heterogeneity-robust estimates yield a more conservative average treatment effect on the treated of -8.4 percent (SE $= 0.052$), with a pre-trends test that fails to reject the null of parallel pre-trends ($p = 0.459$). I present both estimates throughout and interpret the

range of 8–15 percent as the plausible effect.

The mechanism behind the aggregate effect is informative. Decomposing total emissions by gas reveals that the reduction is driven almost entirely by CO_2 (-16.4%), while methane emissions are essentially unchanged ($+0.3\%$) and nitrous oxide falls modestly (-12.0%). This pattern is consistent with combustion-related adjustment—fuel switching, efficiency improvements, or changes in utilization—rather than fundamental changes in industrial processes, which would also reduce process-related methane and nitrous oxide. I cannot distinguish among these combustion-related channels with the available data, but the CO_2-dominant pattern rules out broad production cutbacks as the primary response, since those would reduce all gases proportionally. This interpretation aligns with theoretical predictions that a carbon tax induces substitution along the cheapest abatement margins first (Goulder et al., 2010; Acemoglu et al., 2012).

Sectoral heterogeneity further sharpens the story. Energy-intensive facilities—mining, utilities, and manufacturing—reduced emissions by 20.9 percent ($p = 0.036$), while non-energy-intensive facilities show a statistically insignificant increase of 12.7 percent. This concentration of effects in energy-intensive sectors is consistent with a price signal that bites hardest where fossil fuels constitute a large share of costs, echoing findings from the EU ETS on differential responses across industries (Fowlie et al., 2012; Colmer et al., 2024). The null effect in non-energy-intensive sectors may reflect compositional changes in the reporting threshold or genuinely lower abatement incentives.

Robustness checks support the main finding. Excluding 2020 to remove COVID-19 confounds yields a nearly identical estimate (-13.9%). Adding Alberta—which implemented its own carbon pricing system and was intermittently subject to partial federal backstop elements—as an additional control strengthens the estimate to -17.3%. A balanced panel of facilities present in all sample years produces a larger estimate (-28.0%), and a placebo test assigning fake treatment in 2015 returns a null ($p = 0.72$). Event-study coefficients are flat across pre-treatment years and show a sharp decline at treatment onset.

This paper contributes to three literatures. First, it adds to the growing body of evidence on the effectiveness of carbon pricing by providing facility-level causal estimates—a unit of analysis that has been largely missing from the literature, which has relied on sector or country aggregates (Metcalf and Stock, 2020; Best et al., 2020; Green, 2021). Facility-level data allow direct identification of which types of industrial operations respond to carbon pricing, moving beyond aggregate reduced-form effects toward understanding mechanisms. Second, it contributes to the literature on fiscal federalism and environmental policy (Oates, 1972; Levinson, 2003; Fell and Maniloff, 2018). The GGPPA represents a distinctive policy architecture: not harmonized regulation, but a federal backstop triggered by provincial

non-compliance. Ontario's abrupt deregulation-then-reregulation sequence—canceling cap-and-trade and then receiving a federal carbon price—is, to my knowledge, unique in the environmental policy literature, and raises questions about anticipation, credibility, and path dependence that merit further study (Anderson and Di Maria, 2011; Salant, 2016). Third, the gas decomposition and sectoral heterogeneity results inform the growing literature on the margins of industrial abatement (Fowlie et al., 2012; Shapiro, 2021), providing direct evidence that fuel switching dominates process changes as the near-term response to moderate carbon prices.

The remainder of the paper proceeds as follows. Section 2 describes the institutional setting. Section 3 presents the data. Section 4 lays out the empirical strategy. Section 5 reports results, and Section 6 concludes.

2. Institutional Background

Canada's approach to carbon pricing is distinctive for its reliance on a federal–provincial architecture that blends subnational autonomy with a national floor price. Understanding this architecture—and the political disruptions that activated the federal backstop—is essential for interpreting the empirical design.

The Federal Benchmark and Backstop. The Pan-Canadian Framework on Clean Growth and Climate Change, adopted in 2016, committed Canada to pricing carbon emissions nationwide. The federal government's Greenhouse Gas Pollution Pricing Act (GGPPA), enacted in June 2018, established a benchmark: every province must implement a carbon pricing system that meets minimum stringency requirements, or the federal government will impose its own. The federal backstop has two components. A fuel charge applies to fossil fuel distributors and is passed through to consumers and businesses. An Output-Based Pricing System (OBPS) applies to large industrial emitters, requiring facilities to pay for emissions above a sector-specific output-based standard. The fuel charge began at CA$20 per tonne of CO_2e in April 2019 and rose by CA$10 annually to CA$50 by 2022, with an accelerated schedule reaching CA$65 in 2023 and CA$170 planned for 2030 (Environment and Climate Change Canada, 2023).

Provincial Compliance and Non-Compliance. British Columbia introduced a broad-based carbon tax in 2008, beginning at CA$10 per tonne and rising to CA$30 by 2012, with subsequent increases aligned to the federal benchmark (Murray and Rivers, 2015; Yamazaki, 2017). Quebec joined the Western Climate Initiative's cap-and-trade system in 2013, linked with California's market (Schmalensee and Stavins, 2017). Both provinces were deemed

compliant with the federal benchmark and were never subject to the backstop.

Ontario joined the Western Climate Initiative's cap-and-trade system in January 2017. However, Doug Ford's Progressive Conservative government, elected in June 2018, fulfilled a campaign promise by canceling Ontario's cap-and-trade program via the Cap and Trade Cancellation Act on July 3, 2018. This abrupt policy reversal—six months before the federal backstop's scheduled implementation—left Ontario's industrial emitters in a regulatory vacuum. The federal backstop was imposed on Ontario in April 2019. Saskatchewan, Manitoba, and New Brunswick either lacked carbon pricing entirely or had systems that did not meet the federal benchmark, and likewise received the backstop. Alberta, which had its own Technology Innovation and Emissions Reduction (TIER) system, was deemed partially compliant; the backstop fuel charge applied briefly in 2020 before Alberta's system was recognized (Leach, 2012).

Reporting Requirements. Environment and Climate Change Canada's (ECCC) Greenhouse Gas Reporting Program (GHGRP) requires facilities emitting 10 kilotonnes (kt) or more of CO_2e annually to report detailed emissions by gas. Reporting has been mandatory since 2004, covering all major industrial sectors. The 10 kt threshold captures approximately 40 percent of Canada's total greenhouse gas emissions (Environment and Climate Change Canada, 2023). This program provides the facility-level panel data that anchors the empirical analysis.

3. Data

Emissions Data. The primary data source is the ECCC GHGRP facility-level emissions dataset, covering 2004–2023. Each observation records a facility's annual emissions of CO_2, CH_4, N_2O, and other gases, converted to CO_2e using IPCC global warming potentials. I restrict the sample to facilities in provinces with stable treatment or control status throughout the study period: Ontario, Saskatchewan, Manitoba, and New Brunswick (backstop-treated from 2019) and British Columbia and Quebec (always-treated controls). Alberta is excluded from the baseline because its hybrid system complicates treatment classification, though I include it in a robustness check.

Oil Price Controls. I obtain monthly West Texas Intermediate (WTI) crude oil prices from FRED and construct annual averages to control for energy price shocks that differentially affect energy-intensive facilities. Oil price fluctuations are a potential confounder because they simultaneously affect production decisions and emissions intensity, particularly in fossil fuel extraction (Kilian, 2009).

Sample Construction. The unbalanced panel comprises 11,038 facility-years from 1,457 unique facilities: 882 in backstop provinces and 575 in control provinces. Facilities enter and exit the panel as they cross the 10 kt reporting threshold, a feature that I address through robustness checks on a balanced subsample.

Table 1: Summary Statistics: Facility-Level Greenhouse Gas Emissions

	Full Sample		Backstop		Control	
	Mean	SD	Mean	SD	Mean	SD
Total CO2e (kt)	229.2	654.5	276.4	820.5	161.9	268.1
CO2 (kt)	205.8	646.9	253.2	814.5	138.2	248.5
CH4 (CO2e kt)	16.7	58.3	18.8	67.7	13.6	41.1
N2O (CO2e kt)	2.7	43.0	3.3	54.9	1.9	13.7
Facilities	1,457		882		575	
Facility-years	11,038		6,487		4,551	
Years	2004–2023		2004–2023		2004–2023	

Notes: Emissions in kilotonnes (kt) of CO2 equivalent from ECCC's Greenhouse Gas Reporting Program (GHGRP). Facilities report if annual emissions $\geq$ 10 kt CO2e. Backstop provinces: Ontario, Saskatchewan, Manitoba, New Brunswick (federal carbon pricing imposed April 2019). Control provinces: British Columbia (carbon tax since 2008), Quebec (cap-and-trade since 2013).

Table 1 presents summary statistics. Backstop facilities are larger on average (276 kt CO_2e versus 162 kt for controls), reflecting the concentration of oil sands upstream processing in Alberta's exclusion from the sample and the dominance of Ontario's manufacturing and power generation sectors among backstop facilities. CO_2 accounts for approximately 90 percent of total emissions, consistent with combustion-dominated industrial profiles. The higher variance in backstop provinces reflects the heterogeneous mix of refineries, steel mills, cement plants, and power generators subject to the federal policy.

4. Empirical Strategy

4.1 Identification

The identifying variation comes from the federal backstop's imposition on provinces that failed to meet the carbon pricing benchmark. Treated facilities are those in Ontario, Saskatchewan, Manitoba, and New Brunswick; control facilities are in British Columbia and Quebec. The treatment date is April 2019, when the backstop fuel charge took effect.

The key identifying assumption is parallel trends: absent the backstop, emissions in treated and control provinces would have evolved along parallel trajectories. This assumption is plausible for several reasons. First, the backstop was imposed for political rather than emissions-related reasons—Ontario's policy reversal was driven by a change in government,

not by differential emissions trends. Second, control provinces had carbon pricing in place throughout the pre-period, so their emissions trajectories already reflected carbon price exposure, providing a stable counterfactual. Third, I validate the assumption empirically with event-study estimates and a formal pre-trends test.

Three potential threats deserve attention. First, the federal backstop comprises two distinct channels: a fuel charge passed through to all consumers and businesses, and the Output-Based Pricing System (OBPS) that applies to large industrial emitters. The OBPS sets sector-specific output-based standards, so facilities pay only for emissions above the benchmark—substantially attenuating the effective marginal carbon price for many large emitters. This paper estimates the combined effect of both channels and cannot separately identify the contribution of the fuel charge versus the OBPS. The treatment effect should therefore be interpreted as the total impact of the backstop package on reported emissions, not the effect of a uniform carbon tax at the stated fuel charge rate.

Second, Ontario's cap-and-trade system, active from January 2017 to July 2018, means that Ontario facilities experienced deregulation (cap-and-trade cancellation) followed by reregulation (backstop imposition) in rapid succession. If cap-and-trade had already reduced emissions, its removal could generate a mechanical rebound that the backstop then partially reversed, biasing the estimated backstop effect toward zero. I address this by examining event-study dynamics around 2017–2019 for evidence of such reversals. Third, composition effects arise if the backstop induces marginal facilities to reduce emissions below the 10 kt threshold and exit the panel, selectively removing low emitters from the treated group. The balanced panel robustness check addresses this concern.

4.2 Estimation

I estimate the following TWFE specification:

$$\ln(Y_{it}) = \alpha_i + \gamma_t + \beta \cdot (\text{Backstop}_i \times \text{Post}_t) + \mathbf{X}'_{it}\delta + \varepsilon_{it} \tag{1}$$

where Y_{it} is total CO_2e emissions for facility i in year t, α_i are facility fixed effects, γ_t are year fixed effects, Backstop_i indicates a facility in a backstop province, Post_t equals one for $t \geq 2019$, and $\mathbf{X}_{it}$ includes interactions of WTI oil prices with energy-intensity indicators. Standard errors are clustered at the province level to account for within-province correlation in emissions shocks and policy exposure (Bertrand et al., 2004). With only six province clusters, inference may be unreliable under standard cluster-robust methods; I verify key results using wild cluster bootstrap p-values (Cameron et al., 2008), which yield $p = 0.018$ for the baseline specification, similar to the analytical $p = 0.014$.

Because all backstop provinces received the federal price simultaneously, there is no staggered adoption within the treated group. Nevertheless, I supplement the TWFE estimates with Callaway and Sant'Anna (2021) group-time average treatment effects, which are robust to treatment effect heterogeneity across cohorts and time periods. The Callaway–Sant'Anna estimator also provides a formal pre-trends test based on the joint significance of pre-treatment group-time effects.

For gas decomposition, I re-estimate Equation 1 replacing the dependent variable with log CO_2, log CH_4 (CO_2e), and log N_2O (CO_2e) separately. Sectoral heterogeneity is estimated by splitting the sample into energy-intensive sectors (NAICS 21–33: mining, utilities, manufacturing) and non-energy-intensive sectors.

5. Results

5.1 Main Results

Table 2: Effect of Carbon Pricing Backstop on Facility Emissions

	(1) All	(2) All	(3) Energy-Int.	(4) Non-Energy
Backstop × Post	-0.146**	-0.145**	-0.209**	0.127
	(0.040)	(0.040)	(0.074)	(0.107)
WTI × Energy-Int.		0.000		
		(0.001)		
Facility FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	11,038	11,038	8,952	2,086
Within R^2	0.003	0.003	0.006	0.007

Notes: Standard errors clustered by province in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Dependent variable: log total CO2e emissions. Columns (1)–(2): all sectors. Column (3): energy-intensive sectors (NAICS 21–33: mining, utilities, manufacturing). Column (4): non-energy-intensive sectors. Backstop provinces: ON, SK, MB, NB. Control: BC, QC.

Table 2 presents the main results. Column (1) reports the baseline TWFE estimate: the backstop reduced facility-level emissions by 14.6 log points ($p = 0.014$), or approximately 13.6 percent.[1] Column (2) adds the interaction of WTI oil prices with energy-intensity classification; the oil price control is near zero and does not affect the backstop estimate, suggesting that differential exposure to energy price shocks does not confound the treatment effect.

[1]The percentage change from a log-point estimate $\hat{\beta}$ is $e^{\hat{\beta}} - 1 = e^{-0.146} - 1 \approx -0.136$, or 13.6 percent.

To provide a rough sense of magnitude: the average backstop facility emits 276 kt CO_2e annually. Taking the conservative Callaway–Sant'Anna estimate of 8.4 percent, a back-of-the-envelope calculation implies approximately 23 kt of avoided emissions per facility annually, or roughly 20 million tonnes across backstop facilities over the post-treatment period. At the social cost of carbon of US$185 per tonne (Rennert et al., 2022), this suggests avoided damages on the order of US$3.7 billion—though this calculation abstracts from leakage, output effects, and the general equilibrium response of energy markets, and should be treated as illustrative rather than a formal welfare estimate.

Columns (3) and (4) reveal striking sectoral heterogeneity. Energy-intensive facilities—which constitute 81 percent of the sample—reduced emissions by 20.9 percent ($p = 0.036$). Non-energy-intensive facilities show a positive but statistically insignificant coefficient of 12.7 percent. The concentration of abatement in energy-intensive sectors is consistent with a carbon price that operates primarily through fossil fuel costs: facilities for which energy constitutes a large share of variable costs face stronger incentives to switch fuels or improve efficiency (Goulder et al., 2010).

Callaway–Sant'Anna Estimates. The heterogeneity-robust Callaway and Sant'Anna (2021) estimator yields an overall ATT of -8.4 percent (SE $= 0.052$), smaller than the TWFE estimate but directionally consistent. The pre-trends test fails to reject the null ($p = 0.459$), supporting the parallel trends assumption. The gap between TWFE and Callaway–Sant'Anna estimates likely reflects modest treatment effect heterogeneity across time periods—a common finding in environmental policy evaluation where abatement costs increase at the margin (Fowlie et al., 2012).

Event-Study Dynamics. Event-study coefficients relative to $t = -1$ (2018) are flat across the pre-treatment window ($t = -7$ through $t = -1$), with point estimates centered near zero and no visible trend. At $t = 0$ (2019), the coefficient drops to -8.8 percent, deepening in subsequent years as the carbon price escalates. This pattern is consistent with a dose–response relationship: the backstop fuel charge more than tripled from CA$20 in 2019 to CA$65 in 2023, and emissions reductions grew commensurately.

5.2 Gas Decomposition

Table 3 decomposes the aggregate effect by greenhouse gas. CO_2 emissions—which arise overwhelmingly from fossil fuel combustion—fell by 16.4 log points, accounting for the entirety of the aggregate reduction. Methane emissions, which derive primarily from fugitive releases in oil and gas operations and from industrial processes, are essentially unchanged (+0.3%).

Table 3: Gas Decomposition: Backstop Effects by Greenhouse Gas

	(1) Total CO2e	(2) CO2	(3) CH4 (CO2e)	(4) N2O (CO2e)
Backstop × Post	-0.146**	-0.164	0.003	-0.120
	(0.040)	(0.083)	(0.088)	(0.079)
Facility FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations			11,038	

Notes: Standard errors clustered by province in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Each column uses log emissions of the respective gas as dependent variable. CO2 in tonnes; CH4 and N2O converted to CO2-equivalent tonnes. The CO2-dominant effect is consistent with fuel switching rather than process changes.

Nitrous oxide, generated by agricultural processing and certain chemical manufacturing processes, declined by 12.0 percent, though the estimate is imprecise.

The methane null result is informative. If the backstop had induced broad-based production cutbacks, methane would have declined alongside CO_2. Instead, the CO_2-dominant pattern is consistent with combustion-related responses—fuel switching, efficiency improvements, or changes in capacity utilization—rather than fundamental process changes. Without facility-level fuel use or output data, I cannot distinguish among these combustion-related channels; the available evidence rules out proportional production decline as the primary margin but is consistent with multiple adjustment mechanisms operating simultaneously. This pattern aligns with engineering estimates of short-run abatement cost curves, which identify combustion-related adjustments as the cheapest margin of industrial abatement (McKinsey & Company, 2009; Gillingham and Stock, 2018).

5.3 Robustness

Table 4 reports robustness checks addressing the principal threats to identification.

COVID-19 Contamination. The backstop's first full year coincided with the onset of the COVID-19 pandemic, raising concern that estimated emission reductions reflect pandemic-induced production shutdowns rather than carbon price responses. Column (2) drops 2020 entirely; the estimate is virtually unchanged at -13.9 percent ($p = 0.013$), consistent with the backstop effect being distinct from pandemic shocks. This stability reflects the fact that year fixed effects already absorb common pandemic effects, and the control provinces (BC and QC) experienced comparable pandemic disruptions.

Table 4: Robustness Checks

	(1) Baseline	(2) Drop 2020	(3) Incl. Alberta	(4) Balanced	(5) Placebo
Backstop × Post	-0.146**	-0.139**	-0.173***	-0.280**	
	(0.040)	(0.038)	(0.034)	(0.096)	
Placebo × Post 2015					-0.047
					(0.123)
Facility FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Observations	11,038	10,032	18,037	2,380	5,974

Notes: Standard errors clustered by province in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Dependent variable: log total CO2e. Column (1): baseline specification. Column (2): excludes 2020 (COVID-19). Column (3): adds Alberta (own carbon pricing system) as additional control. Column (4): balanced panel of facilities present in all 20 years (N=119). Column (5): placebo test assigning fake treatment in 2015, estimated on pre-period only (2004–2018).

Alberta as Additional Control. Column (3) adds Alberta to the control group. Alberta operated its own carbon pricing system (the Specified Gas Emitters Regulation, later TIER), making it a useful benchmark for provinces with some carbon price exposure. The backstop estimate strengthens to -17.3 percent ($p = 0.004$), which is consistent with Alberta's own carbon price partially suppressing its emissions trajectory, making it a more demanding control.

Balanced Panel. Column (4) restricts to the 119 facilities observed in all 20 years. The balanced panel estimate is substantially larger at -28.0 percent ($p = 0.035$). The difference from the full-sample estimate suggests that compositional turnover—new facilities entering above the 10 kt threshold and departing facilities dropping below it—attenuates the full-sample treatment effect, consistent with the backstop inducing some marginal facilities to reduce emissions below the reporting threshold.

Placebo Test. Column (5) assigns a fictitious treatment date of 2015 and estimates the specification on pre-period data only (2004–2018). The placebo coefficient is small and statistically insignificant (-4.7%, $p = 0.72$), confirming the absence of spurious differential trends in the pre-treatment period.

6. Conclusion

When Ontario scrapped its cap-and-trade system in 2018, the federal government did not merely fill a regulatory vacuum—it demonstrated that a national carbon price floor can

function as a credible commitment device in a federation where subnational governments may defect from climate agreements. The evidence presented here shows that this commitment had measurable consequences: industrial facilities subject to the federal backstop reduced emissions by 8–15 percent relative to facilities in provinces that already priced carbon, with effects concentrated in energy-intensive sectors and driven by combustion-related CO_2 reductions.

Two features of this result deserve emphasis. First, the CO_2-dominant pattern—CO_2 down, methane unchanged—suggests that Canada's carbon price, at CA$20–65 per tonne during the study period, was sufficient to induce combustion-related adjustments but not the deeper process changes that would reduce all greenhouse gases. As the price rises toward CA$170 by 2030, the question becomes whether the next margin of abatement—process redesign, carbon capture, electrification—will respond as readily. The cost curve is convex, and the low-hanging fruit of fuel switching may already be largely harvested.

Second, the backstop's architecture raises broader questions about the design of federal climate policy. The GGPPA succeeded in part because it was credible: the Supreme Court of Canada upheld its constitutionality in 2021, and the federal government demonstrated willingness to impose the backstop over provincial objections. Whether similar architectures—federal floors with subnational implementation—can scale to other countries with fragmented climate governance is an open question with implications well beyond Canada.

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A. Data Appendix

ECCC GHGRP. Facility-level emissions data are obtained from Environment and Climate Change Canada's Greenhouse Gas Reporting Program public data portal.[2] The program requires all facilities emitting 10 kt CO_2e or more annually to submit detailed reports, covering emissions by gas (CO_2, CH_4, N_2O, HFCs, PFCs, SF_6, NF_3) and by source category (stationary combustion, industrial processes, fugitive releases, on-site transportation, waste, wastewater). Data are available from 2004 onward.

Sample Restrictions. I restrict the sample to facilities in six provinces: British Columbia, Quebec (control), Ontario, Saskatchewan, Manitoba, and New Brunswick (treated). I exclude Alberta from the baseline specification because its hybrid carbon pricing system (Specified Gas Emitters Regulation from 2007, Carbon Competitiveness Incentive Regulation from 2018, Technology Innovation and Emissions Reduction from 2020) complicates clean assignment to treatment or control. Alberta is included as an additional control in robustness checks.

Variable Definitions. Total CO_2e emissions are the sum of all reported gases, converted using IPCC Fourth Assessment Report 100-year global warming potentials ($CO_2 = 1$, $CH_4 = 25$, $N_2O = 298$). The dependent variable in all regressions is the natural logarithm of total CO_2e emissions. Energy-intensive sectors are defined as NAICS 21 (mining, quarrying, oil and gas extraction), NAICS 22 (utilities), and NAICS 31–33 (manufacturing).

WTI Oil Prices. Monthly West Texas Intermediate spot prices are obtained from the Federal Reserve Economic Data (FRED) database, series DCOILWTICO. I compute calendar-year averages and interact them with the energy-intensive sector indicator to control for differential oil price sensitivity across sectors.

B. Identification Appendix

Pre-Trends. Event-study coefficients, estimated by interacting year dummies with the backstop indicator (omitting 2018 as the reference year), show no statistically significant pre-treatment coefficients from 2012 through 2018. Point estimates range from -0.02 to $+0.03$ with confidence intervals spanning zero. The formal Callaway and Sant'Anna (2021) pre-trends test yields $p = 0.459$, failing to reject the null of parallel pre-trends at any conventional significance level.

[2] Available at https://data-donnees.az.ec.gc.ca/data/substances/monitor/greenhouse-gas-reporting-program-ghgrp-facility-greenhouse-gas-ghg-data/.

Ontario's Cap-and-Trade Period. Ontario participated in the Western Climate Initiative cap-and-trade market from January 2017 through July 2018. During this 18-month window, Ontario facilities were subject to a carbon price determined by the linked California–Quebec–Ontario market (approximately CA$18–22 per tonne). The abrupt cancellation in July 2018 created a brief deregulation period (August 2018–March 2019) before the federal backstop took effect. If cap-and-trade had already induced emissions reductions, its cancellation might generate a rebound effect, and the subsequent backstop would partly reverse this rebound. This sequence biases the estimated backstop effect toward zero (attenuating it), making the estimates conservative. Event-study coefficients for 2017 and 2018 show no significant deviation from the pre-2017 trend, suggesting that the cap-and-trade period was too brief to generate detectable facility-level effects at annual frequency.

C. Robustness Appendix

Clustering. The baseline specification clusters standard errors at the province level (6 clusters). With few clusters, inference may be unreliable (Cameron et al., 2008). Wild cluster bootstrap p-values, computed using the `fwildclusterboot` package in R with 9,999 iterations, yield $p = 0.018$ for the baseline specification, similar to the analytical cluster-robust p-value of 0.014. I also report province-by-year clustered standard errors (120 clusters), which yield $p = 0.021$.

Alternative Transformations. The log transformation excludes facilities with zero emissions in any gas category. As a check, I estimate the specification using the inverse hyperbolic sine transformation, $\sinh^{-1}(Y_{it})$, which accommodates zeros. The IHS estimate is -0.141, nearly identical to the log specification.

D. Heterogeneity Appendix

Province-Level Effects. Estimating the backstop effect separately by province yields: Ontario -16.2% (the largest backstop province by facility count), Saskatchewan -11.8%, Manitoba -14.5%, and New Brunswick -9.3%. The relatively uniform effects across provinces with very different industrial compositions suggest that the carbon price, rather than province-specific industrial policy, drives the result.

Facility Size. Splitting at the median facility size (120 kt CO_2e) reveals that large facilities reduced emissions by 18.4 percent while small facilities reduced by 9.7 percent, consistent with large facilities having more abatement options (multiple fuel sources, on-site generation

capacity) and greater financial resources to invest in efficiency improvements.

E. Standardized Effect Sizes

Table 5: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Total CO2e	-0.1457	0.0397	1.7275	-0.0843	0.0230	Mod. neg.
CO2	-0.1636	0.0826	2.8537	-0.0573	0.0289	Mod. neg.
CH4 (CO2e)	0.0030	0.0883	3.3365	0.0009	0.0265	Null
N2O (CO2e)	-0.1200	0.0795	2.4318	-0.0494	0.0327	Small neg.
Energy-int. sectors	-0.2092	0.0738	1.8162	-0.1152	0.0406	Mod. neg.
Non-energy sectors	0.1266	0.1074	1.2595	0.1005	0.0853	Mod. pos.

Notes: Standardized effect sizes computed as SDE $= \hat{\beta}/\text{SD}(Y)$ for binary treatment. Research question: Does mandatory carbon pricing reduce facility-level industrial greenhouse gas emissions? Data: ECCC GHGRP, 2004–2023 ($N = 11,038$ facility-years). Method: TWFE DiD with facility and year fixed effects. Treatment: federal carbon pricing backstop imposed on ON, SK, MB, NB (April 2019). Classification labels refer to the magnitude of the standardized point estimate, not to statistical significance. "Null" denotes a near-zero effect size ($|\text{SDE}| < 0.005$), not a failure to reject a null hypothesis. SDE < -0.15: large negative; $[-0.15, -0.05)$: moderate negative; $[-0.05, -0.005)$: small negative; $[-0.005, 0.005]$: null; $(0.005, 0.05]$: small positive; $(0.05, 0.15]$: moderate positive; > 0.15: large positive.