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Carbon Pricing and the Elasticity of CO_2 Emissions

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ABSTRACT

We study the impacts of carbon pricing on CO_2 emissions across five sectors for a panel of 39 countries covering 1990–2016. Constructing new sector-level carbon price data, we implement a novel approach to estimate the changes in CO_2 emissions associated with (i) the introduction of carbon pricing regardless of the price level, (ii) the elasticity of emissions with respect to the price level, and (iii) the potential response of future emissions to possible carbon price trajectories. Using a synthetic control factor model, we find that the introduction of carbon pricing has reduced growth in total aggregate (national) CO_2 emissions by 1–2 percent on average relative to imputed counterfactuals, with most abatement occurring in the electricity and heat sector. Exploiting variation in observed carbon prices to explain heterogeneity in treatment effects, we decompose the average treatment effect obtained from the synthetic control factor model to distinguish the

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effect of merely introducing a carbon price from the effect of the price level itself. We find a small and imprecisely estimated semielasticity of a 0.03 percent reduction in emissions growth per average \$1/metric ton of CO_2. Simulating the response of future global emissions to several possible carbon price trajectories, we conclude that carbon pricing alone, even if implemented globally at a level equivalent to the world's current highest recorded price in Sweden, is unlikely to be sufficient to achieve emission reductions consistent with the Paris climate agreement.

Keywords: Carbon Pricing, CO_2 Emissions Elasticity, Carbon Tax Effects, Emissions Trading Effects, Climate Policy Impact Evaluation, Generalized Synthetic Control, Emissions-Weighted Carbon Price.

JEL Classifications: Q43, Q48, Q54, Q58, H23.

1. Introduction

Pricing carbon dioxide (CO_2) emissions—via a carbon tax, emissions trading system, or some hybrid scheme—has long been recommended as an integral and, in principle, cost-efficient way to reduce emissions and mitigate the adverse impacts of climate change (Baumol and Oates 1988; Nordhaus 1992; Metcalf 2009; Cramton et al. 2017; Stern-Stiglitz High-Level Commission on Carbon Prices 2017).[1] Since the world's first carbon taxes were implemented in Finland and Poland in 1990, an additional 28 jurisdictions have adopted them. Similarly, since the European Union established the world's first emissions trading system (ETS) covering CO_2 emissions in 2005, the number of carbon markets has grown to 31, with the latest additions in China, the United Kingdom, and Germany in 2021. Carbon pricing initiatives now cover one-fifth of global greenhouse gas (GHG) emissions, or 12 gigatons (Gt) of CO_2 equivalent emissions annually. These initiatives raised public revenues totaling US$53 billion in 2020 (World Bank 2021).

However, behind the proliferation and popularization of the carbon pricing paradigm is a great uncertainty over its role in climate policy. Critics and endorsers alike concede that "optimal" pricing schemes that are cost-efficient and environmentally effective in theory may be politically unfeasible in practice (Rosenbloom et al. 2020a; Stiglitz 2019). A clash of paradigms persists regarding what this means in practical political terms (Rosenbloom et al. 2020b; van den Bergh

[1] The optimal carbon price is typically defined in relation to an ideal objective function that sets the carbon tax rate equal to the monetized damages associated with emitting an additional ton of CO_2, referred to as the "social cost of carbon" (SCC) (Gillingham and Stock 2018). However, global SCC estimates can be US$10/t$CO_2$ to US$1,000/t$CO_2$ and above due to the uncertainties inherent in damage function estimation and alternative ethical parameters (Adler 2017). For policymakers seeking guidance in setting the optimal price level, the unwieldy range of SCC estimates is unhelpful. This has prompted some economic policymakers to advance a target-based approach, whereby the appropriate price path is one that minimizes the cost of achieving a desired quantity of CO_2 reductions over a given period (Hepburn 2017).

and Botzen 2020). Under the 2015 Paris Agreement, 195 countries committed to mitigate against dangerous levels of climate change this century by maintaining global average surface temperatures below 1.5–2°C relative to preindustrial conditions, but this would necessitate a reduction in global emissions of around 50 percent by 2030 relative to 2020 (UNEP 2019).[2] In the Economists' Statement on Carbon Dividends (2019), which claims to be the largest public statement in the history of the economics profession, carbon pricing is hailed as the tool of choice to achieve these reductions at the "scale and speed that is necessary".[3] According to the Stern-Stiglitz High-Level Commission on Carbon Prices (2017), explicit carbon prices in the range of ≥US$40–80/t$CO_2$ by 2020 and ≥US$50–100/t$CO_2$ by 2030 will be "indispensable" to achieving the Paris Agreement goals, albeit with the proviso that they are combined appropriately with complementary policies.[4] However, such assessments have relied on *ex ante* calibrated model projections with limited empirical corroboration. For context, current carbon prices range from <$1/t$CO_2$ in Poland and Ukraine to $137/t$CO_2$ in Sweden (in nominal terms), and nearly half of all covered emissions worldwide are priced at less than $10/t$CO_2$ (World Bank 2021).[5] Globally, the average (emissions-weighted) carbon price is around $3/t$CO_2$ (Dolphin et al. 2020), equivalent to adding approximately US$0.03 per gallon of gasoline (€0.009 per liter of petrol).

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[2] This is a necessary but insufficient condition. A further requirement is that global emissions decline to net zero by around 2050–2070. Any irreducible positive emissions would need to be offset by a range of negative emissions technologies, none of which are a panacea and all of which face considerable biophysical limits, uncertain long-term costs, and political coordination challenges (Griscom et al. 2017; Hepburn et al. 2019; Chatterjee and Huang 2020; Smith et al. 2016).
[3] The statement (2019) includes among its signatories 3,589 US-based economists, four former chairs of the Federal Reserve, 27 Nobel Laureate economists, and 15 former chairs of the Council of Economic Advisers.
[4] As Stiglitz (2019) cautions, carbon price paths will inevitably vary across heterogeneous sociopolitical and economic contexts and, critically, "*there is no presumption that a carbon tax alone can suffice to address optimally the problem of climate change*" (emphasis in original).
[5] As of May 2020. All monetary units throughout this study are in 2015 US dollars.

Empirical evaluations of the impact of implemented carbon prices on CO_2 emissions have been mixed, inconclusive, and, until recently, strikingly scarce. We report the main empirical findings and evaluation methods of previous studies in Section 3. Our key takeaway from this burgeoning evaluation literature is that the fragmentary nature of the evidence precludes systematic inference about the likely response of emissions to carbon pricing across space and time. As we describe in Section 4, the paucity of cross-country empirical assessments is partly a function of the lack of standardized carbon price data adjusted to account for variation in industry exemptions, rebates, and sectoral coverage. But the empirical neglect can also be attributed to the considerable identification challenges, summarized succinctly by Mildenberger (2020):

> Carbon pollution levels are so overdetermined by diverse economic and social forces that retrospective causal identification of policy impacts remains difficult. Economists have offered evaluations of some policies, but these estimates are difficult to compare across countries and time. Nor can we reliably translate simple policy content metrics, like a national carbon price level, into units of carbon pollution reduced. Even identical carbon prices have different effects based on variation in sectoral cost exposure and sectoral differences in the elasticity of carbon-dependent activities.

Motivated by similar concerns, we present a viable empirical modeling approach that largely overcomes these identification challenges. Until recently, the persistent lack of standardized carbon pricing data has compelled researchers to rely predominantly on quasi-experimental methods to estimate generic "treatment effects" of carbon pricing without specifying the initial price level and its subsequent evolution over the treatment period. In effect, essential information about the dynamics and functional form of the relationship between the price level and emissions is ignored or omitted perforce. This has precluded pursuing conventional economic interest in estimating empirical elasticities (in this case, of emissions, with respect to heterogeneous carbon price levels observed across countries, sectors, and time). Furthermore, when treatment effects or elasticities are estimated, the focus has remained on their statistical rather than economic significance, with few empirically grounded studies assessing whether pricing is sufficient to achieve governments' emissions reduction commitments. The practical consequence is that policymakers and the public still know little about the environmental effectiveness of one of the core pillars of climate policy.[6]

We construct a novel dataset comprising average (emissions-weighted) carbon prices across five sectors for a panel of 39 countries that implemented a carbon price during 1990–2016 (and 164 other countries that did not), combined with emissions data from 1975–2016. We aim to answer three questions. First, does pricing carbon reduce emissions? In other words, what is the effect of the introduction of carbon pricing on CO_2 emissions, irrespective of the price level? Second, does the price level matter (do higher carbon prices lead to greater reductions)? Third, is carbon pricing sufficient to achieve international emission-reduction targets?

We report two sets of estimated effects for each sector. First, we estimate the average treatment effect of introducing a carbon price irrespective of the price level. To overcome challenges in identifying treatment effects using conventional difference-in-differences (DiD) and synthetic control approaches, we apply treatment evaluation methods accommodating staggered adoption (Xu 2017; Athey et al. 2018) and control for unobserved time-varying heterogeneity using

[6] Although much of the academic climate economics discourse has focused on estimating the social cost of carbon (with a view to designing socially optimal carbon pricing schemes), government discourse has shifted toward a more target-based approach since the Paris Agreement. For example, countries accounting for two-thirds of global emissions have announced commitments to achieve "net-zero" CO_2 emissions by midcentury or shortly thereafter, raising the question of whether carbon pricing can plausibly achieve the declared goals and, if not, what role it ought to play in the broader policy mix.

interactive fixed effects (Bai 2009). For completeness, we also report estimation results when using conventional two-way fixed effects (TWFE) and interactive fixed effects (IFE) estimators. We find that the average treatment effect implies a statistically significant 1.5 percentage point reduction in aggregate (national) CO_2 emissions growth relative to imputed counterfactual emissions. Notably, significantly greater average treatment effects have been generated in the electricity and heat sector (–2.5 percentage points relative to the counterfactual).

Second, we also propose a new approach to estimating elasticities from counterfactual estimators such as those based on synthetic control methods. Specifically, we estimate the (semi)elasticity of emissions with respect to the carbon price by assessing whether heterogeneity in treatment effects (estimated in the first stage) can be explained by variation in the treatment intensity provided by carbon pricing schemes observed within and between countries over time. In addition, we report elasticity estimates using simple TWFE and IFE models of the emissions response to the price level. Unlike previous empirical studies evaluating carbon pricing impacts, we explicitly estimate the distinct effects of mere policy introduction (regardless of the price level) versus effects attributable to the price level itself. We find that the (semi)elasticity effect is negative but imprecisely estimated for most sectors. Median estimates for aggregate emissions suggest a reduction of around 0.03 percent for each additional \$1/tCO_2, albeit with high uncertainty; these results are only statistically significant for the manufacturing sector (–0.16 percent for each additional \$1/tCO_2). Accounting for possible introduction effects, our results show that the price effect on CO_2 emissions is lower than found in previous studies. This suggests that merely introducing any nonzero carbon price reduces emissions, whereas higher price levels (as observed so far) yield only marginally larger reductions. Conversely, omitting introduction effects from models of the impact of carbon pricing may lead to biased estimates of the emission elasticity. To explain these results, we propose that the estimated introduction effect may elicit changes in realized CO_2 emissions by altering expectations about the future stringency of emission-reduction policies. This impact is intrinsically linked to how economic agents perceive the policy upon introduction.[7]

Third, to assess whether carbon pricing is sufficient to achieve stated emission-reduction targets, we combine our estimates of the introduction effects and emissions elasticities with climate model projections of CO_2 emissions from several indicative reference scenarios to study the emissions abatement potential of different hypothetical pricing schemes over the next three decades.

Our identification strategy attenuates multiple possible sources of carbon-price endogeneity in the following ways: first, by including relevant control variables known to influence national proclivities to introduce a carbon price[8], the level of the carbon price, and CO_2 emissions; second, by using fixed weights when constructing the emissions-weighted carbon price series, so that the computed prices (and associated exemptions) are independent of interannual changes in sectoral energy use and carbon intensities; lastly, by allowing for a multifactor error structure and applying the principal components approach of Bai (2009) to approximate unobservable time-varying common factors which are controlled for in our baseline model specifications (for estimating both average treatment effects and semielasticities).

[7] For instance, Linn and Li (2014) provide evidence that consumers respond more strongly to changes in gasoline taxes than changes in gasoline prices. One explanation they put forward is that consumers *perceive* taxes as more "stable" (whether they actually are is a different question). Although the evidence presented in Linn and Li (2014) is based on marginal changes in tax rates (and retail fuel prices), it seems plausible to expect similar effects upon the inception of new policy instruments.

[8] Including controls such as the value-added of sector-specific economic activities to GDP, and weather anomalies quantified as heating and cooling degree days.

The reported results are robust across estimation methods (synthetic controls, TWFE, IFE), a wide range of model specifications (including separately assessing carbon tax and trading schemes), and additional equilibrium correction (EC) specifications that accommodate global stochastic trends affecting CO_2 emissions. We arrive at an important result: carbon pricing at current observed levels, even if implemented globally, is unlikely to achieve emissions reductions at the scale and speed necessary to achieve the commitments of the Paris Agreement—or even substantial reductions at all. Achieving the requisite level of emission reductions requires global carbon pricing with near 100 percent emission coverage and in excess of $250/t$CO_2$.

It is unlikely that carbon pricing will reach such high average levels globally, but the fact that the introduction of pricing does reduce emissions means that it remains one of many important interventions to tackle anthropogenic climate change. In particular, our findings are consistent with the view that jurisdictions could achieve considerable emissions reductions by introducing carbon pricing mechanisms in sectors that are not currently subject to such policies.

After describing the core elements of carbon-pricing theory that inform our empirical investigation (Section 2) and reviewing empirical evidence from previous carbon-pricing impact assessments (Section 3), we describe the standardized sector-level carbon price data we constructed to estimate emissions elasticities (Section 4). We then explain our identification strategy, baseline model specifications, and multistage estimation procedure, summarizing at each stage the associated country- and sector-level results across 24 model specifications (Section 5). After describing the estimation procedure for simulating the potential response of future emissions to several possible price paths and summarizing the projected impacts (Section 6), we conclude with reflections on the overall policy implications of our full set of results (Section 7).

2. CO_2 Prices, Marginal Abatement Costs, and Emissions

Anthropogenic CO_2 emissions are primarily a by-product of the production process in certain "dirty" sectors of the economy, which implicitly defines a pollution demand schedule for that sector.[9] The quantity of CO_2 emissions generated by these sectors depends primarily on their absolute size, the cost of available CO_2 abatement technologies, and the explicit and implicit (shadow) price of emissions. Therefore, for a given set of CO_2 abatement technologies (assuming a static marginal abatement cost curve), a change in the carbon price is expected to induce changes in the size and/or emissions intensity of the polluting sectors, resulting in a change in CO_2 emissions "demanded" by those sectors.[10] The demand schedule for a rising carbon price is downward sloping and reflects the diminishing marginal value that the economy places on units of CO_2. This generic schema provides the theoretical foundation of our empirical investigation.

The empirical discussion requires some clarification regarding the functional form of the relationship. First, the pollution demand schedule can be reinterpreted as a marginal abatement cost

9. The pollution demand schedule indicates the response of a sector's emissions to a given price of emitting each unit of CO_2.

10. Under conditions of uncertainty around the demand schedule, the quantity of CO_2 emission reductions associated with a given carbon price will depend on the type of policy instrument the legislature or regulatory agency chooses. A strictly positive price signal should, in principle, trigger CO_2 abatement activity. However, if the marginal product of abatement is bounded above, then it is likely that firms and individuals will only undertake abatement activities if the carbon price is above a certain threshold (Copeland and Taylor 2003). The available evidence reviewed in Section III, however, suggests that carbon prices have triggered at least some CO_2 abatement.

schedule: given that the demand schedule provides information about the marginal willingness to pay for emissions, it also constitutes—when read in terms of CO_2 abatement—the marginal cost to the economy of restricting emissions. Theoretical discussions of the relationship between CO_2 emissions and their price often assume that it is nonlinear (Nordhaus 1993). That is, at levels of emissions close to an economy's business as usual (BAU) emissions, pricing CO_2 at a given rate will result in relatively large emission reductions, *ceteris paribus*. But at emission levels far from BAU, a similar increase in price will generate less CO_2 abatement (as the easier and cheaper abatement options have already been exploited). Studies of CO_2 abatement options have, however, found the marginal abatement cost curves for specific jurisdictions or regions to be mostly linear at low carbon prices, with costs rising steeply only toward the end of the curve (Goulder and Hafstead 2017). Empirical CO_2 demand schedules therefore appear to be much flatter than theoretically assumed, at least at the historically implemented carbon price levels considered herein (see Section IV). This has important implications for the empirical relationship to be expected between carbon prices and associated changes in CO_2 emission levels. We take this to suggest that, for the period analyzed here, the appropriate model specification may be linear. We return to the question of functional form in Appendix C with misspecification tests of our baseline model formulation; ultimately, the tests corroborate our initial conjecture that nonlinear relations are absent or nondetectable in the short sample and insignificant at hitherto observed carbon price levels. We conclude that a linear specification is appropriate.

3. Evidence from Previous Evaluations

Studies investigating the response of CO_2 emissions to a carbon price fall into two broad categories: (i) ex ante projections typically based on input-output models, computable general equilibrium (CGE) models, or large integrated assessment models (IAMs); and (ii) ex post evaluations using observational data, typically based on quasi-experimental, instrumental variable (IV), or panel regression methods. Most studies are in the former category, generating policy-response estimates whose wide range is largely a reflection of a priori assumptions regarding output and population size in baseline scenarios, future technology costs, and other unknown parameters, including the price elasticity of CO_2 emissions itself (for a range of perspectives, see, e.g., Barron et al. 2018; Fawcett et al. 2014; Goulder and Hafstead 2017; Edenhofer et al. 2010; Mercure et al. 2016; Ellerman and Buchner 2008).[11] Our study is concerned principally with retrospective policy evaluation, so we focus on ex post methods henceforth.

In contrast to simulation-based assessments, ex post evaluations have remained—until recently—comparatively scarce and rarely present elasticity estimates or counterfactual projections of emissions, despite their potential to provide more robust evidence about real-world policy impacts than can be obtained via theoretical considerations or ex ante projections alone (see, e.g., discussions in OECD 1997; Andersen 2004; Ekins and Barker 2001; Cropper et al. 2018). Consistent with this view, a recent assessment of British Colombia's carbon tax in Carbone et al. (2020) finds that the sign and magnitude of the policy coefficient(s) estimated via a reduced form

[11] The general tendency to rely on ex ante models is understandable given the data-related challenges of empirical carbon pricing evaluations (see Section III), the scarcity of real-world carbon pricing initiatives until the past decade or so, and the growing interest of policymakers in acquiring reasonable projections of the likely environmental and macroeconomic impacts of carbon pricing proposals over the coming decades.

econometric policy-response model correspond closely with those derived from a large CGE model, suggesting that the former are not distorted by general equilibrium effects and can provide empirical evidence that informs subsequent parametrization of the structural model.

The available evidence summarized in Table I has been mixed and somewhat inconclusive. Nevertheless, we can infer a few basic facts from this literature: (i) existing emissions response estimates are heterogeneous across regions and sectors, and it remains difficult to draw systematic comparisons of policy impacts across space and time; (ii) in general, ex post evaluations detect less CO_2 abatement than ex ante studies (but we hesitate to make any systematic comparisons given the fragmentary nature of the available evidence; for recent meta-analyses of the carbon-pricing evaluation literature, see Green (2021) and Lilliestam et al. (2021)); and (iii) researchers aspiring to attribute changes in emissions to carbon pricing instruments have typically adopted a quasi-experimental approach usually based on traditional DiD or synthetic control estimators, which restrict the policy variable to a binary specification.

As a final note concerning the evaluation literature: to the best of our knowledge, only one study, Best et al. (2020), has attempted to estimate emissions elasticities in a cross-country panel using standardized carbon price data, albeit over a shorter time horizon.[12] However, it does not estimate counterfactual emissions, relying instead on causal inference based on correlational evidence from TWFE panel regressions with numerous controls.[13] Furthermore, it does not differentiate between

[12] Best et al. (2020) use OECD data on “effective carbon rates,” but the available time horizon is short (2012–2017).

[13] The included control variables are GDP per capita growth, population growth, the net gasoline tax, fossil fuel subsidies, scores for energy efficiency and renewable energy policies, and a binary dummy indicating the presence/absence of feed-in tariffs.

introduction and price effects (which can bias elasticity estimates, as we find in Section 5), nor does it assess the estimated effect sizes in relation to emission-reduction targets.

Table I. Empirical evaluations of implemented carbon prices and associated CO_2 emission reductions

Study	Jurisdiction(s)	Period	Estimator	Policy instrument	Outcome variable	Change in emissions over entire period	Change in emissions per year
Abrell et al. (2011)	EU	2005–2008	Propensity score matching for priced and unpriced firms	EU ETS	CO_2 emissions growth rate (firm level)	–3 percent in ’07/’08 relative to ’05/’06 (–6 percent for firms with greatest decrease in free allocation)	N/A
Gloaguen and Alberola (2013)	EU	2005–2012	Propensity score matching	EU ETS	CO_2 emissions	–10 percent (100 $MtCO_2$) upper bound	N/A
Bel and Joseph (2015)	EU	2005–2012	Arellano-Bond IV with lags as instruments	EU ETS	Electricity and industry sector CO_2 emissions	33 to 41 $MtCO_2$ over 8 years due to ETS, or –12 percent from total	N/A
Dechezleprêtre et al. (2018)	EU	2005–2012	DiD	EU ETS	CO_2 emissions (plant level)	–6 percent during Phase I (2005–2007) and –15 percent during Phase II (2008–2012)	–2 percent during Phase I and –3 percent during Phase II

Bayer and Aklin (2020)	EU	1990–2016	GSC method with IFE model (synthetic control group composed of unpriced sectors)	EU ETS	Sector/industry CO_2 emissions (energy, metals, minerals, chemicals, and aggregate for priced sectors)	–7.5 percent (–1.2 Gt) on aggregate across priced sectors from 2008–2016	N/A
Klemetsen et al. (2016)	Norway	2001–2013	DiD	EU ETS	CO_2 emissions (plant level)	Significant reductions only during Phase II (2008–2012)	N/A
Dussaux (2020)	France	2014–2018	Regression-based counterfactual inference	Carbon tax	Manufacturing sector CO_2 emissions	N/A	–5 percent in 2018
Wagner et al. (2014)	Germany	1995–2010	DiD	EU ETS	CO_2 emissions (plant level)	–20 percent during Phase II	NA
Schäfer (2019)	Germany	2005–2015	Regression-based counterfactual	EU ETS	Electricity sector CO_2 emissions	<6 percent of total emissions; for 2005–2007: reduction of 10.5–31.4 $MtCO_2$; –2.0 percent emission intensity (first trading period), –2.9 percent (second trading period), –1.2 percent (years 1–3 of third trading period)	Time trend leads to –.5 percent of emissions intensity p.a.
Jaraite and DiMaria (2016)	Lithuania	2003–2010	DiD	EU ETS	CO_2 emissions (plant level)	Insignificant	Insignificant

Metcalf and Stock (2020b)	EU	1990–2018	Panel OLS with LP method and panel SVAR	Carbon taxes	Growth rate of total CO_2 emissions (country level)	–4 to –6 percent over 6 years for a \$40/tCO_2 tax covering 30 percent of CO_2 emissions	N/A
Murray and Maniloff (2015)	RGGI states (US)	1991–2012	Panel OLS with simulated counterfactual	RGGU	CO_2 emissions	–24 percent relative to counterfactual	
Martin et al. (2014)	United Kingdom		Two-stage least squares IV	UK Climate Change Levy	Manufacturing sector CO_2 emissions (plant level)	–7.3 percent	N/A
Abrell et al. 2020	United Kingdom	2013–2016	ML-based counterfactual inference	UK Carbon Price Support	Electricity sector CO_2 emissions (high frequency plant-level data)	–6.2 percent	N/A
Gugler et al. 2020	United Kingdom	2012–2016	RDiT	UK Carbon Price Support	Electricity sector CO_2 emissions (high frequency plant-level data)	–26.2 percent	N/A
Leroutier (2018)	United Kingdom		Synthetic control method (donor pool composed of EU countries)	UK Carbon Price Support	Electricity sector CO_2 emissions (high frequency plant-level data)	–49 percent	N/A
Andersson (2019)	Sweden	1960–2005	DiD and synthetic control	Carbon tax (transport sector)	Transport sector CO_2 emissions	N/A	–6.3 percent per year on average (1990–2005)
Lin and Li (2011)	Denmark, Finland, Netherlands, Norway, Sweden	Inception to 2008	DiD	Carbon taxes	Total per capita CO_2 emissions	N/A	–1.7 percent decline in growth rate in Finland only

Rivers and Schaufele (2015)	British Columbia	1990–2011	Panel model regression with simulated counterfactual	Carbon tax	Province-level CO_2 emissions from gasoline consumption relative to the rest of Canada	–2.4 Mt (over 4 years)	–0.6 Mt
Lawley and Thivierge (2018)	British Columbia	2001–2012 (2008–2012 treatment period)	DiD	Carbon tax	Province-level CO_2 emissions from gasoline consumption relative to the rest of Canada	–1.13 percent to –4.87 percent (5 years)	<–0.97 percent
Erutku and Hildebrand (2018)	British Columbia	1991–2015	DiD	Carbon tax	CO_2 emissions from gasoline consumption relative to the rest of Canada	–0.26 percent to 10.3 percent (5 years)	<–2 percent
Pretis (2019)	British Columbia	1990–2016	DiD, synthetic control, and break detection	Carbon tax	Aggregate and sectoral CO_2 emissions	–19 percent (DiD) and –3 to –15 percent (synth) for road transport CO_2 emissions (2008–2016)	+\$5/tCO_2 increase → –1 percent reduction in road transport emissions
Best et al. (2020)	42 countries	2012–2017	Cross-sectional and panel regressions with many controls	"Effective carbon rate" including taxes and ETSs	Growth rate of road transport CO_2 emissions and aggregate emissions of all nonroad sectors	–2 percent relative to countries without a price	–.03 percent for a €1/tCO_2 price increase
Runst and Thonipara (2020)	Sweden	1990–2016	DiD and synthetic control	Carbon tax	Emissions in the residential buildings sector	–200–800kg per capita in residential buildings	N/A

Colmer et al. (2020)	France (EU ETS)	1996–2012	DiD	ETS	Emissions and emissions intensity in manufacturing sector	N/A	–8.2 percent relative to unregulated firms and –10.7 percent emissions intensity of value added

4. Emissions-Weighted Carbon Price Data

Economic theory has long recommended using a single, uniform price signal to reduce CO_2 emissions at minimal cost,[14] provided that the public authority can credibly commit to an escalating price path (or declining emissions cap) and assuming the absence of transaction costs.[15] Contrary to "first-best'" theory, practical experience shows that governments are routinely constrained by domestic political economy constraints that inhibit optimal carbon pricing, and the transaction costs of implementing and sustaining carbon pricing instruments in some sectors are far from trivial.

[14] The externality associated with each ton of CO_2 emitted to the atmosphere is the same regardless of its source (i.e., country, sector, or technology). Therefore, assuming a policymaker wants to set the carbon price equal to the monetized damages from emitting an additional ton of CO_2, any departure from a single, economywide price signal will inevitably introduce distortions between sectors and/or types of consumers. Following these "first-best" policy prescriptions, the Integrated Assessment Models (IAMs) cited by the Intergovernmental Panel on Climate Change (IPCC) assume that implemented carbon prices are more or less economywide.

[15] If transaction costs (e.g., of monitoring and verifying emissions) are positive, then optimal coverage may not be 100 percent. In that case, emissions should be included only if the marginal benefit in terms of enhanced cost efficiency outweighs the marginal cost of monitoring and verifying emissions. If only CO_2 emissions are covered, various strategic points exist at which fossil fuels, for example, can be priced upstream, midstream, or downstream to minimize transaction costs. However, technical difficulties inhibit implementing schemes covering other greenhouse gases, so it might be suboptimal to aim for 100 percent coverage of GHG emissions.

From the United States and Brazil to India and Russia, the largest carbon-exposed businesses have invested in lobbying activities and tactical rent-seeking to prevent carbon pricing (Meng and Rode 2019; Stokes 2020; Mildenberger 2020; Martus 2019; Gershkovich 2019; Sengupta et al. 2019; Grubb 2014; Helm 2010; Jenkins 2014). Notably, this includes the organized opposition of peak business associations representing industries other than fossil fuels, which are exposed to carbon costs indirectly through extensive supply chain linkages (Cory et al. 2020). In large coal-producing countries with inordinately money-driven political systems, such as the United States and India, the role of campaign contributions during multibillion-dollar election cycles cannot be discounted as a considerable deterrent against raising climate policy as a central campaign issue (Ferguson et al. 2013; Chamon and Kaplan 2013). Beyond heeding the concerns of domestic industry, politicians of nearly all ideological stripes have been cautiously reluctant to rouse civic opposition from tax-averse voters to any salient rise in consumer energy prices that might be attributed to a carbon pricing scheme.

Such distributional effects, sometimes real but often exaggerated or contrived, account for persistently low prices and coverage (Grubb 2014; Helm 2010; Jenkins 2014; Dolphin et al. 2020). Hence, carbon taxes and ETSs have typically been implemented in a limited number of sectors and attenuated by industry exemptions, rebates, and omitted fuels (Metcalf and Weisbach 2009; Martin et al. 2014b; Edenhofer et al. 2014; OECD 2018). It is thus unsurprising that governments have sought to reduce aggregate emissions by employing a diverse mix of policy instruments,[16] the combined environmental impact of which could be similar to a single higher (and more blunt) carbon price but which may face less industry resistance. Furthermore, the use of multiple policy

[16] Examples include product standards, building regulations, emission limits for power plants, renewable energy auctions, R&D, grants and subsidies, public infrastructure investments, and product bans.

instruments may be intended to achieve multiple policy objectives simultaneously (e.g., governments have also cited goals of supporting R&D and industrial policy in nascent green technologies and reducing air pollution). The observed pattern is consistent with the principle, popularized by Tinbergen (1952), that we need at least as many policy instruments as market failures to be corrected.[17] Climate change need not be the only market failure.

This has introduced a major impediment to economywide (let alone cross-country) empirical evaluations of price-induced CO_2 emissions abatement. Coefficient estimates based on nominal price data are only robust and comparable if emissions coverage is assumed to be consistent across units and time,[18] which is compounded by the relatively short time (<5 years) covered by available carbon price data sources (OECD 2018; World Bank et al. 2018; World Bank 2021).

We overcome this impediment[19] by compiling *emissions-weighted carbon price* (ECP) data at a sector level for a panel of 39 countries from 1990 to 2016. The ECP data have been updated from the original aggregate (economywide) CO_2 prices presented in Dolphin et al. (2020). We apply the same methodology to obtain not only the aggregate (economywide) ECP series but also sector-level CO_2 prices for (i) electricity and heat, (ii) manufacturing, (iii) road transport, and (iv) commercial and residential buildings. The ECP in each sector *k* of each country *i* is computed using

[17] In the hypothetical situation where a policymaker wants to achieve only the goal of reducing aggregate CO_2 emissions, perhaps no other policy rivals a carbon tax in terms of its theoretical capacity to cover the entirety of emissions generated by an economy via a single, encompassing policy instrument.

[18] As the World Bank et al. (2018) emphasize: "Prices are not necessarily comparable between carbon pricing initiatives because of differences in the sectors covered and allocation methods applied, specific exemptions, and different compensation methods." Following standard practice, World Bank et al. (2018) present data on nominal carbon prices, which do not take into account these cross-national differences.

[19] In doing so, we avoid a particular type of violation of the "stable-unit-treatment-value" assumption, a required assumption when using any quasi-experimental estimator within the potential outcomes framework (see, e.g., the discussion in Frölich and Sperlich 2019).

coverage and price information at the sector-fuel level, in combination with sector-fuel CO_2 emissions data. A summary of the computation procedure is presented in Appendix A, and a full methodological description is available in Dolphin et al. (2020).

To the best of our knowledge, the ECP data constitute the first centralized and systematic assessment providing a consistent description of carbon prices that simultaneously provides price level information disaggregated at the sector level, extends back to 1990 to include price information for the earliest carbon tax policies, and accounts for as many sector (-fuel) exemptions as accurately possible.

A major benefit of the ECP is that it enables a consistent basis for measuring the price-induced incentive to reduce aggregate CO_2 emissions cross-nationally, making carbon prices truly comparable for panel econometric purposes.[20] Given that ECP data was unavailable until recently, previous ex post evaluations were limited to estimating treatment effects that capture the impact of policy implementation irrespective of the CO_2 price level.[21] This study goes one step further and estimates not only the generic treatment effect but also emissions elasticities with respect to the level and yearly change of prices. Our main results use our ECP data combining emissions trading schemes and carbon taxes. We consider results disaggregated by scheme type (ETS or carbon tax) in section 5.3.2 and Appendix F.

[20] Dolphin et al. (2020) originally developed the ECP data and methodology to identify the determinants of carbon price adoption and stringency (i.e., ECP as a dependent variable); we use the ECP for the first time as an independent variable.

[21] The few studies incorporating empirical information on carbon price levels within a quasi-experimental evaluation framework were confined to one (or a few) jurisdictions (e.g., Andersson 2019; Pretis 2019).

Table 2 highlights the disparity between nominal and emissions-weighted carbon prices. For example, Sweden's nominal price was \$130/tCO_2 in 2015, but its average ECP (accounting for exemptions and coverage restrictions) was approximately \$76/tCO_2. Likewise, Switzerland's highest nominal price in 2015 was \$50/tCO_2, but its average ECP was under \$15/tCO_2. A more granular look at the heterogeneity and dispersion of carbon price levels and coverage over time is provided via time-series heatmaps in Figure 1. Equipped with the ECP data, we proceed in Section 5 to describe our identification strategy, baseline model specifications, and multistage estimation procedure, presenting the results of each estimation stage along the way.

Table II.

Nominal vs. Emissions-Weighted Carbon Prices in Selected Jurisdictions, 2015 (US$/t$CO_2$)

	Nominal CO_2 price	Emissions-weighted CO_2 price	Percent difference
Denmark	26	21.38	–17.8
Finland	64	45.14	–29.5
France	16	8.77	–45.2
Germany	10	5.80	–42
Ireland	22	17.21	–21.8
Italy	9	4.70	–47.8
Japan	2	1.34	–37.8
New Zealand	5	4.53	–9.4
Norway	52	52	0
South Korea	9	7.66	–14.9
Sweden	130	114.80	–11.69
Switzerland	62	17.70	–71.45
United Kingdom	28	14.57	–47.96

Note: All prices are in 2015 US$. Nominal carbon price information is obtained from World Bank and Ecofys (2015) and based on the highest nominal price levied within the jurisdiction in 2015, without accounting for sectoral, industrial, or fuel-specific exemptions. The ECP values are based on the average (economywide) CO_2 price level.

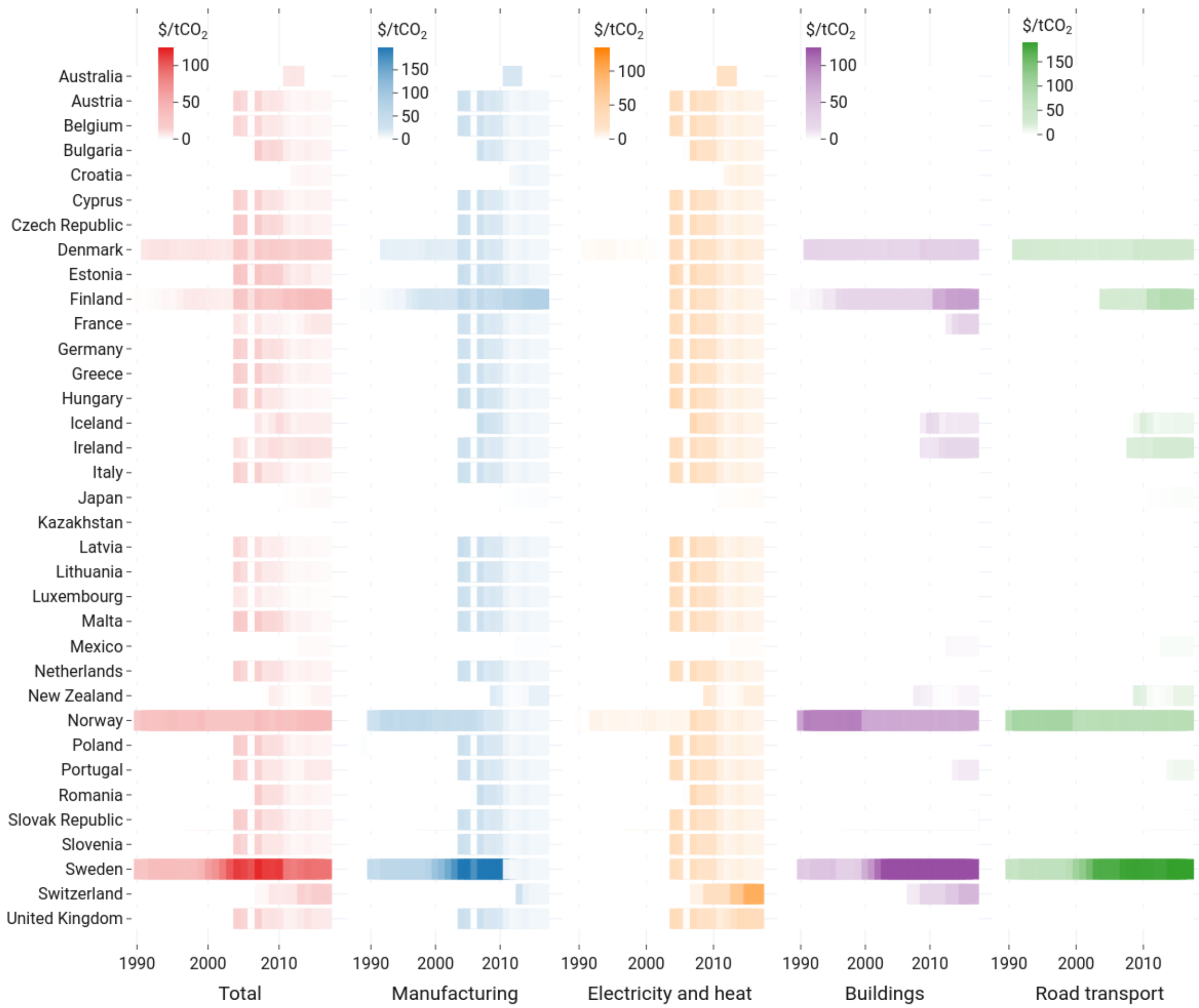

Figure I.

Carbon Price Coverage and Stringency Across Countries and Sectors (1990–2016)

Note: Color-coded tiles indicate a carbon pricing initiative (tax and/or ETS) in a given year, with darker tiles reflecting higher carbon price levels (2015 US$/t$CO_2$). Based on emissions-weighted carbon price data updated from Dolphin et al. (2020) for sectoral analysis.

5. Estimating the Impacts of Carbon Pricing

Using sector-level observations on emissions, we first estimate the average treatment effect on the treated of the introduction of carbon pricing on the growth rate of CO_2 emissions (irrespective of the price level) using TWFE, IFE, and generalized synthetic control methods for policy evaluation under staggered adoption (multiple treated units introduce the policy at varying points in time) (Xu 2017; Athey et al. 2019; see Section 5.1). Using ECP data, we quantify the semielasticity of CO_2 emissions with respect to the carbon price level, allowing for both introduction and price effects. We propose a new approach to estimate elasticities from counterfactual estimators (specifically, synthetic controls) by decomposing variation in the treatment effect using variation in the treatment intensity provided by different levels of carbon pricing (Section 5.2).

5.1 The Average Effect of Introducing a Carbon Price (Average Treatment Effect)

To understand the net impact of the introduction of carbon pricing irrespective of the price level, we focus on the sector-specific average treatment effect on the treated on the growth of CO_2 emissions. We first consider a simple TWFE model, which we expand to an IFE specification and a generalized synthetic control model to address concerns around estimating time-varying and heterogeneous treatment effects.

As a starting point, we consider a simple TWFE model of CO_2 emissions growth in country i, sector k, and year t:

$$\Delta\log(CO2)_{i,k,t} = \delta_k D_{i,k,t} + x'_{i,k,t}\beta + \xi_{i,k} + \tau_{k,t} + \epsilon_{i,k,t}, \quad (1)$$

$$\text{for countries } i \in 1,2, \ldots N_{co}, N_{co}+1, \ldots, N,$$

$$\text{sectors } k \in k_{manufacturing}, k_{electricity\_heat}, k_{buildings}, k_{road}, k_{total},$$

where $D_{i,k,t}$ is a treatment indicator denoting the presence or absence of a carbon price at time t, and δ_k denotes the parameter of interest—the sector-specific treatment effect, capturing the change in emissions attributed to the carbon price conditional on its introduction. Country-sector individual fixed effects are given by $\xi_{i,k}$, year-sector fixed effects are given by $\tau_{k,t}$, and $\epsilon_{i,k,t}$ denotes unobserved idiosyncratic mean zero shocks. We control for q observed time-varying covariates $x' = [x'_1, \ldots, x'_q]'$, including the country-level population growth rate, growth in real aggregate GDP (and its square), and growth in sector-level GDP (and its square) where available.[22] We investigate a wide range of specifications in robustness checks (Section V.4), including population-weighted heating and cooling degree days (HDD, CDD), as control variables to capture the impact of weather on energy demand and emissions (Mistry 2019).

The TWFE model in [1] includes country-sector and year-sector fixed effects; however, there may be a myriad of unobserved common shocks expressed as latent common factors and affect countries and sectors differently. We therefore expand [1] to an IFE (Bai 2009) where we treat the *r* latent common factors F_t and country-sector specific factor loadings $\lambda'_{i,k}$ as IFE parameters to be estimated as a means of controlling for unobserved heterogeneity:

[22] Additional covariates included in the sector-level models include manufacturing GDP, transport GDP for transport emissions, and services and retail GDP for building emissions (UNCTAD 2020a). See Appendix B for a summary of all observed covariates included in the model specifications.

$$\Delta\log(CO2)_{i,k,t} = \delta_k D_{i,k,t} + x'_{i,k,t}\beta + \xi_{i,k} + \lambda'_{i,k} F_{k,t} + \epsilon_{i,k,t} \tag{2}$$

The $(r \times 1)$ vector $F_t = [F_{1t}, \dots, F_{rt}]'$ denotes unobserved (latent) common factors that may be correlated with $\Delta\log(CO2)$, D, and x'; $\lambda_{i,k} = [\lambda_{i,k,1}, \dots, \lambda_{i,k,r}]'$ is an $(r \times 1)$ vector of unknown heterogeneous factor loadings. F_t may represent common shocks (e.g., international climate accords, pandemics, financial crises), unobservable national trends (e.g., motivation to mitigate climate change), co-movements in the volatility of international coal, oil, and gas prices, the confluence of deindustrialization in OECD countries and rapid industrialization in Asia, downward-sloping technology learning curves (e.g., solar PV, wind, and battery storage), or cross-sectionally correlated climatic trends (e.g., the effect of warmer temperatures on energy demand).

We are faced with multiple treated countries implementing carbon pricing schemes at different times and potentially exhibiting distinct pretreatment trends. Conventional TWFE and IFE estimators rely on the restrictive assumption of parallel trends in the outcomes of treated and control units. Further, both base specifications of the TWFE and IFE models in [1] and [2] are restrictive in terms of heterogeneity and stability of the treatment effects. The effect of carbon pricing on emissions growth may differ by country and be nonconstant over time. To allow for heterogeneity and time-varying treatment effects and relax the parallel trends assumption (and avoid problems of estimating treatment effects in staggered adoption settings; see, e.g., Goodman-Bacon 2021, de Chaisemartin and D'Haultfoeuille 2020, Baker et al. 2021, Callaway and Sant' Anna 2020), we employ recent developments in counterfactual estimation with staggered adoption.

Specifically, we apply the generalized synthetic control estimator proposed by Xu (2017) based on panel IFE models (Bai 2009). Intuitively, this approach uses the pretreatment period to estimate an

IFE model that is used to project an untreated counterfactual for the treated units. We also report results using the matrix completion estimator of Athey et al. (2018) in our robustness checks. We model the CO_2 emissions growth rate in sector k of country i at time t using an IFE model that can be written as

$$\Delta\log(CO2)_{i,k,t} = \delta_{i,k,t}D_{i,k,t} + x'_{i,k,t}\beta + \xi_{i,k} + \lambda'_{i,k}F_{k,t} + \epsilon_{i,k,t} \qquad (3)$$

$$\text{for countries } i \in 1,2, \ldots N_{co}, N_{co}+1, \ldots, N,$$

$$\text{sectors } k \in k_{manufacturing}, k_{electricity\_heat}, k_{buildings}, k_{road}, k_{total},$$

where the treatment effect, $\delta_{i,k,t}$, may be heterogeneous over i and potentially time varying, capturing the change in emissions attributed to the carbon price conditional on its introduction. The baseline model specification includes unit fixed effects, $\xi_{i,k,}$ which enter the model additively, and factors F capture potential common latent trends. Our base model includes both unit and time fixed effects, and we assess the robustness of our results to the choice of fixed effects in Section 5.3.

Bai (2009) shows that when T is large and of comparable size to N, as here, least squares estimation of model (1) is robust to serial correlation and heteroskedasticities of an unknown form in the idiosyncratic errors.[23] As in Bai (2009), we make no assumption about whether F_t and $\lambda'_{i,k}$ have a zero mean or are independent over time. F_t may affect CO_2 emissions only, but it also may correlate with treatment assignment D, the carbon price level $p_{i,k}$, and/or the observed control variables $x'_{i,k,t}$. The factor loadings, $\lambda'_{i,k}$, capture the heterogeneous effects that the common factors generate in each country and sector. Although F are unobserved and their true number, r, is unknown when

[23] This contrasts with first-generation factor models wherein the lack of identification is well known.

estimating β (and vice versa), we can impose an initial estimate of r and proceed to jointly estimate $\hat{\beta}$, $\hat{F}$, and $\hat{\Lambda}$ by solving the least squares objective functions in Bai (2009) until the sum of squared residuals is iteratively minimized.[24] To capture the (potential) multidimensionality of the factor structure without overfitting, we use an algorithm to select the optimal number of factors (between 1–3) for each model iteration using the cross-validation procedure described in Xu (2017).

Models (1–3) can accommodate the theoretical schema described in Section 2, where the quantity of CO_2 emissions generated by each sector in a given year depends primarily on the sector's absolute size, the cost of available CO_2 abatement technologies, and the explicit and implicit (shadow) price of emissions. We require, however, some further assumptions.

> ASSUMPTION 1. The idiosyncratic errors, $\epsilon_{i,k,t}$, are independent of the policy treatment, conditional on the observed covariates, latent factors, and factor loadings, $\mathbb{E}\left[\epsilon_{i,k,t} \middle| D_{i,k,t}, x_{i,j,t}, f_t, \lambda_{i,k}\right] = \mathbb{E}\left[\epsilon_{i,k,t} \middle| x_{i,k,t}, f_t, \lambda_{i,k}\right] = 0$.

This strict exogeneity assumption is needed in order for the carbon pricing treatment effect, $\delta_{i,k,t}$, to be identified despite the presence of unmeasured country-specific confounders, including the unknown CO_2-equivalent shadow price signal, endogenous technical change, and other time-varying idiosyncrasies specific to each jurisdiction. Assumption 1 permits the treatment indicator $D_{i,k,t}$ to be correlated with $x_{i,j,t}$ and f_t.

[24] See Bai (2009) for a full methodological description.

ASSUMPTION 2. Transitory shocks in $\epsilon_{i,k,t}$ are cross-sectionally independent, such that any unobserved common factors and heterogeneities that have a substantive bearing on emissions in model (1) are captured or closely approximated by the additive (time and unit) fixed effects τ_t and $\xi_{i,k}$, or the multiplicative factor structure, $\lambda'_{i,k}f_t$.

To the extent that this assumption holds, the IFE estimator effectively obviates endogeneity concerns related to (potential) presence of unobserved common factors and time-varying heterogeneity correlated with the observed covariates (Bai 2009). Under analogous assumptions, the IFE estimator has been used to mitigate cross-section dependence and endogeneity biases in studies estimating the effects of spillovers on private returns to R&D (Eberhardt et al. 2013) and divorce law reforms on divorce rates (Kim and Oka 2014), among others. Gobillon and Magnac (2016) provide Monte Carlo evidence showing that the conventional DiD estimator is generically biased in the presence of common error components, whereas the synthetic control method performs relatively well under specific conditions and the IFE estimator usually produces the least bias.

ASSUMPTION 3. The absolute size of each sector $k \in k_{manufacturing}, k_{electricity\_heat}, k_{buildings}, k_{road}, k_{total}$ is independent of the carbon price.

We capture the size of the sector by controlling for sector-level GDP growth, total GDP growth (and their squares to allow for nonlinear relationships), and population growth, which are denoted by $x'_{i,k,t}$ in equation (1). To satisfy strict exogeneity, we require that sector-level and total GDP growth are invariant to introducing the carbon price and the price level itself. There is little evidence that carbon prices had discernible impacts on countries' GDP, positive or otherwise. The simulation evidence in Goulder and Hafstead (2017) and the empirical evidence in Metcalf and Stock (2020; 2020b) reassure us that any inferable impact of a carbon price on GDP is likely to be negligible, at least with respect to the historically observed price levels considered here. This assumption is plausible for the period under consideration, but it might be violated in the future if more stringent carbon prices are implemented. We therefore also report results omitting GDP growth as controls in Section 5.3. Further, we note that Metcalf and Stock (2020) show, using local projections, little evidence of feedback of emissions or GDP on the carbon price level (or, by extension, the introduction of pricing itself).

We follow Xu (2017) in extending the IFE estimator of Bai (2009) to the quasi-experimental framework using synthetic controls (Abadie et al. 2010, 2015; Billmeier and Nannicini 2013). The resulting generalized synthetic control method can be understood as a bias-corrected version of the IFE estimator that can accommodate both cross-sectional and temporal heterogeneity in the treatment effects. In a first step, the IFE model is estimated using only control group data. Having obtained a fixed number of latent factors, factor loadings are estimated for each treated country by linearly projecting their pretreatment outcomes onto the space spanned by these factors. In a final step, the counterfactuals for treated units are estimated based on those factors and factor loadings obtained in the previous step. Like the original synthetic control method, countries in the donor pool are weighted using pretreatment outcomes in the treated country as the benchmark. The imputed counterfactuals for treated countries are estimated using cross-sectional correlations between treated and control group countries.[25]

[25] The GSC method differs from the conventional synthetic control approach in that it employs dimension reduction to smooth vectors for the control group prior to reweighting (Xu 2017).

To estimate counterfactual emissions, we extend our dataset further back to 1975 or 1980, based on data availability. If the weights assigned to each control unit successfully produce a synthetic control group that closely predicts the treated unit's CO_2 emissions during the pretreatment period, we can have greater confidence that the posttreatment counterfactual can serve as a credible baseline to assess the effect of the carbon-pricing intervention. Tests of "no treatment effect" based on synthetic controls can be extremely oversized (and thus misleadingly rejected) if nonstationarity is ignored (Masini and Medeiros 2020), so we focus on specifications in first differences (growth rates of CO_2 emissions). Unit root tests confirm that observed CO_2 emission levels are $I(1)$ nonstationary but become stationary in first differences (see Appendix C). To differentiate between level and growth effects, we introduce lags of the emissions growth rate; their sign allows us to determine whether pricing affected primarily the growth or level of CO_2 emissions.

Our model (3) using the IFE estimator (Bai 2009) in a generalized synthetic control framework (Xu 2017) yields estimates of the sector-, country-, and time-specific treatment effects $\delta_{i,k,t}$. We report the average treatment effect over treated countries for each sector and each period as

$$\widehat{ATT_{t,k}} = \frac{1}{n_{Tr_{k,t}}} \sum_{i \in Tr} \hat{\delta}_{i,k,t} \tag{4}$$

where $n_{Tr_{k,t}}$ is the number of treated countries in each sector and year, and the overall average treatment effect for each sector is given by the weighted average of $\widehat{ATT_{t,k}}$ over all treated periods. We conduct inference on $ATT_{t,k}$ and $\hat{\delta}_{i,k,t}$ using a nonparametric bootstrap.[26] The "base"

[26] All models are estimated using the gsynth (Xu and Liu 2018) and lfe (Gaure et al. 2013) packages in R for a range of specifications to assess the robustness of the results (see Section 5.3 for robustness checks).

specification reported here includes IFE and additive two-way (country and time) fixed effects, restricts the treated countries to those with pretreatment data spanning a minimum of 15 years, requires countries in the control group to have average population, real GDP, and emissions levels at least as high as the lowest average in the treatment group, allows for 1–3 common factors (determined using cross-validation), and requires at least five years of treatment. For completeness, we also report the estimates obtained using the TWFE and IFE models in (1) and (2). We investigate a wide range of model specifications in our robustness checks (Section 5.3).

5.1.1 Results: The Average Effect of Introducing a Carbon Price (Average Treatment Effect)

Estimation results show that the introduction of carbon pricing resulted in a significant decrease in the growth rate of CO_2 emissions (Table III and Figure II) relative to the estimated counterfactual. The average treatment effect over treated countries and periods estimated using generalized synthetic controls suggests that growth in total CO_2 emissions is roughly 1.6 percentage points (SE = 0.8 points) lower compared to the estimated counterfactual. Results at the sector level indicate that emissions growth is 2.8 percentage points (SE = 1.3 points) lower for electricity and heat, 1.4 percentage points (SE = 1.7 points) lower for manufacturing, 0.5 percentage points (SE = 1.6 points) lower for road transport, and 1.1 percentage points (SE = 1.1 points) lower for buildings. These results are robust across a wide range of model specifications and estimation methods (see Section 5.3 for robustness checks); Appendix D reports TWFE and IFE estimates.

Figure IV shows the estimated treatment effects for each treated country by sector. Strikingly, treatment effects do not appear to vary much over observed price levels, an initial finding that we investigate further in Section 5.2 on elasticities. For now, we simply note that the ostensible invariance of estimated country-sector treatment effects to the country-sector average carbon price level seems to contradict conventional wisdom, which assumes that higher prices should lead to discernably larger quantities of avoided emissions on average, *ceteris paribus*.

Our estimation results suggest that the introduction of carbon pricing primarily affects the growth rate of CO_2 emissions rather than the level. Visual inspection of the time-varying treatment effects shows a persistent difference between the observed and counterfactual growth rate, rather than a one-off change (which would correspond to a level change). We further estimate the TWFE (1) and IFE (2) models including lags of the treatment indicator. If the effect was on the level rather than the growth rate, we would expect opposite-signed coefficients on the contemporaneous and lagged treatment dummy, which does not occur, supporting the interpretation that carbon pricing primarily impacts emissions via growth rather than level effects (see Appendix D).

Table III: Average Treatment Effects of the Introduction of Carbon Pricing [Dependent Variable: $\Delta\log(CO2)_{i,k,t}$]

	Total	Electricity and heat	Manufacturing	Road transport	Buildings
ATT	–0.016 (0.008) [p = 0.05]	–0.028 (0.013) [p = 0.03]	–0.014 (0.017) [p = 0.44]	–0.005 (0.016) [p = 0.65]	–0.011 (0.011) [p = 0.42]
Δlog(GDP)	0.40207	–0.59072	–0.47818	–0.03008	–1.81785
	(0.45619)	(1.0307)	(1.59878)	(0.49753)	(2.35302)
$\Delta\log(\text{GDP})^2$	–0.00495	0.0443	0.03174	0.01849	0.08496
	(0.01871)	(0.04288)	(0.06822)	(0.02258)	(0.09644)
Δlog(population)	0.39359	0.22088	–0.06424	0.22569	1.37189
	(0.15253)	(0.24974)	(0.53198)	(0.18432)	(0.56168)
Δlog(servicesGDP)					0.9296
	NA	NA	NA	NA	(1.01796)
$\Delta\log(\text{servicesGDP})^2$					–0.03912
	NA	NA	NA	NA	(0.0515)
Δlog(manfacturingGDP)			1.65345		
	NA	NA	(0.69482)	NA	NA
$\Delta\log(\text{manfacturingGDP})^2$			–0.06557		
	NA	NA	(0.03841)	NA	NA
Δlog(transportGDP)				0.13592	
	NA	NA	NA	(0.1421)	NA

Δlog(transportGDP)^2				–0.0013	
	NA	NA	NA	(0.00861)	NA
Δlog(heatingdegreedays)	NA	NA	NA	NA	NA
Δlog(coolingdegreedays)	NA	NA	NA	NA	NA
r	1	1	1	1	1
N_{TR}	17	16	16	6	7
N_{CO}	29	27	27	21	40
Specification #	1	1	1	1	2

Note: Bootstrap standard errors are shown in parentheses, with the bootstrap *p*-value for the ATT reported in square brackets. Section 5.3 shows results with heating and cooling degree days. Table V presents specifications; we report results using specification #2 for the buildings sector to ensure a sufficient number of treated countries.

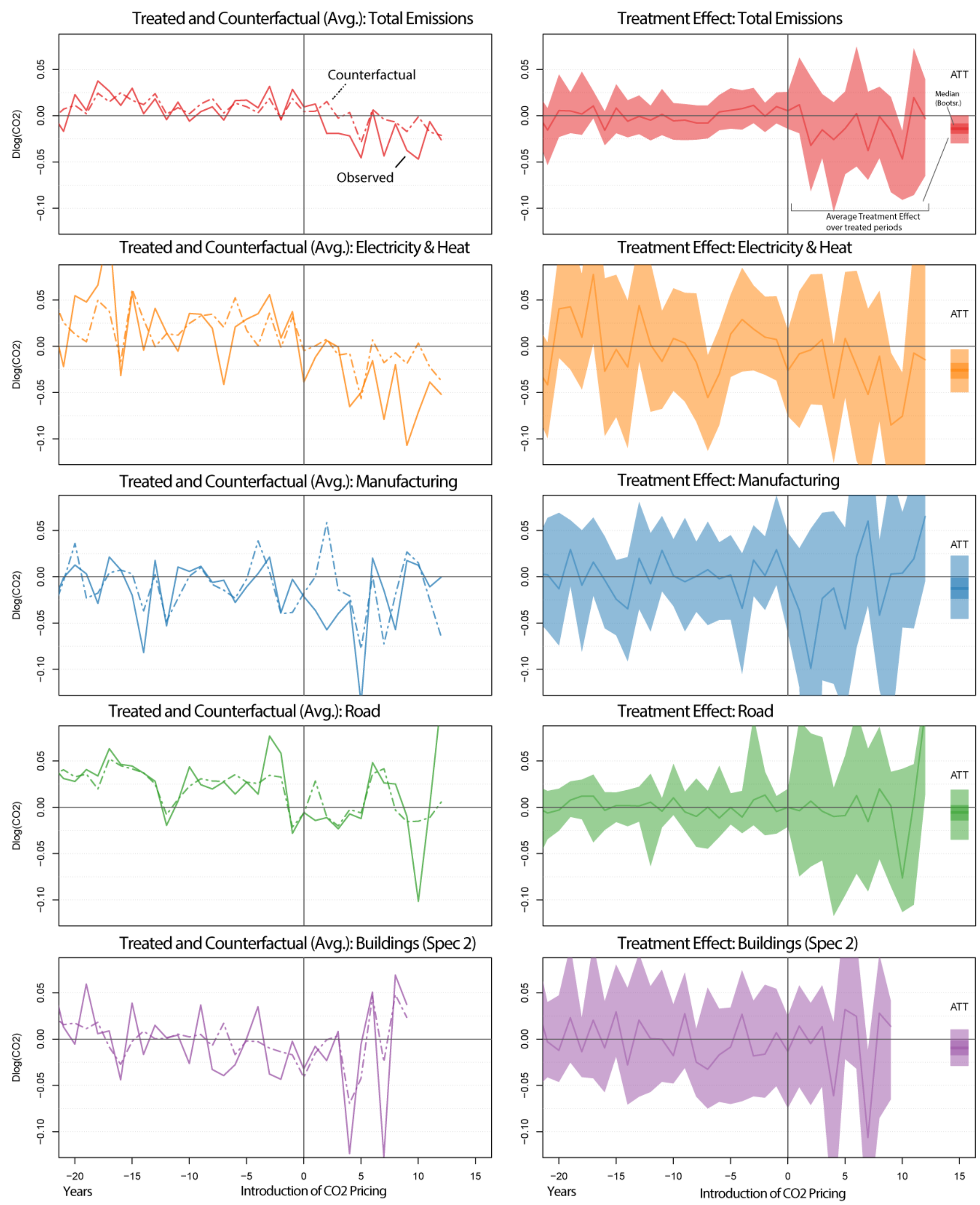

Figure II.

Generalized Synthetic Control Estimates of Average Treatment Effects

Note: Left panels show observed (solid) and counterfactual (dashed) change in log emissions by sector. Right panels show the estimated treatment effects as the difference between observed and counterfactual, with the estimate of the average treatment effects and its 95 percent bootstrap confidence interval (shaded).

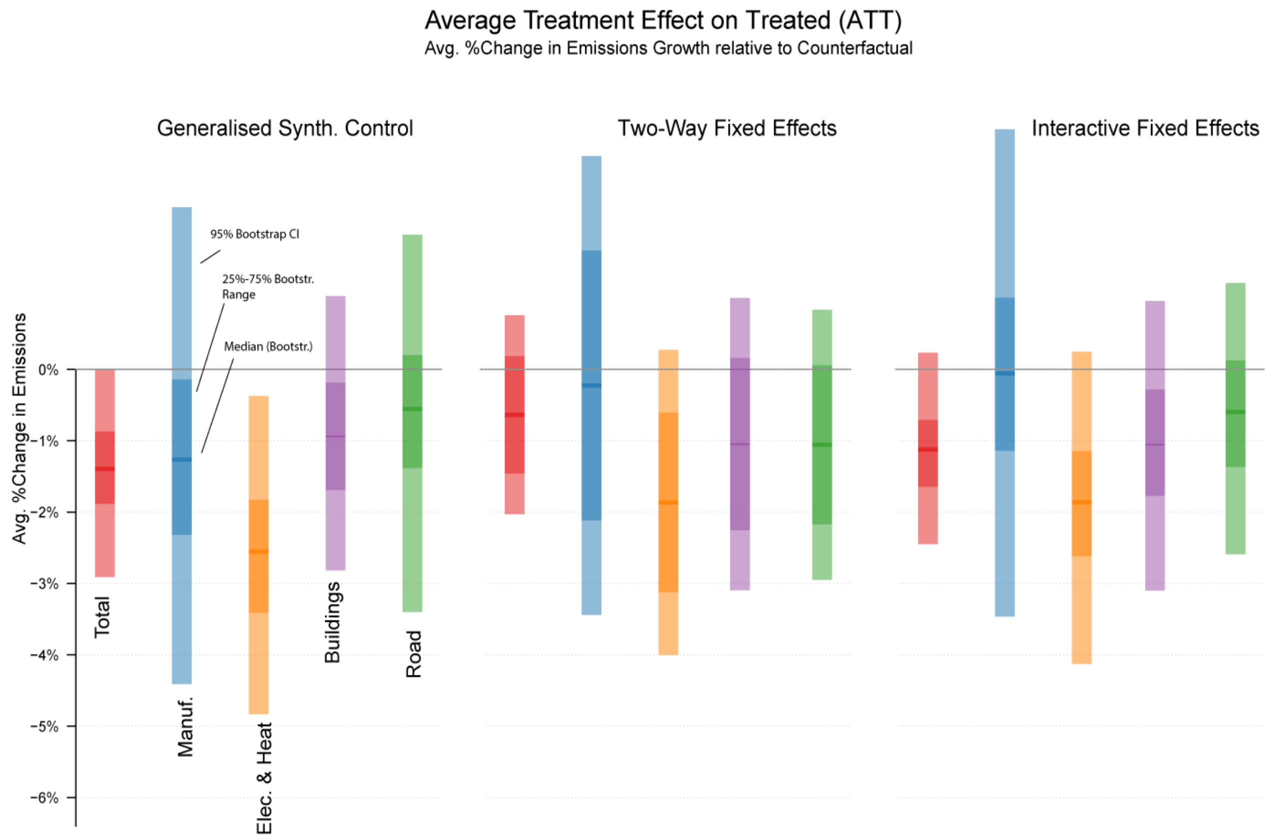

Figure III.

Average Treatment Effects on Treated By Sector: Generalized Synthetic Control (left), Two-Way Fixed Effects (middle), and Interactive Fixed Effects (right)

Note: Estimates for buildings sector given for specification #2.

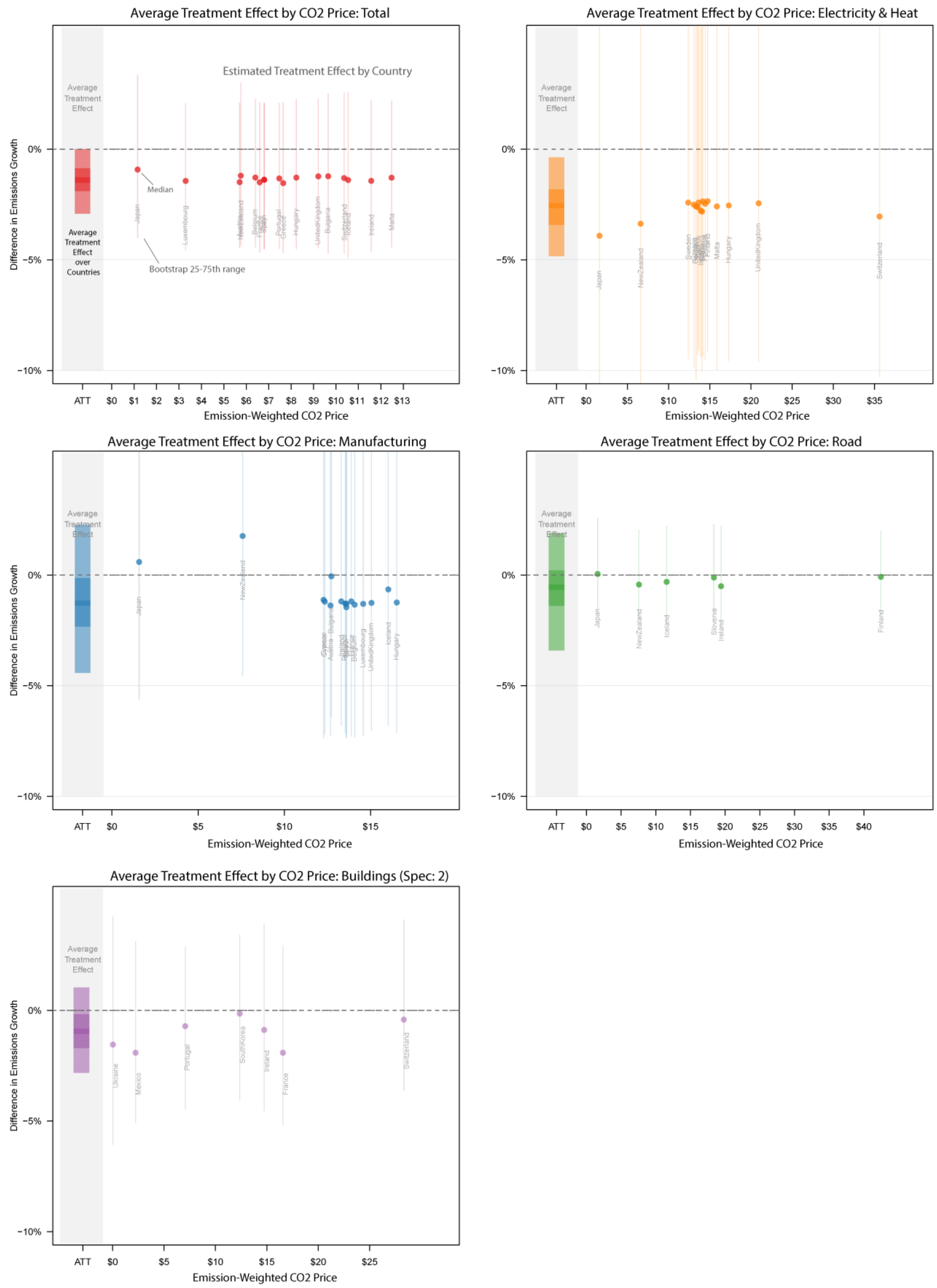

Figure IV.

Average Overall Treatment Effects and Between-Country Variation in Treatment Effects By Sector and Over Average Observed Carbon Price Levels

Note: Panels show the distribution of average treatment effects of each treated unit plotted against the average carbon price levels for different sectors. Average treatment effects (across treated units) obtained from the generalized synthetic control analysis are shown as bars with the 95 percent bootstrap confidence interval (shaded).

5.2 The Effect of the Carbon Price Level (Semielasticity)

The estimated average treatment effects indicate that the introduction of carbon pricing resulted in a decrease in the growth of CO_2 emissions. However, it is not clear whether higher price levels result in larger emission reductions. Merely introducing any nonzero carbon price might drive the apparent reductions by altering expectations (see e.g., Fried et al. 2020). We refer to this as the "introduction effect." A concern is that simple treatment effect estimates, $\hat{\delta}_{i,k}$, obtained using TWFE, IFE, or the generalized synthetic control approach, do not allow us to differentiate between the emission reductions stemming from the introduction effect versus from a given price level (the price effect). To assess whether higher price levels lead to larger reductions in emissions requires an estimate of the (semi)elasticity of emissions with respect to the (emissions-weighted) carbon price.

We decompose the treatment effect into introduction ($a_{i,k}$) and price (b_k) effects to estimate the emissions elasticity with respect to the carbon price. The (potentially heterogeneous and time-varying) treatment effect, $\delta_{i,k,t}$ ($\delta_{i,k,t} = \Delta\log(CO_2)_{i,k,t|D_{i,t}=1} - \Delta\log(CO_2)_{i,k,t|D_{i,t}=0}$), captures the difference in emissions growth resulting from introducing carbon pricing, relative to the "no policy" counterfactual. The treatment effect is potentially a function of a sector-specific introduction effect ($a_{i,k}$), semielasticity with regard to the carbon price ($b_{i,k}$), and the price level itself ($p_{i,k,t}$):

$$\delta_{i,k,t} = f(a_{i,k}, b_{i,k}, p_{i,k,t}) \tag{5}$$

We consider a linear model for $\delta_{i,k,t}$ estimating sector-specific effects:

$$\delta_{i,k,t} = a_k + b_k \times p_{i,k,t} \tag{6}$$

Here, a_k denotes the effect of introducing any carbon price in sector k, and it captures the impact on expectations generated by the introduction of a carbon price, regardless of the price level. Our main parameter of interest is b_k, denoting the (semi)elasticity of CO_2 emissions with respect to the carbon price, $p_{i,k,t}$. If b_k is negative, then a higher carbon price would lead to larger reductions in emissions beyond mere introduction effects. Naturally, a myriad of possible extensions exist, allowing the introduction and price effects to vary over *i*, or for different functional forms, such as including lags of prices to capture potential growth instead of level effects.

We expand the TWFE, IFE, and synthetic control models to estimate both the introduction and price effects. This is straightforward for the TWFE and IFE models. We consider heterogeneity over *i* as well as temporal-dynamic effects to differentiate between level and growth effects in our robustness checks. We propose a novel approach to estimate elasticities and introduction effects from counterfactual estimators (i.e., the synthetic control model here) in Section 5.2.1.

For the TWFE and IFE models, we substitute our expression for $\delta_{i,k,t}$ from equation (6) into equation (1), resulting in

$$\Delta log(CO2)_{i,k,t} \; = \; a_k D_{i,k,t} + b_k (p_{i,k,t} D_{i,k,t}) + \; x_{i,k,t}^{'} \beta + \; \xi_{i,k} + \; \tau_{k,t} + \; \epsilon_{i,k,t} \tag{7}$$

and into equation (2), resulting in

$$\Delta \log(CO2)_{i,k,t} = a_k D_{i,k,t} + b_k (p_{i,k,t} D_{i,k,t}) + x'_{i,k,t}\beta + \xi_{i,k} + \tau_{k,t} + \lambda'_{i,k} F_{k,t} + \epsilon_{i,k,t} \quad (8)$$

Models (7) and (8) thus include country-sector specific dummy variables capturing treated countries posttreatment (with coefficients a_k denoting the introduction effects), and country-sector specific dummy variables interacted with the carbon price levels ($p_{i,k,t}$), and associated coefficients b_k denoting the emissions semielasticities with regard to the price. Best et al. (2020) estimated models comparable to the TWFE model including the price level only (i.e., equation (7) but with the introduction effect term, $a_k D_{i,k,t}$, removed) but did not account for possible introduction effects. This risks confounding the introduction effect with the price effect. Thus, omitting the introduction effect term may bias the estimate of the emissions elasticity, b_k.

To differentiate between impacts on the emissions level versus growth rate and test for potential lagged price effects, we also estimate TWFE model (7) and IFE model (8) including lags of $p_{i,k,t}$ (see Appendix D). To estimate elasticities, we require the additional exogeneity assumption for the level of carbon pricing:

> ASSUMPTION 4. The level of the carbon price at time t is independent of $\Delta log(CO2)_{i,k,t,\ldots,t-L}$, conditional on the set of observed regressors, additive fixed effects, and estimated factor structure $\lambda'_{i,k} f_t$.

In other words, we assume that changes in the carbon price are strictly exogenous. This assumption is arguably reasonable, as many pricing schemes have committed changes in advance, and price changes are unlikely to be driven by contemporaneous growth in CO_2 emissions.[27] Several considerations support this assumption. First, economists have explicitly recognized the long time lags between (uncertain) CO_2 emissions outcomes and politically initiated adjustments to the carbon tax rate (or the emissions cap for carbon markets). To mitigate the uncertainty about policy efficacy created by these time lags, Hafstead et al. (2017), Metcalf (2020) and related studies have proposed methods of redesigning carbon pricing schemes so that they include built-in price-adjustment mechanisms that respond to unanticipated emissions outcomes, thereby providing assurance that carbon price levels can be preemptively adjusted in accordance with specific emission-reduction targets. To the best of our knowledge, such autonomous CO_2 price-adjustment mechanisms have yet to be adopted in any jurisdiction.[28] Furthermore, we have not identified a single case where policymakers have manually adjusted the carbon tax rate (or emissions cap) as a *contemporaneous* response to unanticipated changes in emissions.[29] In their broad macroeconometric analysis, Metcalf and Stock (2020) detect little to no evidence of feedback between CO_2 emissions (or GDP) and the level of the carbon price.

In ETSs, the issue of simultaneity is more complex. Economic theory would suggest a priori that the CO_2 permit price should respond to overachievement or underachievement of emissions

[27] An alternative to the IFE model here would be to use the local projection method in Metcalf and Stock (2020; 2020b).

[28] The Market Stability Reserve in the EU ETS comes close to an autonomous price-adjustment mechanism, but this is scheduled for 2023 onward and does not affect the period considered in this study.

[29] One possible exception is Australia, in which the federal government repealed a carbon tax in 2014 that had been implemented just two years earlier, arguably in response to the tax having imposed substantive policy costs on carbon-exposed industry. However, for our purposes, this case poses no problem and does not violate strict exogeneity, as the year of the tax repeal simply marks the end of the treatment period.

abatement with respect to the cap set by regulators. However, a compelling body of empirical evidence indicates that occasional bouts of volatility and nonstationarity in CO_2 permit prices in the EU ETS since 2005 have predominantly been a function of exogenous events—unanticipated regulatory changes and policy announcements regarding the allocation and banking of allowances—and the CO_2 permit price is poorly predicted by market fundamentals, negative demand shocks, or lagged emissions (Koch et al. 2014, 2016; Friedrich et al. 2019). These regulatory events or "shocks"[30] are best understood as the product of protracted negotiations with emissions-intensive and trade-exposed industries—often resulting in substantial overcompensation (Grubb 2014; Martin et al. 2014b)—rather than contemporaneous responses to overachievement or underachievement under the cap. For extended periods, the EU carbon market has been stationary at low CO_2 prices, only occasionally undergoing periods of volatility in response to politically determined (rather than "emissions determined") changes in the expectations of market participants, at least for the period considered in our study.[31]

[30] For example, Friedrich et al. (2019) model EU ETS price volatility in response to the March 2018 amendment passed by the European Commission, which announced plans to cancel excess allowances from 2023 onward under a Market Stability Reserve. Another major regulatory change to the EU ETS, the introduction of the linear reduction factor, is modeled in Bocklet et al. (2019).

[31] Our argument relates to a key point in Sims (1983): "[t]he fact that some effects of a policy action occur through effects on expectations does not necessarily imply that one must explicitly identify the parameters of expectation-formation mechanisms to obtain models that correctly project the effects of the action."

5.2.1 Elasticity Estimates Using Treatment Effects from Synthetic Controls

It is straightforward to modify the TWFE and IFE models to allow for introduction and price effects and then estimate the semielasticity with regard to the carbon price, but these models still potentially suffer the same challenges as the originals in (1) and (2), including heterogeneous treatment effects, nonparallel trends, and staggered adoption. Ideally, we could use the synthetic control treatment estimates from (3) when estimating elasticities and introduction effects. In this section, we propose a novel approach to estimating elasticities using treatment effect estimates obtained from counterfactual estimators, such as synthetic control and related methods.

Few studies have estimated elasticities directly from the treatment effects obtained from counterfactual estimators. Dube and Zipperer (2015) and Cengiz et al. (2019) are notable exceptions. The authors estimate elasticities using multiple treatment estimates (obtained via synthetic control methods—one for each treated unit[32] in Dube and Zipperer) scaled by the magnitude of treatment to assess whether changes in unemployment can be attributed to the magnitude of changes in minimum wages. Their application focuses solely on existing minimum-wage policies, thus avoiding the challenge of separating introduction from price effects.

Our proposed approach is to model variation in the country-specific treatment effects using observed variation in the carbon price levels within and between countries over time. Specifically, we assess whether heterogeneity over i (and t) in the estimated treatment effect $\hat{\delta}_{i,k,t}$ obtained using

[32] Combining multiple synthetic control estimates to conduct inference on an average treatment effect is an approach that also been applied by Isaksen (2020) for pollutant emissions and Gobillon and Magnac (2016) for unemployment.

synthetic controls (as in Section 5.1) can be attributed to variation in observed carbon prices and their interannual trajectories over relevant treatment periods. We estimate this elasticity using both between- and within-country variation.

5.2.1.1 Elasticity Estimates Using Between-Country Variation

To estimate the (semi)elasticity of CO_2 emissions growth with respect to the carbon price using between-country variation, we model the estimated sector-specific treatment effect from the synthetic control model (3) for each country i averaged over time[33], $\bar{\hat{\delta}}_{i,k}$, as a function of the average carbon price level $\bar{p}_{i,k}$ of country i:

$$\bar{\hat{\delta}}_{i,k} = a_k + b_k \bar{p}_{i,k} \tag{9}$$

where $\bar{\hat{\delta}}_{i,k} = \frac{1}{T_{tr,i,k}} \sum_{t=1}^{T_{tr,i,k}} \hat{\delta}_{i,k,t}$,

and $\bar{p}_{i,k} = \frac{1}{T_{tr,i,k}} \sum_{t=1}^{T_{tr,i,k}} p_{i,k,t}$,

with b_k denoting the parameter of interest: the change in the average sector-level treatment effect (i.e., change in the growth rate of CO_2 emissions) in response to a $1 increase in the average emission-weighted carbon price. This approach is closely related to Cengiz et al. (2019), who scale their minimum-wage treatment effects by the level of the minimum wage. The equivalent approach in our setting would be to set $a_k = 0$ in models (7) and (8) and then estimate b_k by dividing $\bar{\hat{\delta}}_{i,k}$

[33] $T_{tr,i,k}$ in the description just after equation (9) is the number of treatment years in sector k of country i.

by $\bar{p}_{i,k}$. However, we cannot rule out nonzero introduction effects, and thus we do not impose the zero-intercept restriction in equation (9). Given the variation in the treatment length (the number of years carbon prices have been in force), countries with shorter treatment periods might exhibit higher variance in their treatment effects. To account for this potential heteroskedasticity, we estimate (9) using an estimator weighted by treatment length:

$$\bar{\hat{\delta}}^*{}_{i,k} = a_k x^*_{0,i,k} + b_k \bar{p}^*{}_{i,k} \,, \tag{10}$$

where the weighted variables are given by

$$\bar{\hat{\delta}}^*{}_{i,k} = \sqrt{l_{i,k}}\bar{\hat{\delta}}_{i,k},$$

$$x^*_{0,i,k} = \sqrt{l_{i,k}},$$

$$\bar{p}^*{}_{i,k} = \sqrt{l_{i,k}}\bar{p}^*{}_{i,k},$$

with $l_{i,k}$ denoting the treatment length for sector k in treated unit i. To alleviate concerns about single outlying countries distorting the estimates, we estimate (10) using an outlier-robust MM estimator (Koller and Stahel 2011).[34] To conduct inference on b_k, we bootstrap (10) by sampling n_{treat} observations (where n_{treat} refers to the number of treated countries in the sample) from the bootstrap samples obtained using the generalized synthetic control estimator from Section 5.1. For example, in a sample of 22 treated countries ($n_{treat} = 22$), we sample one treatment effect for each country 1,000 times from the original bootstrap draws and estimate this robust weighted regression with 22 observations 1,000 times to approximate the distribution of b_k. Our models here, (9) and

[34] Implemented using the R package lmrobust.

(10), implicitly assume that the introduction effect $a_{i,k}$ is identical for all countries. We next relax this assumption when considering the within-country estimator of the implementation elasticity.

5.2.1.2 Elasticity Estimates Using Within-Country Variation

Using between-country variation to estimate the semielasticity of CO_2 emissions with respect to the carbon price does not control for country-specific characteristics that might lead to heterogeneous introduction effects. This model assumes that the pure introduction effect captured by $a_{i,k}$ is the same for all countries *i.* We therefore also estimate the effect of the carbon price on the estimated treatment effect using within-country variation of the carbon price level, allowing us to control for country fixed effects of the introduction of carbon pricing. We estimate a fixed effects panel model of the country-year specific treatment effects for each sector given in (10):

$$\hat{\delta}_{i,k,t} = a_{i,k} + b_k p_{i,k,t} \tag{11}$$

where $a_{i,k}$ are country fixed effects capturing the (potentially heterogenous) country-specific introduction effects of carbon pricing. We further estimate (11) including the first lag of the carbon price to test whether any price effect works through first differences rather than levels. We formally test heterogeneity of the introduction effects and price effects using tests of poolability of the fixed effects ($a_{i,k} = a_k \; \forall \; i$) and coefficients ($b_{i,k} = b_k \; \forall \; i$). We conduct inference on b_k in (11) by estimating the panel model 1,000 times using each bootstrap draw of the treatment effect $\hat{\delta}_{i,k,t}$ obtained from the generalized synthetic control estimator in Section 5.1.

5.2.2 Results: The Effect of the Price Level

(Semielasticity with Respect to the Carbon Price)

The point estimate of the emission semielasticity with respect to the carbon price is negative for most sectors but imprecisely estimated. Table IV shows the between-country and within-country estimates of the implementation semielasticity, with Figure V plotting the country-level average treatment effects against average carbon price levels used to derive the between-country estimates of the implementation semielasticity. The results suggest a 0.07 percent reduction in the growth rate of total CO_2 emissions for a \$1/tCO_2 increase in the average carbon price. However, the 95 percent bootstrap confidence interval includes zero, from –0.4 to +0.2 percent per dollar. Model results assessing level versus growth rate effects using lagged prices in the within-country model (and the TWFE and IFE estimates) are reported in Appendix D, primarily supporting an effect of the level of, rather than change in, the price. The results are robust to the choice of estimation method; the main results using the generalized synthetic control model are nearly identical to those obtained using the TWFE and IFE models.[35]

The null hypotheses that the carbon price coefficients and fixed effects are homogeneous over countries and therefore poolable are both rejected only in the case of the model of manufacturing CO_2 emissions.[36] Note that the model of manufacturing CO_2 emissions is the only one with really large estimates for the semielasticity (particularly for the within-country estimate) in Table IV,

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[35] The number of factors in the IFE model is chosen to match the number of factors determined using cross-validation in the estimation of the associated synthetic control factor model from Section 5.1.

[36] In Table IV, the null hypothesis that fixed effects are poolable is also rejected for the model of total aggregate emissions, whereas we cannot reject the null hypothesis that carbon price coefficients are poolable in this model.

suggesting that a small number of countries may be driving the results. We explore this possibility further in robustness checks in Section 5.3 by estimating the model of manufacturing emissions in EC form; Appendix E presents the results.

Our results showing substantial treatment effects but small and uncertain elasticity estimates suggest that the introduction effect accounts for much of the change in CO_2 emissions growth in response to the introduction of carbon pricing. This holds true for both the between- and within-country estimators when using synthetic controls (Table IV and Figure V) and TWFE and IFE estimates (Appendix D and Figure V). As Figure V (panel b) shows, the treatment effects do not vary much with the level of the carbon price (this also holds when including countries with much higher carbon prices, such as Sweden and Norway—see specification #6 plotted in Figure D1 in Appendix D). Merely introducing carbon pricing appears to result in emissions reductions and, at current observed price levels, additional reductions in emissions in response to higher price levels are marginal. The corollary is that *not* controlling for introduction effects (i.e., omitting $a_{i,k}$ and modeling CO_2 growth solely as a function of carbon prices) likely biases estimates of the emissions elasticities.

This is apparent in Figure V (panel b), where allowing for introduction effects shows no resulting change in the estimated treatment effect across price levels. However, *not* allowing for introduction effects is akin to forcing the intercept to be zero (in the relationship between treatment effects and price levels). The dashed line in panel b of Figure V shows the relationship between treatment effects and price levels when the intercept is omitted: the slope (the semielasticity) is notably steeper compared to that when allowing for introduction effects. This may explain why we find smaller elasticity estimates than earlier studies (e.g., Best et al. 2020), including those using fuel tax rates as proxies for carbon pricing (e.g., Davis and Kilian 2011).

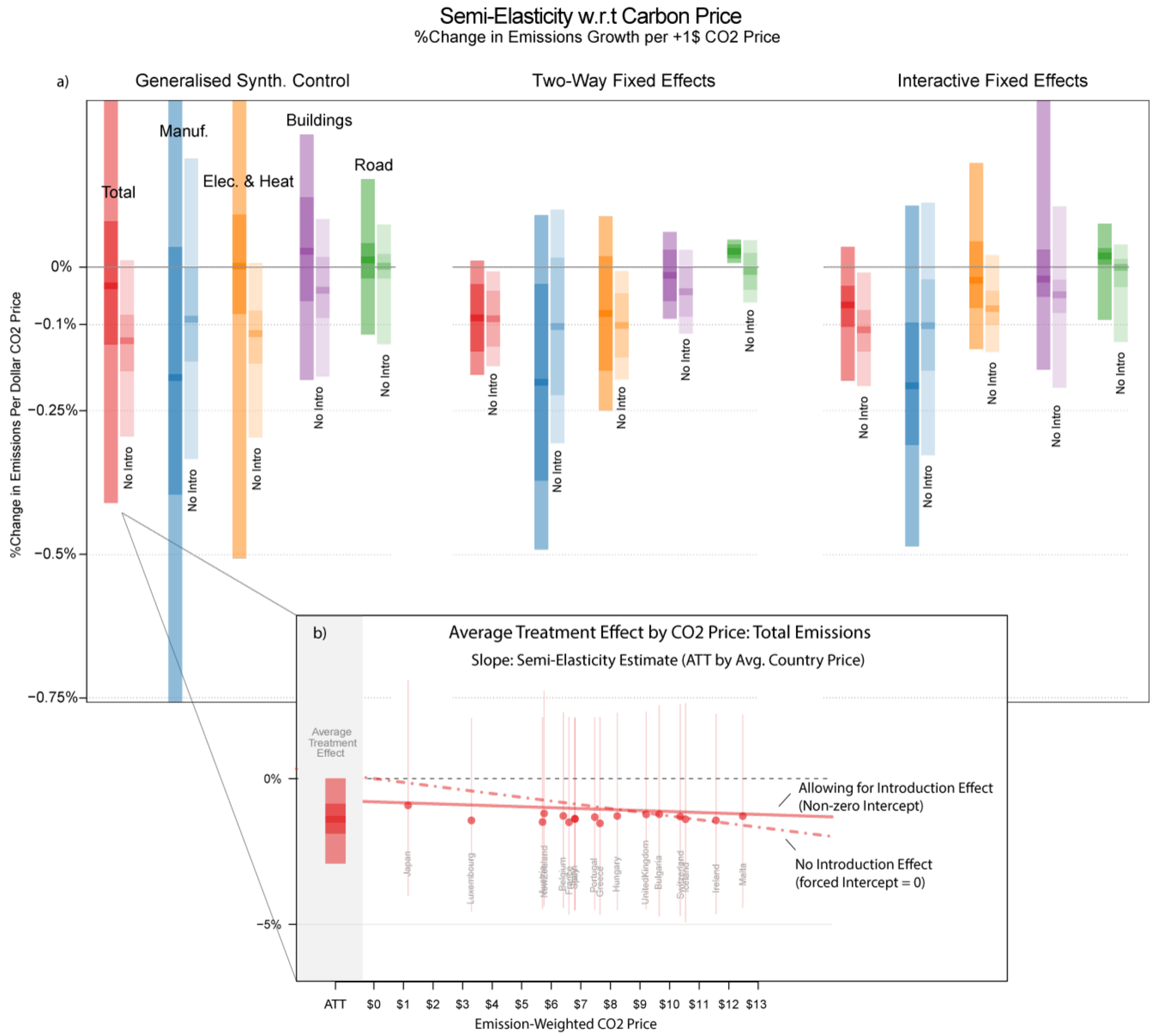

Figure V.

Semielasticity with Respect to Carbon Price Levels by Sector

Note: Panel (a): generalized synthetic control (left), two-way fixed effects (middle) and IFE (right) models across sectors. Estimates for the buildings sector given for specification #2. Light shading reports elasticities when omitting introduction effects (denoted as "no intro"). Panel (b) shows how omission of introduction effects biases the estimates of emission elasticities with regard to carbon pricing.

Table IV. Semielasticity with Respect to the Carbon Price

[Dependent Variable: $\Delta\log(CO2)_{i,k,t}$]

	Total	Electricity and heat	Manufacturing	Buildings	Road transport
Semielasticity (between-country)	–0.033 percent (–0.41 percent, 0.303 percent)	0.002 percent (–0.506 percent, 0.3 percent)	–0.192 percent (–0.773 percent, 0.432 percent)	0.012 percent (–0.117 percent, 0.152 percent)	0.028 percent (–0.195 percent, 0.23 percent)
Semielasticity (within-country)	0.001 percent (–0.279 percent, 0.344 percent)	0.093 percent (–0.102 percent, 0.276 percent)	–0.258 percent (–0.672 percent, 0.11 percent)	0.001 percent (–0.178 percent, 0.089 percent)	–0.045 percent (–0.201 percent, 0.173 percent)
N_{Tr}	17	16	16	6	7
F test for poolability of carbon price coefficient	$p = 0.645$	$p = 0.943$	$p = 0.005$	$p = 0.905$	$p = 0.984$
F test for poolability of fixed effects	$p = 0.009$	$p = 0.71$	$p = 0.012$	$p = 0.518$	$p = 0.744$
Specification #	1	1	1	1	2

Note: The 95 percent bootstrap confidence interval is shown in parentheses.

5.3 Robustness of the Results

We consider a range of model specifications for the estimated treatment effects and semielasticities and separate the effects of emission trading schemes from those of carbon taxes.

5.3.1 Robustness to Model Specification

Figure VI shows estimates of the average treatment effect and semielasticity obtained using generalized synthetic controls (and matrix completion) for each of the five sectors across 15 model specifications summarized in Table V. We vary the minimum number of pretreatment and posttreatment observations, criteria for control variables used to select the units in the donor pool (average level of emissions and all observed control variables must be at least as large as the minimum, or 25th percentile, for treated units), and forced additive fixed effect specifications in the IFE model. We also vary the set of control variables across specifications: omitting GDP growth to alleviate potential concerns of GDP growth itself being affected by carbon pricing and including HDD and CDD to control for weather fluctuation. To assess whether results are sensitive to our chosen estimator, we include additional specifications based on the matrix completion estimator developed in Athey et al. (2018).[37]

[37] Athey et al. (2018) show that the generalized synthetic control estimator (based on the IFE model) and their proposed MC estimator belong to a general class of matrix completion methods based on matrix factorization. Whereas the synthetic control approach minimizes the sum of squared errors given a fixed number of latent factors, their MC estimator determines the rank of the missing counterfactual matrix using nuclear norm penalization. The MC approach employs cross-validation to select the penalty term, λ, for regularization, similar to the generalized synthetic control (with IFE) approach to selecting the rank of common factors (Xu 2017). Most importantly for this study, both estimators accommodate staggered adoption across multiple treated units.

Table V. Model Specifications for Robustness Analysis

Spec. ID #	Min. treated years	Min. pretreatment years	Donor pool quantiles	Start year	Fixed effects	Estimator	Observed control variables
1 (base)	5	15	0	1980	two-way	IFE	soc_econ
2	0	15	0	1980	two-way	IFE	soc_econ
3	5	20	0	1980	two-way	IFE	soc_econ
4	5	15	no min	1980	two-way	IFE	soc_econ
5	5	15	0.25	1980	two-way	IFE	soc_econ
6	5	15	0	1975	two-way	IFE	soc_econ
7	5	15	0	1980	unit	IFE	soc_econ
8	5	15	0	1980	none	IFE	soc_econ
9	2	15	0	1980	two-way	IFE	soc_econ
10	5	15	0	1980	two-way	IFE	soc_econ_weather
11	5	15	0	1980	two-way	IF	pop_only
12	5	15	0	1980	two-way	MC	soc_econ
13	5	15	no min	1980	two-way	MC	soc_econ
14	5	15	0.25	1980	two-way	MC	soc_econ
15	5	15	0	1975	two-way	MC	soc_econ

Note: Base specification shown in main results section corresponds to specification number = 1 (shaded in grey). “Donor pool quantiles” refers to restrictions on countries included in the control group; “0” indicates that their average levels of emissions, GDP, population, and all other covariates must be equal to or greater than the minimum levels in the treated units; "no min." indicates that no limits are imposed; and "0.25" indicates that their average levels for each variable must exceed the 25th percentile of each variable in the treated units. "Start year" refers to the sample start date, "socioeconomic" refers to inclusion of GDP, sector-level GDP, and population control variables; and "weather" refers to inclusion of population-weighted heating degree days and cooling degree days.

Estimates of the average treatment effects and elasticities are robust across specifications. With respect to aggregate (economywide) emissions, the average treatment effect is centered around a –1.5 percentage point change in the growth rate of emissions, whereas emissions semielasticity is around –0.03 percent per average emissions-weighted dollar of CO_2 pricing.

Several aspects of the robustness analysis presented in Figure VI are noteworthy. First, our estimates are robust excluding GDP as a control variable, which alleviates the concern—discussed in Section 5.1—that the carbon price might affect emissions vis-à-vis its potential impact on economic output. Second, including HDD and CDD yields a significant increase in the ATT and marginal semielasticity point estimates for the buildings sector, with a considerable narrowing of the 95 percent bootstrap confidence intervals. This finding is consistent with the well-established empirical literature demonstrating the substantial impact of weather variation on energy demand (Mistry 2019) and indicates that our preferred specifications for the buildings sector should be number 10. Third, our estimates are robust to the choice of counterfactual estimator: generalized synthetic control with IFE or matrix completion. Fourth, although elasticities are imprecisely estimated, the bootstrap confidence intervals show long negative tails in many specifications, making increases in emissions in response to carbon pricing unlikely.

As a further robustness check, we estimate panel EC models for each sector that includes any treated country $i_{TR} \in 1,2, \ldots N_{TR}$, with a sufficiently long treated period $t_{TR} \in t_1, \ldots \geq t_{23}$ with respect to carbon pricing in sector k. Appendix E provides a summary of these specifications and results. Estimating these models allows us to check for potential cointegrating relations and average long-run effects that may be muted by our main model specifications in first differences. The EC specification also allows us to further investigate the results from Section 5.2, where F tests indicated that the carbon price coefficient and fixed effects are not poolable for the model of manufacturing emissions and, moreover, that fixed effects may not be poolable for the model of total emissions. More specifically, as the relatively large implementation semielasticities estimated in the manufacturing sector may be driven by a small number of countries, we can use the EC specification to check if any of the countries with a relatively long treatment period (Finland, Sweden, and Poland) in the manufacturing sector are driving this result. This intuition is confirmed in Appendix E: Finland accounts for the large semielasticity of manufacturing emissions. We reject the null hypothesis of "no cointegration" for the models of total and manufacturing emissions but cannot reject it for other sectors. As shown in Appendix E, the average long-run effects of an additional \$1/tCO_2 are a 0.2–0.6 percent reduction in the growth rate of total CO_2 emissions and manufacturing emissions, respectively.

5.3.2 Comparing Emissions Trading and Carbon Tax Schemes

The main estimates we report refer to the introduction of any carbon price, whether a carbon tax, ETS, or some hybrid scheme. We repeat our analysis for carbon taxes and ETSs in isolation, where we restrict the set of countries in the control group to those without any pricing scheme at all (to ensure clean control groups). Figures VII and VIII show the results when considering the carbon-tax-only and ETS-only treatment effects and elasticities in turn, with full estimation results in Appendix F.

With respect to aggregate (economywide) emissions, the average ETS-only treatment effect is centered around a -1.5 percentage point change in the growth rate of emissions (roughly equivalent to the estimated ATT of carbon pricing regardless of the policy type), and the ETS-only emissions semielasticity is close to zero (-0.01 percent per average dollar of carbon pricing). The average carbon-tax-only treatment effect is similarly centered around a -1.5 percentage point change in emissions growth, whereas the associated emissions elasticity cannot be estimated based on insufficient in-sample observations. Overall, the distinction between ETS and carbon tax effects seems so miniscule as to be substantively irrelevant.

Substantively important distinctions between ETS and carbon tax impacts are discernable only at the sector level. Specifically, we find that the majority of *ETS*-induced emissions abatement occurred in the electricity and heat and manufacturing sectors, where emissions trading elicited significantly greater negative effects on emissions growth relative to the (relatively few) carbon tax schemes that have been applied in these sectors. By contrast, the majority of *tax*-induced emissions abatement occurred in the road and buildings sectors, with significantly greater emissions reductions than were generated via emissions trading, a less commonly used form of carbon pricing in these sectors. This is likely due in large part to the relatively high administrative costs and additional monitoring requirements of implementing ETSs for road and buildings sectors at the national level, as compared to carbon taxes, which can be applied relatively seamlessly to the carbon content of purchased fuels. We conclude that the ostensible differences in the sector-specific emissions response to these two main forms of carbon pricing are probably mere artifacts of the relative ease with which each can be applied to the relevant sectors, rather than any intrinsic difference in environmental efficacy per se.

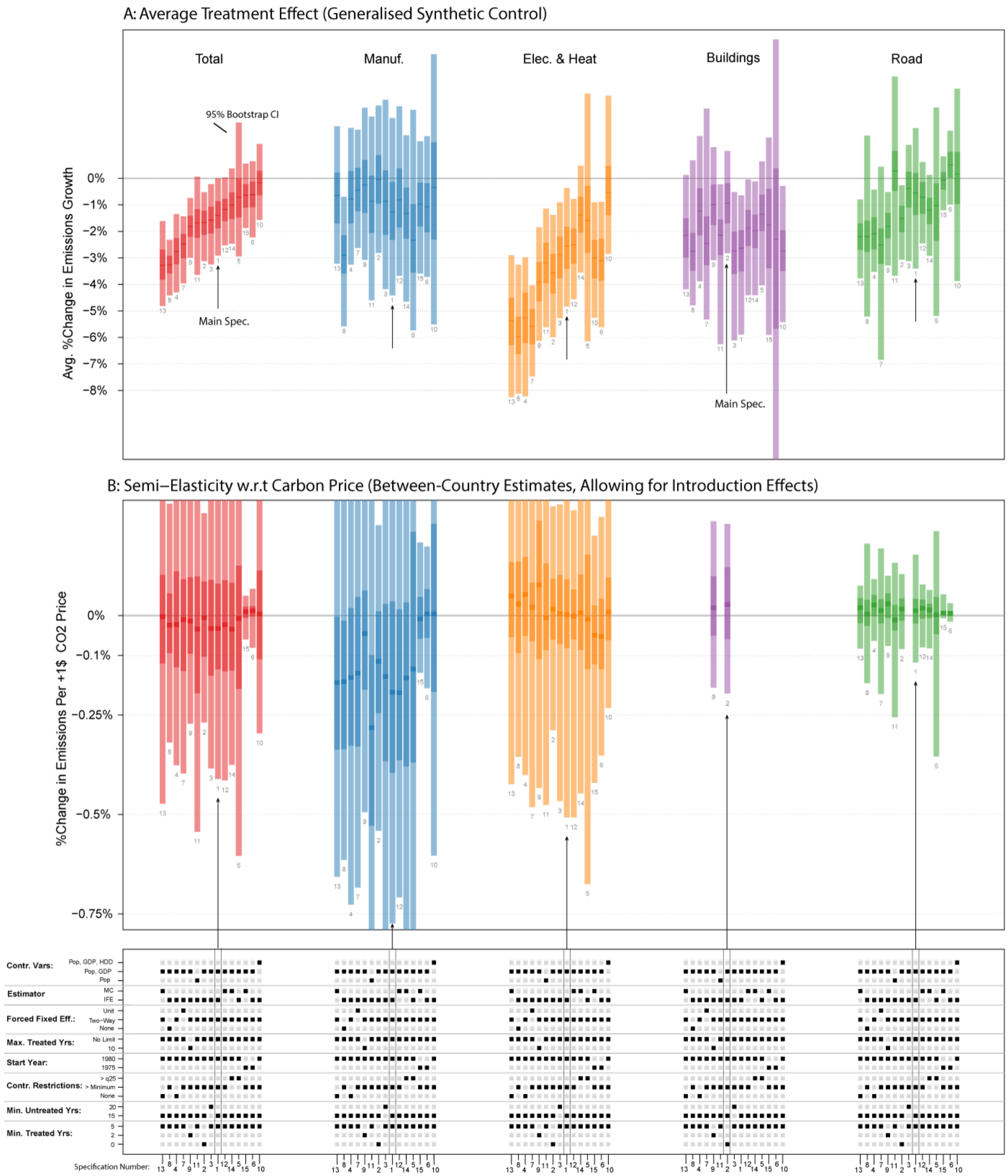

Figure VI.

Average Treatment Effects and Semielasticities (Using Synthetic Controls and Between-Country Variation) Across 15 Model Specifications

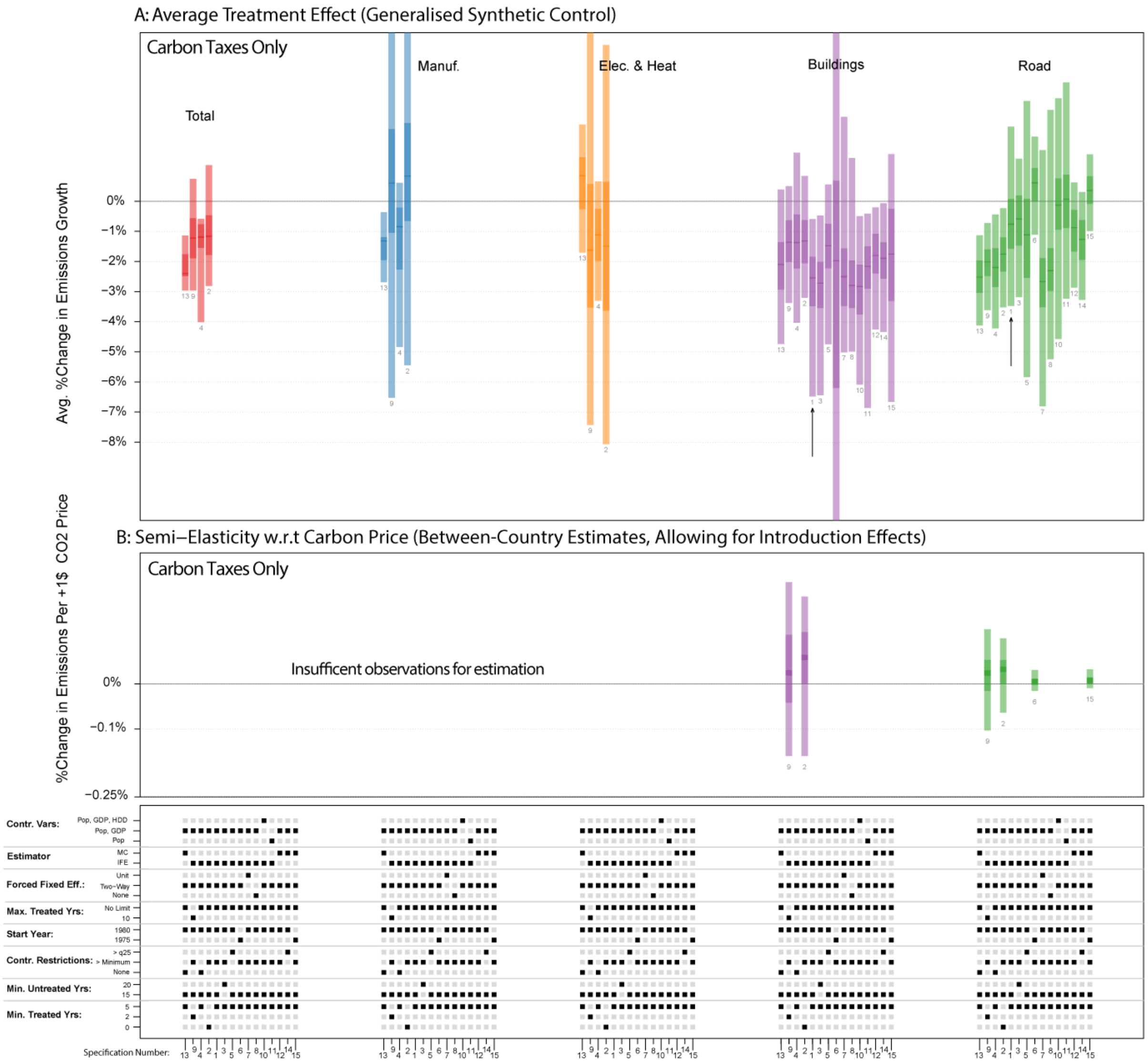

Figure VII.

Carbon Tax–Only Sample: Average Treatment Effects and Semielasticities (Using Synthetic Controls and Between-Country Variation) Across 15 Model Specifications

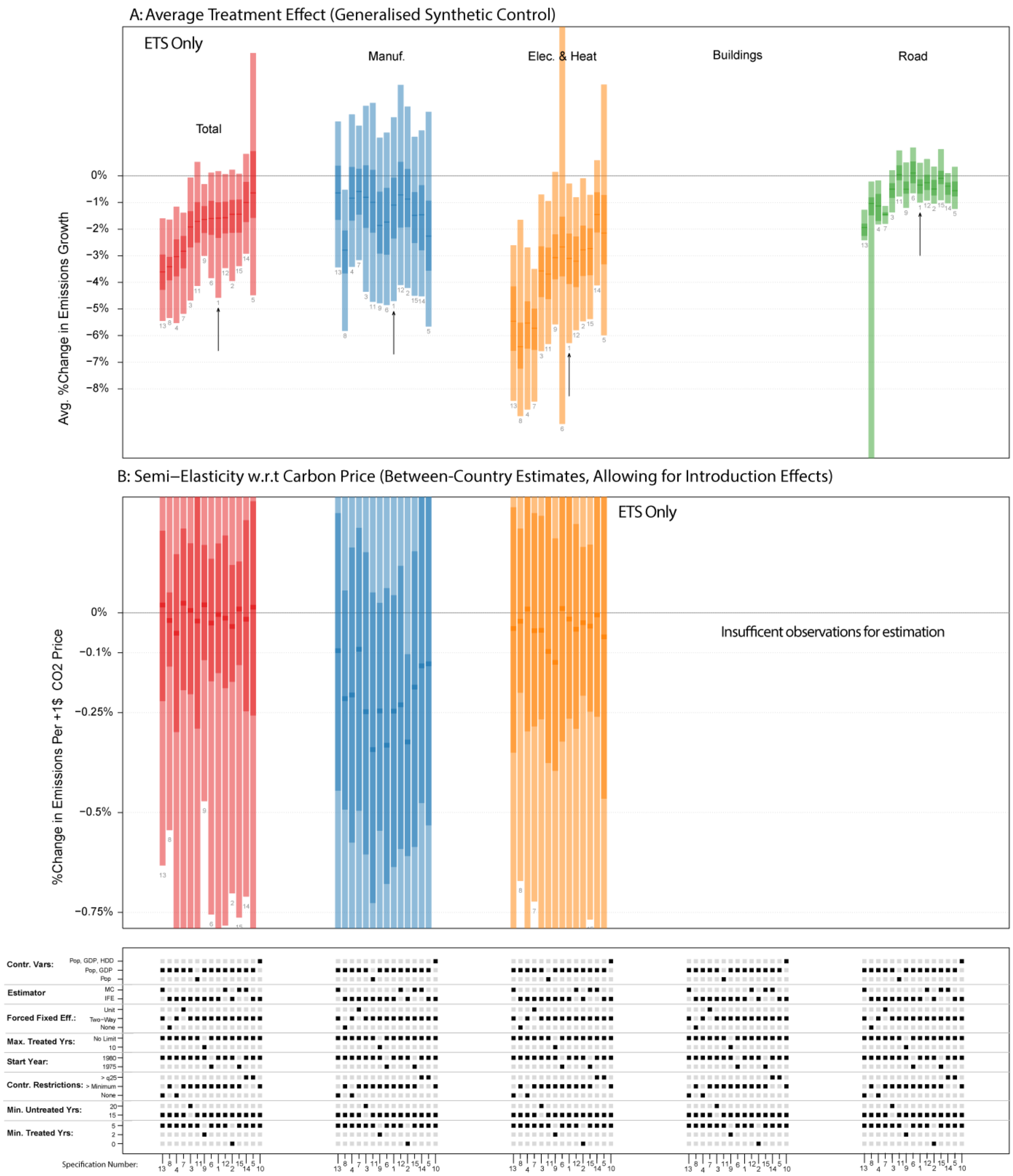

Figure VIII.

ETS-Only Sample: Average Treatment Effects and Semielasticities (Using Synthetic Controls and Between-Country Variation) Across 15 Model Specifications

6. Simulating the Emissions Response to Future Price Paths

Policymakers have long sought the answer to the question of what changes in emissions can be expected in response to a specific carbon pricing scheme; this is particularly pressing due to the international commitments under the Paris Agreement. More recently, several governments have issued statements (Japan), submitted legislative proposals (Canada, EU) or enacted laws (UK, New Zealand) committing to net-zero emissions by midcentury (Climate Action Tracker 2021).

Many economists have hailed carbon prices as the tool of choice to implement such emission reductions at the "scale and speed that is necessary" (Economists' Statement on Carbon Dividends 2019). However, these claims were made with little empirical evidence to support them. Using our estimates of the implementation and marginal semielasticities, we simulate the impact of carbon pricing on projected emissions to assess whether it is likely to be sufficient to achieve reductions at the required scale and speed. We compare emissions under carbon pricing to no-pricing scenarios using projected CO_2 emissions from the Shared Socioeconomic Pathways (SSPs), a set of reference scenarios from 2005 to 2050 (Riahi et al. 2017).[38] We consider a hypothetical global carbon price introduced in 2021. We simulate projected total (*tot*) emissions as

$$\log(\widehat{CO2})_{tot,t} = \log(\widehat{CO2})_{tot,t-1} + \Delta\log(\widehat{CO2})_{tot,t}, \tag{12}$$

$$\text{for } t = 2006, \ldots, 2100$$

[38] SSP emissions pathways are available from the SSP database hosted at the IIASA website: (https://tntcat.iiasa.ac.at/SspDb/dsd?Action=htmlpage&page=about) and provided in 10-year time-steps. We interpolate the SSP projected emissions linearly to an annual frequency to match our estimates of the implementation and marginal semielasticities.

with the initial value $\log(\widehat{CO2})_{tot,t=2005}$ provided by the 2005 level of emissions in the SSP scenario and the projected change in emissions given by

$$\Delta \log\left(\widehat{CO}2\right)_{tot,t} = \Delta \log(CO2)_{tot,t,Base} + \Delta \log(CO2)_{tot,t,Policy} \tag{13}$$

where $\Delta \log(CO2)_{tot,t,Base}$ is the CO_2 emissions growth rate given in the SSP reference scenario and $\Delta \log(CO2)_{tot,t,Policy}$ is the emissions growth in a hypothetical carbon pricing scenario. We consider two approaches to project the impact of pricing on emissions. In the first, we specify the policy impact on the growth rate as solely the average treatment effect on the treated:

$$\Delta \log(CO2)_{tot,t,Policy} = \bar{\hat{\delta}}_{tot} \tag{14}$$

where $\bar{\hat{\delta}}_{tot}$ is the average treatment effect on the treatment estimated using synthetic controls for emissions aggregated across sectors. Thus, the counterfactual simulation using (11, REF) projects the change in emissions combining the average introduction effects and the average in-sample price level (around \$3/tCO_2).

In the second set of scenario projections, we decompose the treatment effect into the estimated introduction and price effects:

$$\Delta \log(CO2)_{tot,t,Policy} = \hat{a}_{tot} + \hat{b}_{tot} p_t \tag{15}$$

where $\hat{b}_{tot}$ is the estimated semielasticity using between-country variation from Section 5.2, $p_{t,tot}$ denotes the carbon price at time t during the projected treated period (2021–2050) and $\hat{a}_{tot}$ is the intercept in our model used to estimate the elasticity. Taken together, $\hat{a}_{tot} + \hat{b}_{tot}p_t$ correspond to our model of the average treatment effect. The elasticity and introduction estimates are taken from the baseline model specification (1) summarized in Table 6. The model in (12) allows us to simulate the impact of any hypothetical price path p_t.

We simulate the uncertainty range around projected emissions by sampling over the bootstrap draws of the treatment effect $\bar{\hat{\delta}}_{tot}$, price coefficient, $\hat{b}_{tot}$, and introduction effect (intercept), $\hat{a}_{tot}$. We implicitly assume that these parameters remain constant over the projected period and, therefore, that no gradual phase-in of effects or nonlinearities take place. Granted, it is not guaranteed that the emissions elasticity will be constant or the demand function will be smooth and continuous into the future; as renewable energy resources become cheaper than fossil fuels in a growing number of sectors and markets, economies may reach an inflection point where the price elasticity of emissions shifts upward as demand for fossil fuels plummets. Parameter constancy is a strong simplifying assumption, but any variation in emissions that occurs due to time-dependency of policy effects likely falls well within the already wide range of simulated outcomes. Uncertainty about the phasing in of treatment effects is likely dwarfed by the uncertainty in the parameter estimates. The following simulations are perhaps optimistic in the short run, because they assume prices affect emissions immediately.

Figure 9 shows projected emissions for the SSP2 reference scenario (commonly referred to as the "middle-of-the-road" scenario) together with hypothetical carbon price paths.[39] The first scheme (purple) introduces a constant emission-weighted price of \$8/tCO_2 (the median across all current schemes). A second scheme simulates an initial \$20/tCO_2 price that increases by \$5/tCO_2 per year until reaching \$100/tCO2 (green). A third simulates a constant \$250/tCO_2 price (red).

Even though the semielasticity is imprecisely estimated, the median projected difference in emissions suggests a 35 percent reduction in the level of CO_2 emissions by 2050 for the \$8 constant pricing scheme. It is critical to note that this is relative to the reference scenario, and even a 35 percent reduction in the emissions level relative to the SSP2 baseline corresponds to only a little over 10 percent emissions reduction relative to 2020 (bottom panel in Figure 6). The wide uncertainty range of projected emissions implied by the bootstrap intervals shows we cannot be certain of carbon pricing guaranteeing large-scale emission reductions (the 25–75 percent interquartile bootstrap ranges are shown as shaded for the constant \$8 and \$250 pricing schemes).

Our conclusion is that achieving a median projected emissions reduction of 50 percent by 2030 relative to 2020 using only carbon pricing seems all but impossible. Projected median emission changes in response to a \$20/tCO_2 carbon price that is ramped up by \$5 per year until reaching to \$100 result in a 15 percent reduction by 2030 and 40 percent by 2050. Without persistence in emissions, achieving the desired reduction would likely require a global emission-weighted economywide carbon price in excess of \$250/tCO_2 (red pricing scheme in Figure IX, with very high uncertainty). This seems far outside the realm of political feasibility.

[39] Fricko et al. (2017) offers a full description of the SSP2 scenario and its underlying assumptions.

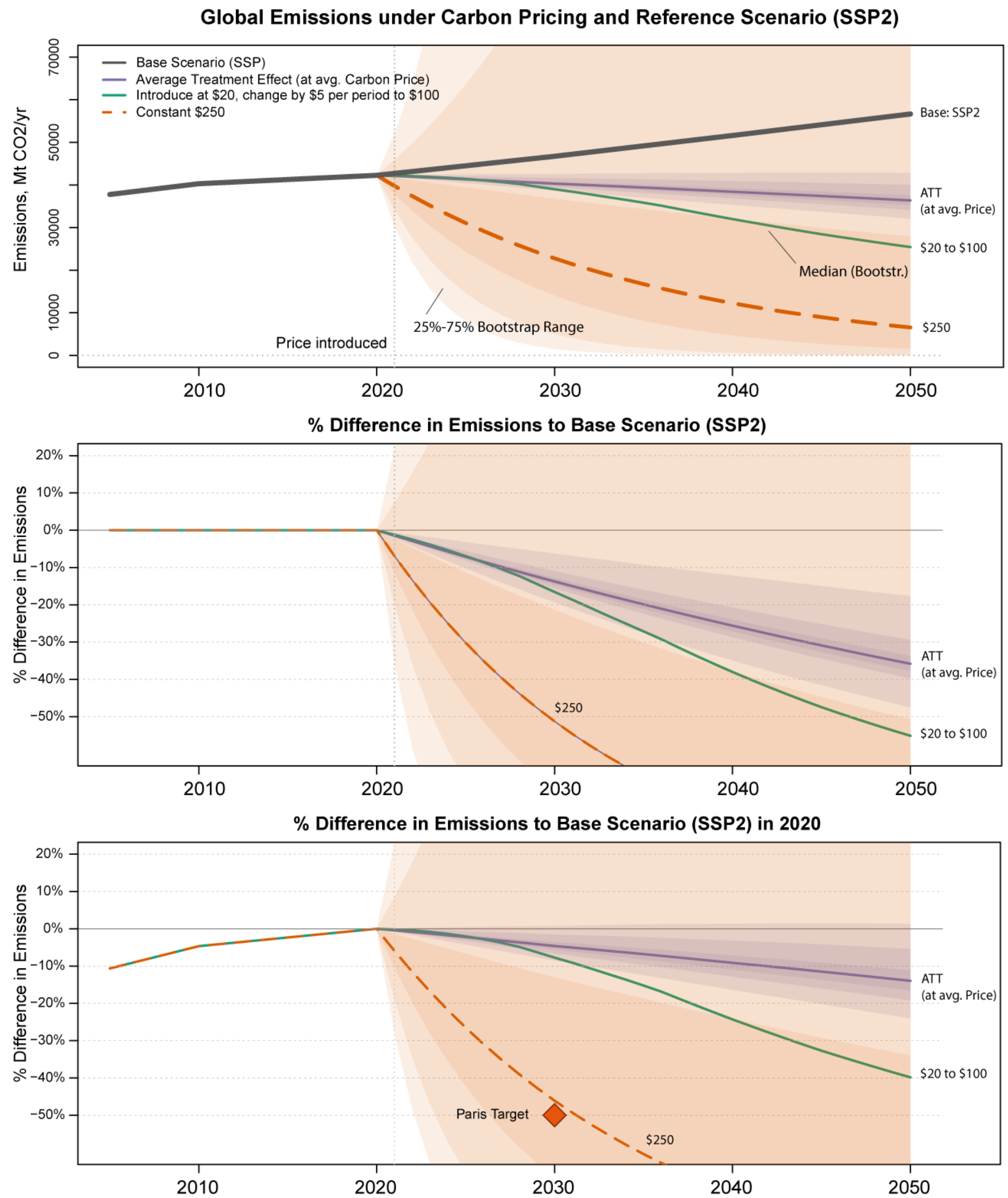

Figure IX.

Global CO_2 Emissions Relative to Reference Scenario (SSP2, "Middle of the Road") Using Empirical Estimates of the Emissions Response to CO_2 Pricing

Note: Top panel shows the projected emissions, with the reference scenario in black and median hypothetical emissions for different pricing schemes: constant $8 (purple); initial $20 and increasing until reaching $100 (green); and constant $250 (red). The middle panel shows the percentage difference to baseline in each year, and the bottom panel shows the percentage difference from the reference scenario in 2020. Shaded bands denote a 25–75 percent bootstrap interquartile range for the purple and red schemes. The Paris Agreement target of a 50 percent reduction relative to 2020 by 2030 is indicated by the red diamond.

7. Conclusion: Policy Implications

Few questions are as pressing today in the arena of climate policy as the effectiveness of carbon pricing at reducing emissions, given the preponderant preference for (or at least promotion of) market-based approaches at numerous government ministries, NGOs, carbon-intensive corporations, OECD, IMF, World Bank, and UNFCCC. Our retrospective evaluation contributes to a fuller understanding of this question, based on a novel approach to estimating changes in CO_2 emissions associated with (i) the introduction of carbon pricing irrespective of the price level, (ii) the effect of carbon pricing conditional on the price level, and (iii) the response of future emissions to possible carbon price paths based on our empirical estimates of average treatment effects and emissions elasticities.

Consistent across a range of model specifications, carbon pricing instruments have reduced the annual growth rate of CO_2 emissions by 1–2.5 percentage points on average relative to counterfactual emissions, with most abatement occurring in the electricity and heat sector (where estimates of the average treatment effect reach up to –6 percentage points in some specifications). The response of emissions to a higher price level is imprecisely estimated in all sectors, with the potential exception of manufacturing. Negative point estimates for the semielasticity are centered around a 0.1 percent reduction in the growth rate of total emissions for each additional $\$1/tCO_2$

and roughly 0.2 percent in the manufacturing sector. This suggests that merely introducing carbon pricing (even at low levels) can reduce emissions growth. However, perhaps only marginal additional reductions can be achieved at higher price levels for the range of prices currently observed in sample. For example, in response to carbon pricing, New Zealand—with an average price of around \$6 dollars per metric ton of CO_2 in the electricity and heat sector—reduced emissions by around 3 percentage points, and Switzerland, with a much higher average carbon price of \$36, experienced a similar 3 percentage point reduction in emissions growth.

Based on our simulations of potential future CO_2 emissions reductions in response to alternative carbon price paths up to 2050, we conclude that emissions are unlikely to decline to levels consistent with Paris climate targets given plausible levels of carbon pricing in the decades ahead, absent complementary (nonpricing) policies and substantial public investments to deploy green technologies and infrastructure.

Our estimates of (semi)elasticities indicate that emissions may be substantially less responsive to the level of the carbon price than suggested by previous empirical studies, whereas perhaps the mere introduction of carbon pricing sends a signal that leads to reductions in emissions. The energy demand elasticities assumed in energy-climate models, for example, typically fall between –0.3 and –0.7—see discussions in Madlener et al. (2011), Webster et al. (2008), and Parry (2020). By contrast, our (implied) energy demand elasticity estimates center around –0.18 for electricity and heat, buildings, and the economy as a whole.[40] For the road transport sector, Sterner (2007) reports

[40] This is calculated based on our estimate of the average marginal semielasticity by computing the effect of a \$1/tCO_2 price increase relative to an average price of \$8/tCO_2 in sample. The same holds for the subsequent estimate reported in this paragraph for the price elasticity of gasoline demand.

globally averaged gasoline price elasticities of around –0.7 based on estimates from Europe and the United States, and the estimates in Dahl (2012) are closer to about –0.25 on average. Our (implied) gasoline price elasticity estimates center around –0.25. We add a caveat: our implied elasticity estimates here assume that the (carbon)-price elasticity of energy demand is equivalent to the generic price elasticity of energy demand. If instead one were to assume that the CO_2-price elasticity is around threefold greater than the generic price elasticity, as suggested in several recent studies,[41] then the disparity between our estimates and those of previous empirical studies would be even greater.

Several considerations lead us to conclude that our significantly lower elasticity estimates are not mere artifacts of statistical noise but rather indicative of poignant empirical realities.

1. The difference between our estimates and earlier results could partly stem from our explicit differentiation between introduction and price effects. If we do not allow for introduction effects, this may bias the elasticity estimates with respect to carbon prices.
2. Relying on empirical estimates of energy demand elasticities based on data from the 1980s and earlier may lead researchers and policymakers to underestimate the extent to which energy demand has been shifting toward relatively fast-growing and less price-responsive products and regions.[42]
3. Policy-response models of CO_2 emissions (both ex ante and ex post) have tended to poorly capture the inertia of infrastructure lock-in.[43]

[41] See, for example, Andersson (2019).
[42] See, for example, the evidence for world oil demand in Daragay and Gately (2010).
[43] See, for example, the analysis in Avner et al. (2014) of urban vs. rural responses to carbon pricing under varying densities of mass public transport infrastructure.

4. Our empirical evaluation is the first to explicitly account for cross-country and temporal variation in carbon price exemptions across different sectors and industries. The importance of this can be seen when considering that governments may be incentivized to “offload” higher carbon prices onto sectors and industries that are either (i) relatively price inelastic but able to bear the policy costs due to relatively less carbon exposure; or (ii) highly price elastic but have already undergone critical processes of decarbonization in the years preceding the introduction of carbon pricing.[44]

Taken together, these considerations should cast doubt on the notion that the price elasticity of energy demand should be stable over time, an implicit assumption of our simulation exercise. Instead, emissions elasticities are likely to be a function of not only the price of emissions but also the initial state in the evolutionary process of complex energy-technological systems to which the price is applied (Mercure et al. 2014; Grubb 2014). As a consequence, we emphasize that any conclusions drawn from our simulation exercise, although they are based on empirically grounded and up-to-date elasticity estimates, are limited by an irreducible element of uncertainty.

Our assessment corroborates several best practices for optimizing carbon pricing reforms that have been identified elsewhere. First, carbon prices are undermined the more they are volatile interannually; their environmental efficacy tends to be enhanced when they are on a credible upward trajectory, which has been rare but can be reinforced through built-in price-adjustment mechanisms (Hafstead et al. 2017; Metcalf 2020). Alternatively, policymakers may attempt to price CO_2 emissions at very high levels initially to better capture climate externalities under conditions of uncertainty, which may counterintuitively imply a declining CO_2 price path over time (Daniel

[44] For example, Denmark and Germany underwent multidecade processes of energy system transformation in response to the oil price shocks of the 1970s, as discussed in Grubb et al. (2017) and elsewhere.

et al. 2019). Such an experiment would be intriguing, but it seems unlikely to pass muster without substantial revenue recycling in the early phase to counteract any regressive impacts on individuals whose carbon cost exposure comprises a salient share of their household income (Klenert et al. 2018).

Second, despite compelling arguments that might lead policymakers to prefer carbon pricing schemes that strategically target a small number of industries or sectors with significant intersectoral linkages (King et al. 2019), those opting for such an approach should recognize that the discrepancy between current coverage levels and those that are likely needed to comply with 1.5–2°C climate targets remains stark. Thus, additional regulations that implicitly price CO_2 emissions or public green investments that reduce the costs of alternatives will be needed to incentivize decarbonization wherever an explicit and sufficiently high CO_2 price is absent. Under a targeted carbon pricing scheme, exemptions for emissions-intensive industries should still be eliminated to the greatest extent possible, including in the implicit form of unpriced carbon embodied in internationally traded goods (Moran et al. 2018); nor should greater reliance on nonpricing climate measures distract policymakers from the need to eliminate fossil fuel subsidies that function as a negative carbon price, about three-quarters of which globally are due to domestic factors that are alterable via energy pricing reforms (Coady et al. 2019).

Climate policies, when strategically targeted and combined, may be highly synergistic (Farmer et al. 2019; Grubb 2014; Mercure et al. 2014). Carbon pricing still has the potential to be a powerful tool contributing to emission reductions, but it is no panacea.

Appendix A.

Computing ECPs

To compute the emissions-weighted carbon price (ECP), the following information is required: (i) the coverage of the carbon pricing policy (volume of CO_2 emissions to which the price applies), (ii) verified total CO_2 emissions in each jurisdiction, and (iii) the nominal emissions price (/tCO_2). This information is collected at the sector-fuel level. Sectoral disaggregation follows the guidelines of the International Panel on Climate Change (IPCC 2006). The main anthropogenic sources of national (territorial) CO_2 emissions are included based on three IPCC source categories: "Fuel Combustion Activities—Sectoral Approach" (category 1A); "Fugitive Emissions from Fuels, Gas Flaring, and Venting" (category 1B); and "Industrial Processes and Product Use, Including Cement" (category 2). These categories accounted for 92 and 72 percent of total global CO_2 and GHG emissions, respectively, in 2012 (IEA 2018; UNFCCC 2018).

Information pertaining to the fuels, sectors, and quantity of emissions to which each carbon pricing policy instrument applies within each country is from various sources, including primary legislation, the OECD Database on Instruments Used for Environmental Policy (OECD 2020), customs agencies' documentation, academic journal articles, and policy assessment reports (for a full list, see https://github.com/g-dolphin/WorldCarbonPricingDatabase).

Verified data on total CO_2 emissions in each jurisdiction is derived from IEA (2018). Information about nominal emission prices (tax rate or allowance price) is from different sources depending on the type of policy instrument and particular jurisdiction. For carbon taxes, we rely on the IEA's annual Energy Prices and Taxes publication, jurisdictions' budget proposals, and primary and secondary legislative acts (for exhaustive information on data sources for CO_2 prices in ETSs and carbon tax schemes, see https://github.com/g-dolphin/WorldCarbonPricingDatabase).

With this information, the emissions-weighted carbon price (ECP) can be computed at the sector and economywide levels. Formally, the ECP of sector j of country i in year t can be expressed as

$$ECP_{i,t,j} = \frac{\sum_k \left[\tau_{i,t,j,k}\left(q_{i,t,j,k}^{tax} + q_{i,t,j,k}^{ets,tax}\right) + p_{i,t,j,k}\left(q_{i,t,j,k}^{ets} + q_{i,t,j,k}^{ets,tax}\right)\right]}{q_{i,t,j}^{CO2}} \tag{A1}$$

where

$\tau_{i,t,j,k}$ is the carbon tax rate applicable to fuel k,

$q_{i,t,j,k}^{tax}$ is the quantity of CO_2 emissions covered by a tax only,

$p_{i,t,j,k}$ is the price of an emission permit,

$q_{i,t,j,k}^{ets}$ is the quantity of CO_2 emissions covered by an emissions trading system (ETS),

$q_{i,t,j,k}^{ets,tax}$ is the quantity of CO_2 emissions covered by both an ETS and a carbon tax, and

$q_{i,t,j}^{CO2}$ is the total quantity of CO_2 emissions in sector j of country i in year t.

Should a sector be covered by only one of the two policy instruments and all CO_2 emissions (i.e., all of its fuels are covered), the $ECP_{i,t,j}$ would collapse to either $\tau_{i,t,j}$ or $p_{i,t,j}$.

An economywide ECP is then computed as a weighted average of the sectoral carbon rates. The weights correspond to the quantities of emissions subject to each individual carbon rate, such that

$$ECP_{i,t} = \sum_{j} ECP_{i,t,j}\gamma_{i,t,j}, \tag{A2}$$

where $\gamma_{i,t,j}$ represents the CO_2 emissions of sector *j* as a share of total CO_2 emissions in each jurisdiction (i.e., $q_{i,t,j}^{CO2}/q_{i,t}^{CO2}$). All prices are expressed in US dollars at constant 2019 prices.

To ensure that the computed ECP levels are not biased by interannual changes in CO_2 emissions that may be a consequence of the policy itself, all years are weighted using emissions data of the year before the policy was introduced. For country-level ECP, this means that the weights are based on emissions in the year before the carbon pricing policy in *any* sector of the economy. For sector-level ECP, weights are based on emissions in the preceding year for that sector.

When considering prices arising from ETS and tax schemes separately, weights are based on emissions in the year preceding the introduction of *any* pricing scheme.

APPENDIX B.

DATA SUMMARY

Table B1.

Summary of Observed Covariates

Covariates	Unit	Source
CO_2 emissions: total (economywide), electricity and heat, manufacturing, road transport, and buildings (commercial and residential)	Million tons of CO_2 ($MtCO_2$)	IEA (2018)
Emissions-weighted carbon price: total (economywide), electricity and heat, manufacturing, road transport, and buildings (commercial and residential)	US dollars per ton CO_2 (constant 2015 prices)	Updated from Dolphin et al. (2020)
GDP: total, manufacturing, transport, and services	US dollars (millions, constant 2015 prices)	UNCTAD (2020a), based on United Nations DESA Statistics Division, National Accounts Main Aggregates Database
Population size	Absolute value in thousands	UNCTAD (2020b), based on United Nations DESA Population Division, World Population Prospects: The 2019 Revision

Degree days: heating, cooling	Population-weighted (18.3°C base temperature)	Mistry (2019)

Appendix C.

Diagnostics and Misspecification Tests

Our model specifications are informed by diagnostic tests for cross-section dependence, common factors, unit roots, and panel cointegration.

First, we strongly reject the null hypothesis of cross-section independence (and weak cross-section dependence) of the errors for our baseline model when variables are in levels, but we cannot reject the null when the model is specified in first differences. Hence, differencing not only eliminates serial correlation of the errors but also allays concerns about cross-section dependence. Using the unit root tests developed in Im et al. (2003) and Pesaran (2007), we cannot reject the null hypothesis that the covariates contain unit roots for all panels, but we reject the null when variables are in first differences. Thus, all variables are integrated of order $I(1)$.

The null hypothesis that additive (time and unit) fixed effects are sufficient is strongly rejected at the 1 percent level using the Hausman-type test in Bai (2009). The null hypothesis that the dimensionality of common factors equals zero is strongly rejected at the 1 percent level, regardless of whether the factors are assumed to be $I(0)$ or $I(1)$ (Bai 2009; Kneip et al. 2012). We determine the optimal number of factors to be 2–5 depending on the sector and model specification, based on the dimensionality test criteria proposed in Ahn and Horenstein (2013), Kneip et al. (2012), and Bai and Ng (2002).[45]

[45] All tests are computed using phtt in R.

To distinguish between common and idiosyncratic components of the residuals (Bai and Ng 2004, 2010), we apply the PANNICA testing procedure described in Reese and Westerlund (2016); Table C.1 presents the results. The procedure combines the strong small sample performance of the tests developed in Pesaran (2006) with the flexibility regarding orders of integration for common and idiosyncratic error components as in the tests from Bai and Ng (2004, 2010). The results corroborate the presence of multiple common factors. When variables are entered in levels, we fail to reject the null hypothesis of fewer unit roots than common factors, suggesting global stochastic trends. But when variables are in first differences, we do not detect unit roots in the remaining factors. Furthermore, we reject the null hypothesis of a unit root in the idiosyncratic errors of all countries using the Bai and Ng (2010) tests. Hence, all tests consistently suggest that nonstationarity is driven entirely by common error components, whereas stationarity is attained in the first-differenced model conditional on the observed regressors.

This naturally leads to tests for cointegration. We apply those proposed by Westerlund (2007) to the baseline specification for the model of total aggregate CO_2 emissions. Table C.2 presents the results. Bootstrap critical values of these tests are robust in the presence of common factors. We strongly reject the null hypothesis of no cointegration at the 1 percent level.

Table C1.

Panel Analysis of Nonstationarity in Idiosyncratic and Common Components (PANIC)

		Common factors		Unit-specific residuals		
	k	MQ_c	MQ_f	P_a	P_b	$PMSB$
$\log(CO_{2\_total})$	3	–24.089		–12.486	–5.166	–2.12
	2		–10.935	(0.000)	(0.000)	(0.017)
$\log(CO_{2\_industry})$	4	–39.428	–38.999	–21.91	–8.806	–3.468
				(0.000)	(0.000)	(0.0003)
$\log(CO_{2\_electricity})$	6	–46	–42	–31.885	–11.271	–3.99
				(0.000)	(0.000)	(0.000)
$\log(CO_{2\_road})$	6	–46	–42	–23.952	–8.203	–2.824
				(0.000)	(0.000)	(0.0024)

Notes: We apply the iterative estimation procedure of Bai and Ng (2004) to obtain MQ_c and MQ_f, which are modified versions of the "corrected" Q_c and "filtered" Q_f tests in Stock and Watson (1988), where k denotes the number of independent stochastic trends driving the common factors. The null hypothesis of both tests is k unit roots in the common factors; we report only the test statistics for iterations where it cannot be rejected. For the idiosyncratic (unit-specific) component, we compute the three test statistics from Bai and Ng (2010): *PMSB* is a panel-modified Sargan–Bhargava test that does not require estimation of p, the pooled autoregressive coefficient of the unit-specific errors. The null hypothesis of all three unit-specific tests is that all units are nonstationary, which we strongly reject. All test statistics are computed using xtpanicca in Stata, with thanks to Simon Reese for helpful input.

Table C2.

Tests for Panel Cointegration

[Dependent variable: $\boldsymbol{\Delta\log(CO2)_{i,k,t}}$]

G_τ	G_α	P_τ	P_α
–6.127	6.067	2.458	2.384
(0.000)	(1.000)	(0.993)	(0.991)

Note: Bootstrap *p*-values based on 1,000 replications are shown in parentheses. Critical values of the test statistics are robust in the presence of common factors. The optimal lag and lead length for each series is selected using the Akaike information criterion. The long-run variance is based on semiparametric estimation using the Bartlett kernel.

Appendix D.

Additional Estimation Results

D.1 Two-Way Fixed Effects and Interactive Fixed Effects Results

D.1.1 Treatment Effect Estimates

We report the full set of results when estimating treatment effects using TWFE and IFE models. Table D1.1.1 shows the estimation results of the TWFE model in (1); Table D1.1.2 shows the estimation results of the IFE model in (2). The number of factors in the IFE model is chosen to match the number of factors determined using cross-validation in the synthetic control factor model. TWFE standard errors are clustered at the country level. IFE standard errors are derived using 500 bootstrap draws. The estimation results are comparable to those from generalized synthetic control methods, though the latter generally exhibit lower estimation uncertainty.

To differentiate between growth and level effects we expand models (1) and (2) to further include a lagged treatment indicator in the TWFE model:

$$\Delta \log(CO2)_{i,k,t} = \delta_{0,k} D_{i,k,t} + \delta_{1,k} D_{i,k,t-1} + x'_{i,k,t}\beta + \xi_{i,k} + \tau_{k,t} + \epsilon_{i,k,t} \tag{D1}$$

and the IFE model:

$$\Delta \log(CO2)_{i,k,t} = \delta_{0,k} D_{i,k,t} + \delta_{1,k} D_{i,k,t-1} + x'_{i,k,t}\beta + \xi_{i,k} \tag{D2}$$

$$+ \tau_{k,t} + \lambda'_{i,k} F_{k,t} + \epsilon_{i,k,t}$$

If the introduction of carbon pricing affects the level of CO_2 emissions rather than the growth rate, we expect the change in that rate to be transitory and the coefficient $\delta_{0,k}$ on the contemporaneous treatment variable to have the opposite sign from the coefficient $\delta_{1,k}$ on the lagged treatment variable. Tables D1 and D2 also report estimation results, showing little evidence of level rather than growth effects. The coefficients are predominantly not opposite signed.

Table D1. TWFE Results: Average Treatment Effects

	Total		Elec. & Heat		Manufacturing		Road transport		Buildings	
Introduction	–0.006 (0.007) [p = 0.37]	0.006 (0.011) [p = 0.58]	–0.019 (0.011) [p = 0.1]	–0.003 (0.033) [p = 0.93]	–0.002 (0.016) [p = 0.89]	–0.025 (0.031) [p = 0.43]	–0.011 (0.01) [p = 0.28]	–0.002 (0.019) [p = 0.92]	–0.014 (0.01) [p = 0.2]	–0.005 (0.025) [p = 0.86]
L1.Introduction	NA	–0.014 (0.013) [p = 0.28]	NA	–0.018 (0.033) [p = 0.59]	NA	0.025 (0.031) [p = 0.42]	NA	–0.01 (0.018) [p = 0.57]	NA	–0.011 (0.023) [p = 0.65]
n_{Tr}	23	23	20	20	24	24	7	7	2	2
n_{sample}	1,768	1,768	1,613	1,613	1,688	1,688	937	937	472	472
Specification #	1	1	1	1	1	1	1	1	1	1

Table D2. IFE Results: Average Treatment Effects

	Total		Elec. & Heat		Manufacturing		Road transport		Buildings	

Introduction	–0.0147 (0.0072) [p = 0.11]	0.0047 (0.0129) [p = 0.71]	–0.0205 (0.0112) [p = 0.09]	–9e-04 (0.0313) [p = 1]	–0.0049 (0.0169) [p = 0.97]	–0.0267 (0.0315) [p = 0.41]	–0.0071 (0.0114) [p = 0.58]	–0.0258 (0.0313) [p = 0.38]	–0.0308 (0.0118) [p = 0]	–0.0453 (0.0252) [p=0.12]
L1.Introductoon	NA	–0.0219 (0.0141) [p = 0.2]	NA	–0.022 (0.032) [p = 0.5]	NA	0.0246 (0.0298) [p = 0.31]	NA	0.0219 (0.0292) [p = 0.42]	NA	0.0167 (0.0286) [p = 0.57]
n_{Tr}	23	23	20	20	23	23	6	6	2	2
n_{sample}	1,872	1,872	1,692	1,692	1,872	1,872	1,008	1,008	504	504
r	1	1	1	1	1	1	1	1	1	1
Specification #	1	1	1	1	1	1	1	1	1	1

D.1.2 Elasticity Estimates

We report the elasticity estimates obtained using TWFE (D1.2.1) and IFE (D1.2.2). We show the estimation results allowing for both an introduction and a price effect and also report the estimation results when introduction effects are omitted and models are estimated including solely the carbon price in the TWFE model:

$$\Delta\log(CO2)_{i,k,t} = b_k(p_{i,k,t}D_{i,k,t}) + x'_{i,k,t}\beta + \xi_{i,k} + \tau_{k,t} + \epsilon_{i,k,t} \qquad \text{(D3)}$$

and the IFE model:

$$\Delta\log(CO2)_{i,k,t} = b_k(p_{i,k,t}D_{i,k,t}) + x'_{i,k,t}\beta + \xi_{i,k} + \tau_{k,t} + \lambda'_{i,k}F_{k,t} + \epsilon_{i,k,t} \qquad \text{(D4)}$$

TWFE standard errors are clustered at the country level. IFE standard errors are derived using 500 bootstrap draws.

To differentiate between level and growth effects, we also estimate versions including the lag of the carbon price in the TWFE model:

$$\begin{aligned}\Delta\log(CO2)_{i,k,t} = {} & a_k D_{i,k,t} + b_{0,k}(p_{i,k,t}D_{i,k,t}) + b_{1,k}(p_{i,k,t-1}D_{i,k,t}) \\ & + x'_{i,k,t}\beta + \xi_{i,k} + \tau_{k,t} + \epsilon_{i,k,t}\end{aligned} \qquad \text{(D5)}$$

and the IFE model:

$$\Delta \log(CO2)_{i,k,t} = a_k D_{i,k,t} + b_{0,k}(p_{i,k,t} D_{i,k,t}) + b_{1,k}(p_{i,k,t-1} D_{i,k,t}) \tag{D6}$$
$$+ x'_{i,k,t}\beta + \xi_{i,k} + \tau_{k,t} + \lambda'_{i,k} F_{k,t} + \epsilon_{i,k,t}$$

If the level of the carbon price primarily affects the level (rather than the growth rate) of CO_2 emissions, we expect the coefficient on the contemporaneous price variable to have the opposite sign to the coefficient on the lagged price. Lag results are shown in Tables D3 and D4.

Table D3. TWFE Results: Introduction and Price Effects

	Total		Elec. & Heat		Manufacturing		Road transport		Buildings	
Intro	–3e-04 (0.0093) [p = 0.98]	-	–0.0067 (0.0204) [p = 0.75]	–	0.0248 (0.0231) [p = 0.29]	–	–0.0158 (0.0098) [p = 0.12]	–	–0.009 (0.0254) [p = 0.73]	–
Price	–9e-04 (5e-04) [p = 0.09]	–0.001 (0) [p = 0.04]	–8e-04 (9e-04) [p = 0.35]	–0.001 (0) [p = 0.04]	–0.002 (0.0015) [p = 0.18]	–0.001 (0.001) [p = 0.32]	3e-04 (1e-04) [p = 0.01]	0 (0) [p = 0.79]	–2e-04 (7e-04) [p = 0.76]	0 (0) [p = 0.03]
n_{Tr}	23	23	20	20	24	24	7	7	2	2
n_{sample}	1,714	1,714	1,559	1,559	1,688	1,688	937	937	472	472
Spec. #	1	1	1	1	1	1	1	1	1	1

Table D4. IFE Results: Introduction and Price Effects

	Total		Elec. & Heat		Manufacturing		Road transport		Buildings	
Introduction	– 0.0089 (0.009) [p = 0.44]	–	–0.0157 (0.0195) [p = 0.4]	–	0.0239 (0.0227) [p = 0.25]	–	–0.0115 (0.0419) [p = 0.41]	–	–0.0456 (0.027) [p = 0.01]	–
Price	–8e-04 (6e-04) [p = 0.2]	–0.0013 (5e-04) [p = 0.04]	–3e-04 (8e-04) [p = 0.82]	–8e-04 (4e-04) [p = 0.12]	–0.0022 (0.0015) [p = 0.19]	–0.0012 (0.0011) [p = 0.38]	2e-04 (0.0027) [p = 0.46]	0 (8e-04) [p = 0.99]	7e-04 (0.0015) [p = 0.23]	–7e-04 (0.001) [p = 0]
n_{Tr}	23	23	20	20	23	23	6	6	2	2
n_{sample}	1,872	1,872	1,692	1,692	1,872	1,872	1,008	1,008	504	504
r	1	1	1	1	1	1	1	1	1	1
Specification #	1	1	1	1	1	1	1	1	1	1

Table D5. TWFE Results: Lag Prices

	Total		Elec. & Heat		Manufacturing		Road transport		Buildings	
Intro	–0.001 (0.009) [*p* = 0.91]	–0.0069 (0.0081) [*p* = 0.4]	–0.0143 (0.0251) [*p* = 0.57]	–0.0263 (0.0223) [*p* = 0.24]	0.0338 (0.028) [*p* = 0.23]	0.0075 (0.0209) [*p* = 0.72]	–0.0157 (0.0098) [*p* = 0.12]	–0.015 (0.0098) [*p* = 0.14]	–0.0091 (0.0253) [*p* = 0.72]	–0.0124 (0.0222) [*p* = 0.58]
Price	–9e-04 (6e-04) [p = 0.12]	–	–0.001 (9e-04) [*p* = 0.29]	–	–0.002 (0.0015) [*p* = 0.18]	–	2e-04 (4e-04) [*p* = 0.72]	–	–7e-04 (0.0011) [*p* = 0.55]	–
L1.price	1e-04 (6e-04) [*p* = 0.83]	–1e-04 (6e-04) [*p* = 0.9]	7e-04 (0.001) [*p* = 0.47]	5e-04 (0.001) [*p* =0.64]	–7e-04 (0.0013) [*p* = 0.56]	–8e-04 (0.0012) [*p* = 0.54]	1e-04 (5e-04) [*p* = 0.79]	3e-04 (2e-04) [*p* = 0.1]	6e-04 (0.0012) [*p* = 0.62]	–1e-04 (7e-04) [*p* = 0.92]
n_{Tr}	23	23	20	20	23	23	6	6	2	2
n_{sample}	1,714	1,714	1,559	1,559	1,688	1,688	937	937	472	472
Spec. #	1	1	1	1	1	1	1	1	1	1

Table D6. IFE Results: Lag Prices

	Total		Elec. & Heat		Manufacturing		Road transport		Buildings	
Intro	–0.0042 (0.0096) [*p* = 0.78]	–0.0071 (0.0086) [*p* = 0.49]	–0.0224 (0.0275) [*p* = 0.34]	–0.0281 (0.0217) [*p* = 0.19]	0.0418 (0.0288) [*p* = 0.16]	0.0115 (0.0206) [*p* = 0.55]	–0.0112 (0.064) [*p* = 0.38]	–0.0135 (0.0272) [*p* = 0.28]	–0.0467 (0.0281) [*p* =0.03]	–0.0524 (0.0147) [*p* = 0]
Price	–5E-04 (6E-04) [*p* = 0.4]	–	–5E-04 (0.001) [*p* = 0.77]	–	–0.0022 (0.0016) [*p* = 0.17]	–	–5E-04 (0.0051) [*p* = 0.27]	–	–0.0012 (0.0027) [*p* = 0.46]	–
L1.price	–0.001 (8E-04) [*p* = 0.33]	–0.0011 (7E-04) [*p* = 0.24]	7E-04 (0.0011) [*p* = 0.49]	5E-04 (0.0011) [*p* = 0.55]	–0.0014 (0.0012) [*p* = 0.34]	–0.0014 (0.0011) [*p* = 0.32]	9E-04 (0.0024) [*p* = 0.13]	4E-04 (0.0019) [*p* = 0.18]	0.0025 (0.0015) [*p* = 0.22]	0.0012 (7E-04) [*p* = 0.05]
n_{Tr}	23	23	20	20	23	23	6	6	2	2
n_{sample}	1,872	1,872	1,692	1,692	1,872	1,872	1,008	1,008	504	504
r	1	1	1	1	1	1	1	1	1	1

Spec. #	1	1	1	1	1	1	1	1	1	1

D.2 Additional Results Using Synthetic Controls

Within-country estimation results of the panel model of treatment effects as a function of the price level allowing for level or growth effects through the inclusion of lagged carbon prices. Table 2.1 reports the estimation results. If carbon prices affect the level of CO_2 emissions instead of the growth rate, we expect the coefficient on contemporaneous prices to have the opposite sign to the coefficient on lagged prices. There is little evidence supporting level effects, the coefficients are not generally opposite signed, and when the point estimates exhibits the opposite sign, the effects are not statistically different from zero.

Table D7.

Country-Year Specific Treatment Effects from

Panel Model Allowing for Level or Growth Effects

	Total	Electricity and heat	Manufacturing	Road transport	Buildings
P_t	0.001	–0.002	–0.005	–0.001	–0.002
	(0.003)	(0.001)	(0.006)	(0)	(0.001)
P_{t-1}	0.001	0.001	–0.007	0	0.003
	(0.002)	(0.001)	(0.005)	(0)	(0.001)
N_{sample}	171	162	162	38	21
Spec #	1	1	1	1	1

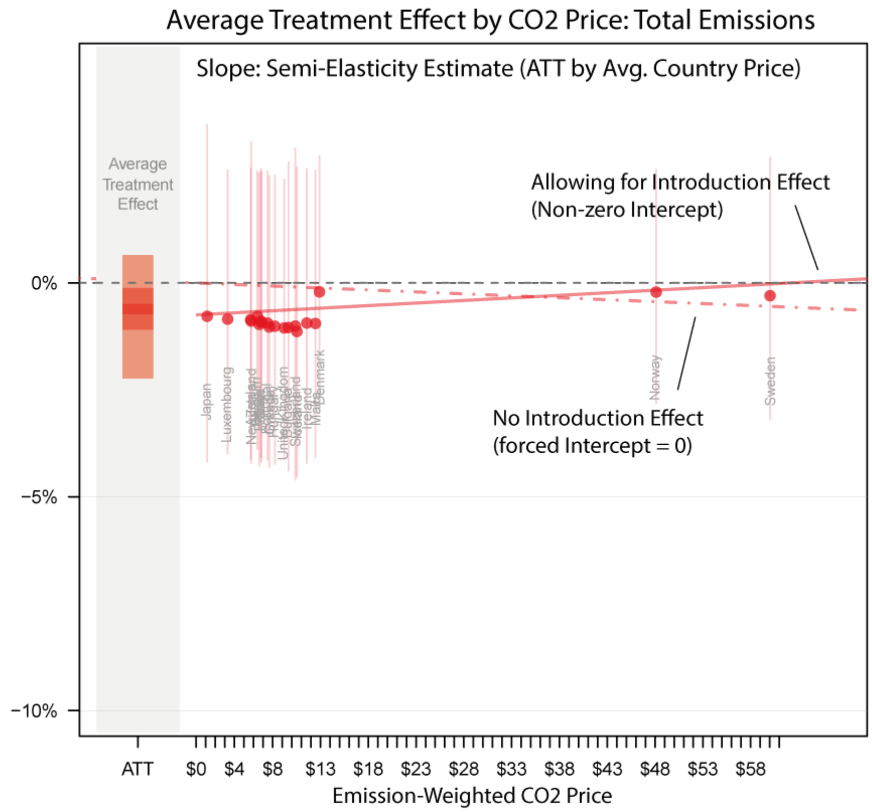

Figure D1.

Treatment Effect Estimates for Specification #6

APPENDIX E.

TESTING FOR LONG-RUN EFFECTS IN EQUILIBRIUM CORRECTION MODEL

We estimate the following panel equilibrium correction (EC) model for each treated country $i_{TR} \in 1,2, \dots N_{TR}$, that has had a sufficiently long treated period $t_{TR} \in t_1, \dots \geq t_{23}$ with respect to carbon pricing in sector k:

$$
\begin{aligned}
\Delta\log(CO2)_{i,k,t} = \;& \alpha_{i,k} + \beta^{EC}_{0,i.k}\log(CO2)_{i,k,t-1} + \beta_{1,i,k}\Delta p_{i,k,t} + \\
& \beta_{2,i,k}p_{i,k,t-1}\beta_{3,i,k}\Delta\log(x')_{i,k,t} + \beta_{4,i,k}\log(x')_{i,k,t-1} + \\
& \omega^{CA}_{0,i,k}\overline{\log(CO2)}_{i,k,t-L} + \omega^{CA}_{1,i,k}\Delta\bar{p}_{i,k,t} + \omega^{CA}_{2,i,k}\bar{p}_{i,k,t-L} + \\
& \omega^{CA}_{3,i,k}\Delta\overline{\log(x')}_{i,k,t} + \omega^{CA}_{4,i,k}\overline{\log(x')}_{i,k,t-L} + \\
& \textstyle\sum_{i,k}^{Ntr}\pi_{0,i,k}\Delta\log(CO2)^{Sel}_{i,k,t-D} + \sum_{i,k}^{Ntr}\pi_{1,i,k}\Delta p^{Sel}_{i,k,t-D} + \\
& \textstyle\sum_{i,k}^{Ntr}\pi_{2,i,k}\Delta x'^{Sel}_{i,k,t-D} + \epsilon_{i,k,t},
\end{aligned}
\tag{E1}
$$

where p is the ECP, the bars indicate cross-section averages of the variables, $\omega^{CA}_{1,i,k}, \dots, \omega^{CA}_{4,i,k}$ are the unknown coefficients for the cross-section averages, and the superscript Sel indicates that the number of lags of first-differenced variables (which may be heterogeneous of i) are selected using a general-to-specific lag truncation procedure.[46] We investigate cointegration between the variables by assessing the EC coefficient $\beta_{0,i,k}$ in (E.E1). Specifically, we compute the unweighted mean-group EC coefficient as

[46] For each country i, the largest lag of each variable in first differences (up to $t-2$) is dropped if it is insignificant at the 10 percent level, and then the selection procedure is repeated until the largest lags of the variables in first differences are significant (if any).

$\sum_i(\beta_{0,i,\ldots N,k,\ldots K})/N$ and obtain the average t-statistic and corresponding p-value based on the critical values in Gengenbach et al. (2016). To determine whether the long-run average emissions semielasticity (with respect to the carbon price) is significantly different from zero, we compute the long-run average coefficient as

$$\varrho = -(\sum_{i,k}(\omega_{1,i,k}, \ldots \omega_{C,i,k}) \Big/ \sum_{i}(\omega_{0,i,k})) \tag{E2}$$

where the standard error, $\bar{T}$ statistic, and p-value are computed using the Delta method.[47] To assess whether augmenting the equation with cross-section averages of the variables is effective at removing cross-section dependence, we apply the test of weak cross-section dependence developed in Pesaran (2015) to the dependent and independent variables and model residuals. Consistent with Kapetanios et al. (2011) and Chudik and Pesaran (2015), we find that adding a sufficient number of lags of cross-section averages, $L^{CA} = T^{\wedge 1/3} - 1$, in model (9) is a powerful means of resolving cross-sectional dependence (see CD tests in Table E.1, confirming that the residuals are cross-sectionally independent). The $\bar{T}$ statistic in Table E.1 leads us to reject the null hypothesis of no cointegration at the 1 percent level. The average long-run coefficient is significant.

[47] We compute the equilibrium correction models and associated misspecification tests via xtcaec in Stata.

Table E1. Average Long Run Semielasticity

(Dependent variable: $\Delta\log(CO2)_{i,k,t}$,

in panel mean-group equilibrium correction model]

	Total	Manufacturing	Road transport
Average long run semielasticity	–1.57 percent	–0.6 percent	–2.55 percent (1.39)
	(0.4)	(0.2)	[–.0529, 0.0018]
$\beta_{0,i.k}^{EC}$	–1.058	–1.104	–.6107
	(.432)	(.372)	(0.283)
Short run marginal semielasticity	–1.06 percent	–0.32 percent	–0.76 percent
	(0.82)	(0.15)	(0.77)
Treated countries	2	5	3
Treated observations	50	119	
Total observations			129
Countries used to compute [CA]	39	39	39
RMSE	0.0119	0.0283	0.0121
Panel EC $\bar{T}$ test for $\log(CO2)_{i,k,t-1}$	–4.480	–7.141	–3.574
	[$p \leq 0.01$]	[$p \leq 0.01$]	[$p \leq 0.05$]
CD test for $\log(CO2)_{i,k,t}$	–5.116	7.549	0.387
	(0.000)	(0.000)	[$p = 0.699$]
CD test for $\epsilon_{i,k,t}$	1.6	1.757	–0.612
	[$p = 0.109$]	[$p = 0.079$]	[$p = 0.540$]

Note: All mean-group coefficients are calculated as unweighted means of the country-specific estimates. Standard errors in parentheses are derived nonparametrically following Pesaran and Smith (1995). The 95 percent confidence intervals for elasticity estimates are in brackets. $\beta_{0,i.k}^{EC}$

denotes the speed of equilibrium adjustment; the panel EC $\bar{T}$ statistic tests the significance of the cointegrating relationship; RMSE is the root mean squared error; and “CD test” refers to the Pesaran (2015) test for weak cross-section dependence, under the null hypothesis of cross-section independence.

Appendix F.

Assessing Heterogeneity of Effects in Emission Trading Versus Carbon Tax Schemes

We provide the estimation results when the models are limited to ETS or carbon taxes (relative to the overall carbon pricing results regardless of the nature of the pricing scheme reported in the main text). When estimating the treatment effects and semielasticities for carbon price in either ETS or under carbon tax schemes, we limit potential control countries to include only those without any carbon pricing scheme to ensure clean control groups in all specifications. For example, a country that operates a carbon tax scheme is not included as a potential candidate for the control group when assessing the impact of introducing an ETS.

Tables F1 and F2 show the estimation results for ETS-only treatments; Tables F3 and F4 show results for carbon tax–only treatments.

Table F1. Estimating ETS-Specific Impacts

	Total	Electricity and heat	Manufacturing	Road transport	Buildings
ATT	–0.018 (0.017) [*p* = 0.07]	–0.032 (0.018) [*p* = 0.03]	–0.014 (0.018) [*p* = 0.48]	–0.005 (0.004) [*p* = 0.27]	NA
Δlog(GDP)	0.40424 (0.65553)	–0.68713 (0.98872)	–0.49469 (1.66986)	–0.74685 (0.76339)	NA
Δlog(GDP)^2	–0.00539 (0.02634)	0.04767 (0.042)	0.03252 (0.07279)	0.04412 (0.03003)	NA
Δlog(population)	0.38441 (0.1639)	0.10163 (0.25968)	–0.06342 (0.41497)	0.36537 (0.43272)	NA
Δlog(servicesGDP)	NA	NA	NA	NA	NA
Δlog(servicesGDP)^2	NA	NA	NA	NA	NA
Δlog(manfacturingGDP)	NA	NA	1.68059 (0.76086)	NA	NA
Δlog(manfacturingGDP)^2	NA	NA	–0.06737 (0.04254)	NA	NA
Δlog(transportGDP)	NA	NA	NA	0.5378 (0.46355)	NA
Δlog(transportGDP)^2	NA	NA	NA	–0.01834 (0.02164)	NA
Δlog(heatingdegreedays)	NA	NA	NA	NA	NA
Δlog(coolingdegreedays)	NA	NA	NA	NA	NA

r	1	1	1	1	NA
N_{TR}	10	12	15	1	NA
N_{CO}	27	25	27	12	NA
Specification #	1	1	1	1	NA

Table F2. Estimating ETS-Specific Impacts

	Total	Electricity and heat	Manufacturing	Road	Buildings
Elasticity (between-country)	–0.004 percent (–0.942 percent, 0.712 percent)	–0.015 percent (–1.324 percent, 1.463 percent)	–0.246 percent (–1.475 percent, 0.984 percent)	NA	NA
Elasticity (within-country)	0.019 percent (–0.629 percent, 0.563 percent)	0.126 percent (–0.096 percent, 0.367 percent)	–0.294 percent (–0.736 percent, 0.094 percent)	NA	NA
n_{Tr} (between-country)	10	12	15	NA	NA
n_{Tr} (within-country)	10	12	15	NA	NA
F test for poolability of price coefficients	$p = 0.681$	$p = 0.931$	$p = 0.005$	NA	NA
F test for poolability of introduction effects	$p = 0.012$	$p = 0.497$	$p = 0.016$	NA	NA
Spec. #	1	1	1	NA	NA

Table F3. Estimating Tax-Specific Impacts

	Total	Electricity and heat	Manufacturing	Road transport	Buildings
ATT	NA	NA	NA	–0.007 (0.017) [p = 0.54]	–0.03 (0.014) [p = 0.02]
Δlog(GDP)	NA	NA	NA	–0.04219 (0.45828)	–2.07581 (1.91964)
Δlog(GDP)^2	NA	NA	NA	0.01941 (0.02164)	0.08036 (0.06945)
Δlog(population)	NA	NA	NA	0.22258 (0.18995)	1.45328 (0.87253)
Δlog(servicesGDP)	NA	NA	NA	NA	1.94165 (1.35541)
Δlog(servicesGDP)^2	NA	NA	NA	NA	–0.07451 (0.0569)
Δlog(manfacturingGDP)	NA	NA	NA	NA	NA
Δlog(manfacturingGDP)^2	NA	NA	NA	NA	NA
Δlog(transportGDP)	NA	NA	NA	0.13622 (0.12511)	NA
Δlog(transportGDP)^2	NA	NA	NA	–0.00145 (0.00802)	NA
Δlog(heatingdegreedays)	NA	NA	NA	NA	NA
Δlog(coolingdegreedays)	NA	NA	NA	NA	NA
r	NA	NA	NA	1	1
N_{TR}	NA	NA	NA	5	2

N_{CO}	NA	NA	NA	21	12
Specification #	NA	NA	NA	1	1

Table F4. Estimating Tax-Specific Impacts

	Total	Electricity and heat	Manufacturing	Road transport	Buildings
Elasticity (between-country)	NA	NA	NA	0.032 percent (–0.063 percent, 0.1 percent)	0.059 percent (–0.159 percent, 0.193 percent)
Elasticity (within-country)	NA	NA	NA	–0.015 percent (–0.146 percent, 0.074 percent)	–0.063 percent (–0.215 percent, 0.191 percent)
n_{Tr} (between-country)	4	4	4	9	6
n_{Tr} (within-country)	NA	NA	NA	9	6
F test for poolability of price coefficients	NA	NA	NA	$p = 0.074$	$p = 0.98$
F test for poolability of introduction effects	NA	NA	NA	$p = 0.743$	$p = 0.744$
Spec. #	2	2	2	2	2

Supplementary Material

The data and R code required to replicate the model results in this study will be made available upon request and be accessible online upon final publication of the manuscript.

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