Carbon pricing and the elasticity of CO₂ emissions

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Abstract   We study the impact of carbon pricing on CO2emissions across five sectors for a panel of 39 countries over 1990-2016. Using newly constructed sector-level carbon price data, we implement a novel approach to estimate the changes in CO2emissions associated with (i) the introduction of carbon pricing regardless of the price level; (ii) the implementation effect as a function of the price level; and (iii) post-implementation marginal changes in the CO2price.We find that the introduction of carbon pricing has reduced growth in CO2emissions by 1% to 2.5% on average relative to counterfactual emissions, with most abatement occurring in the electricity and heat sector. Exploiting variation in carbon pricing to explain heterogeneity in treatment effects, we find an imprecisely estimated semi-elasticity of a 0.05% reduction in emissions growth per average \$1/metric ton (hereafter abbreviated as: ton) of CO2.After the carbon price has been implemented, each marginal price increase of \$1/tCO2 has temporarily lowered the growth rate of CO2emissions by around 0.01%. These are disappointingly small effects. Simulating potential future emissions reductions in response to carbon price paths, we conclude that –in the absence of complementary non-pricing policy interventions – carbon pricing alone, even if implemented globally, is unlikely to be sufficient to achieve emission reductions consistent with the Paris climate agreement.

Keywords   Carbon Pricing, CO2 Emissions, Decarbonization, Carbon Tax, Climate Change, Climate Policy

JEL Classification   Q43, Q48, Q54, Q58, H23

Carbon Pricing and the Elasticity of CO₂ Emissions

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ABSTRACT

We study the impact of carbon pricing on CO₂ emissions across five sectors for a panel of 39 countries over 1990-2016. Using newly constructed sector-level carbon price data, we implement a novel approach to estimate the changes in CO₂ emissions associated with (i) the introduction of carbon pricing irrespective of the price level; (ii) the implementation effect as a function of the price level; and (iii) post-implementation marginal changes in the CO₂ price. We find that the introduction of carbon pricing has reduced growth in CO₂ emissions by 1% to 2.5% on average relative to counterfactual emissions, with most abatement occurring in the electricity and heat sector. Exploiting variation in carbon pricing to explain heterogeneity in treatment effects, we find an (imprecisely estimated) semi-elasticity of a 0.05% reduction in emissions growth per average $1/tCO₂. After the carbon price has been implemented, each marginal price increase of $1/tCO₂ has temporarily lowered the growth rate of CO₂ emissions by around 0.01%. Simulating potential future emissions reductions in response to carbon price paths, we conclude that – in the absence of additional non-pricing policies – carbon pricing alone is unlikely to be sufficient to achieve emission reductions consistent with the Paris climate agreement.

JEL CODES: Q43; Q48; Q54; Q58; H23.

I. INTRODUCTION

Pricing carbon dioxide (CO_2) emissions – either via a carbon tax, emissions trading system, or hybrid scheme – has long been recommended as an integral and, in principle, cost-efficient means of reducing emissions and mitigating the 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 carbon taxes. Similarly, since the European Union (EU) implemented the world's first emissions trading system (ETS) covering CO_2 emissions in 2005, the number of carbon markets has grown to 29, with additional carbon markets scheduled for implementation in China and Germany in 2021. Carbon pricing initiatives now exist in 45 national and 32 subnational jurisdictions, covering one-fifth of global greenhouse gas emissions (or 12 gigatons of CO_2 equivalent emissions (GtCO2e) annually). These initiatives raised public revenues totalling US$45 billion in 2019 (World Bank 2020).

However, behind the undeniable proliferation and popularization of the carbon pricing paradigm over the past decade there is a great uncertainty over its role in climate policy. Critics and endorsers alike concede that what is cost-efficient and environmentally effective in theory may be politically unfeasible in practice (Rosenbloom *et al.* 2020*a*; Stiglitz 2019). Under the 2015 Paris Agreement, 195 countries committed to preventing dangerous levels of climate change this century by maintaining global average surface temperatures below 1.5°C relative to pre-industrial conditions, but this would necessitate a 50% reduction in global emissions in 2030 relative to 2020 (UNEP 2019). In the Economists' Statement on Carbon Dividends (2019), which claims to be the largest public statement in the history of the economics

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[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 tonne of CO_2, referred to as the 'social cost of carbon' (SCC) (Gillingham and Stock 2018). However, global SCC estimates can range from US$10/$tCO_2$ to US$1,000/$tCO_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 that which minimizes the cost of achieving a desired quantity of CO_2 reductions over a given period (Hepburn 2017).

profession, carbon pricing is hailed as the tool of choice to achieve these emissions reductions at the '*scale and speed that is necessary*'.[2] According to the Stern-Stiglitz High-Level Commission on Carbon Prices (2017), explicit carbon prices in the range of $\geq$US\$40–80/tCO$_2$ by 2020 and $\geq$US\$50–100/tCO$_2$ by 2030 will be 'indispensable' to achieving Paris targets, albeit with the proviso that they are combined appropriately with complementary policies.[3] However, such assessments have been proffered based on ex ante, theory-laden projections with limited empirical corroboration. For context, currently implemented carbon prices range from $<$\$1/tCO$_2$ in Poland and Ukraine to \$119/tCO$_2$ in Sweden (in nominal terms), and nearly half of all covered emissions worldwide are priced at less than \$10/tCO$_2$ (World Bank 2020).[4] Globally, the average (emissions-weighted) carbon price is below \$3/tCO$_2$ (Dolphin *et al.* 2020), equivalent to adding approximately US\$0.03 per gallon of gasoline (€0.009 per litre of petrol).

Empirical evaluations of the impact of implemented carbon prices on CO$_2$ emissions have been mixed, inconclusive, and scarce. We report the main empirical findings and evaluation methods of previous studies in §III. Our key takeaway from the literature review in §III is that, thus far, the fragmentary nature of this burgeoning evaluation literature precludes systematic inference on the likely response of emissions to carbon pricing across space and time. As we describe in §IV, the paucity of empirical assessments hitherto 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

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[2] Emphasis is ours.

[3] As Stiglitz (2019) cautions, carbon price paths will inevitably vary across heterogeneous socio-political 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).

[4] As of May 2020. All monetary units throughout this study are in 2015 US dollars.

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, our purpose here is to present a viable empirical modelling approach that largely overcomes the aforementioned 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 carbon-pricing 'treatment effects' 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 pursuance of 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). The practical consequence of these methodological setbacks is that policymakers and the public still know arguably little about the environmental effectiveness hitherto of one of the core pillars of climate policy.

In this study, we construct a novel dataset comprising average (emissions-weighted) carbon prices across five sectors for a panel of 39 OECD countries from 1990-2016, following the computation methodology of Dolphin *et al.* (2020), combined with emissions data from 1975-2016. We aim to answer three questions: First, what is the effect of the introduction of carbon pricing on CO_2 emissions, irrespective of the level of the carbon price? Second, do higher carbon price levels lead to greater reductions in CO_2 emissions? Third, conditional on having already implemented a carbon price, what is the effect of subsequent year-on-year changes in the price level?

To address these questions, we report three sets of estimated effects for each sector. First, we estimate the 'average treatment effect' of the introduction of a carbon price irrespective of the price level. To overcome challenges in identification of treatment effects in conventional difference-in-differences and synthetic control approaches, we apply treatment evaluation methods based on matrix completion with

staggered adoption (Xu 2017; Athey *et al.* 2018) while controlling for unobserved time-varying heterogeneity using interactive fixed effects (Bai 2009).

Second, beyond average treatment effects, we propose a new approach to estimating elasticities from synthetic control methods by estimating what we refer to as the 'implementation (semi-)elasticity' – i.e. the change in the growth rate of CO_2 emissions as a function of the level of the carbon price. Specifically, we estimate the implementation semi-elasticity by assessing whether heterogeneity in treatment effects estimated in the first stage can be explained by variation in the treatment intensity provided by different carbon price levels observed within and between countries over time.

Third, using interactive fixed effects panel models we estimate the effect of price changes on CO_2 emissions, conditional on having already implemented a carbon price. We refer to this as the 'marginal (semi-)elasticity' of emissions with respect to carbon pricing.

Finally, we combine our estimates of the implementation and marginal elasticities with climate model projections of future CO_2 emissions from several indicative reference scenarios to study the emissions abatement potential of different hypothetical pricing schemes over the next three decades.

We find that the average treatment effect of carbon pricing (weighted to account for different lengths of treatment) corresponds to a significant -1.5% reduction in economy-wide 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% relative to counterfactual). We find that the implementation (semi-)elasticity – the change in the growth of CO_2 emissions as a function of the level of the carbon price – is negative but imprecisely estimated for most sectors. Median estimates for aggregate emissions suggest a reduction of around -0.07% for each additional $1/tCO_2$ albeit with high uncertainty and only statistically significant for the manufacturing sector (-0.16% for each additional $1/tCO_2$). Furthermore, we find that the marginal (semi-)elasticity – the change in the growth of CO_2 emissions in response to a $1/tCO_2$ price increase conditional on having already implemented a carbon price – has been a -0.16% reduction in total aggregate

emissions, but this effect has been driven predominantly by the electricity and heat sector (-0.26%). Finally, in robustness checks we confirm that these results are robust across a range of model specifications. We additionally test several equilibrium correction model specifications in order to accommodate global stochastic trends affecting CO_2 emissions; in doing so, we reject the null hypothesis of 'no cointegration' for the economy-wide and manufacturing sector models, with each \$1/tCO$_2$ price increase generating an average long run change in the growth rate of emissions of -1.6% for total emissions and -0.6% for manufacturing emissions. These robustness checks cohere with and complement our core findings, but we note that a longer time horizon of observed carbon prices will be required before future research can more fully explore the implications of cointegration when estimating the (potentially larger) emissions reductions associated with carbon pricing over the long run.

Combining our empirical estimates of the implementation and marginal elasticities with projected future emissions, we show that carbon pricing at current observed levels even if implemented globally are likely insufficient to achieve emission reductions at the *scale and speed necessary* to achieve the commitments of the Paris agreement (or even substantial reductions at all). Achieving the required emission reductions in line with the Paris agreement requires global carbon pricing with 100% emission coverage in excess of \$110/tCO$_2$, which is roughly 50% higher than the current highest existing (emissions-weighted) carbon price in Sweden.

After describing the core elements of carbon-pricing theory that inform our empirical investigation (§II) and reviewing the prior evidence from the evaluation literature (§III), we describe the standardized carbon price data used to estimate emissions elasticities (§IV). We then explicate our identification strategy, baseline model specification, and estimation procedure (§V). After summarizing the country-level and sector-level results across 24 model specifications (§VI), we conclude with reflections on the policy implications of our findings (§VII).

II. 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.[5] 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.[6] 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, however, some additional clarification regarding the functional form of the relationship. First, note that the pollution demand schedule can be reinterpreted as a marginal abatement cost schedule: given that the demand schedule provides information about the marginal willingness to pay for emissions, it also provides - 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 this relationship 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). Empirical investigations of CO$_2$

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5. The pollution demand schedule indicates the response of a sector's emissions to a given price of emitting each unit of CO$_2$.

6. 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 the undertaking of 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 §III, however, suggests that carbon prices have triggered at least some CO$_2$ abatement.

abatement options have, however, found the marginal abatement cost curves for specific jurisdictions or regions to be mostly linear at low carbon prices, with abatement costs rising steeply only towards the end of the curve (Goulder and Hafstead 2017). In other words, empirical CO_2 demand schedules appear to be much flatter than theoretically assumed, at least at the historically implemented carbon price levels considered herein (see §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 time period analysed herein, the appropriate model specification may be linear. We return to the question of functional form in Appendix C with mis-specification tests of our baseline model formulation; ultimately, the tests corroborate our initial conjecture that nonlinear relations are absent or non-detectable in the short sample and insignificant at hitherto observed carbon price levels, and we conclude that a linear specification is appropriate.

III. 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 hitherto fall into 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; Zhang *et al.* 2016; Edenhofer *et al.* 2010; Mercure *et al.* 2016; Ellerman and Buchner 2008).[7] Our study is concerned principally with retrospective policy evaluation, and we thus focus on ex post methods henceforth.

In contrast to simulation-based assessments, ex post evaluations of implemented carbon pricing policies on emissions remain scarce and often absent of elasticity estimates, 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 (e.g. see discussions in OECD 1997; Andersen 2004; Andersen *et al.* 2000; 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 econometric policy response model correspond closely with those derived from a large CGE model, suggesting that the former are not undermined by general equilibrium effects and can provide empirical evidence that informs subsequent parametrization of the latter.

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[7] The reliance on ex ante simulation approaches hitherto is understandable given the data-related challenges attendant to empirical carbon pricing evaluations (see §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.

Table 1. Empirical evaluations of implemented carbon prices and associated CO₂ 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	Dynamic panel model with propensity score matching for priced and unpriced firms	EU ETS	CO_2 emissions growth rate (firm level)	-3% in '07/'08 relative to '05/'06 (-6% for firms with greatest decrease in free allocation)	N/A
Gloaguen and Alberola (2013)	EU	2005-2012	Dynamic panel model with propensity score matching	EU ETS	CO_2 emissions	-10% (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; 12% of total	N/A
Dechezleprêtre *et al.* (2018)	EU	2005-2012	DiD	EU ETS	CO_2 emissions (plant level)	-6% during Phase I (2005-2007) and -15% during Phase II (2008-2012)	-2% during Phase I and -3% during Phase II
Bayer and Aklin (2020)	EU	1990-2016	GSC method with IFE model (using unpriced sectors to impute counterfactuals for priced sectors)	EU ETS	Sector/industry-level CO_2 emissions (energy, metals, minerals, chemicals, and aggregate for priced sectors)	-7.5% (-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% in 2018
Wagner *et al.* (2014)	Germany	1995-2010	DiD	EU ETS	CO_2 emissions (plant level)	-20% during Phase II	
Jaraite and DiMaria (2016)	Lithuania	2003-2010	DiD	EU ETS	CO_2 emissions (plant level)	Insignificant	Insignificant
Metcalf and Stock (2020*b*)	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% over a 6-year period for a $40/$tCO_2$ tax covering 30% of CO_2 emissions.	N/A
Martin *et al.* (2014)	United Kingdom		Two-stage least squares IV	UK Climate Change Levy	Manufacturing sector CO_2 emissions (plant level)	-7.3%	N/A
Abrell *et al.* 2020	United Kingdom	2013-2016	ML counterfactual inference	UK Carbon Price Support	Electricity sector CO_2 emissions (high frequency plant-level data)	-6.2%	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%	N/A
Leroutier (2018)	United Kingdom		Counterfactual based on synthetic control of EU countries	UK Carbon Price Support	Electricity sector CO_2 emissions (high frequency plant-level data)	-49%	N/A
Andersson (2019)	Sweden	1960-2005	DiD and synthetic control	Carbon tax (transport sector)	Transport sector CO_2 emissions	N/A	-6.3% 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% decline in growth rate in Finland only
Rivers and Schaufele (2015)	British Columbia	1990-2011	Panel model using changes in gasoline consumption (relative to a simulated counterfactual) as a proxy for changes in road transport CO_2 emissions	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 using household gasoline consumption in BC relative to the rest of Canada as a proxy for the change in emissions	Carbon tax	Province-level CO_2 emissions from gasoline consumption relative to the rest of Canada	-1.13% to -4.87% (5 years)	<-0.97%
Erutku and Hildebrand (2018)	British Columbia	1991-2015	DiD using per capita gasoline use in BC relative to the rest of Canada as a proxy for the change in emissions	Carbon tax	CO_2 emissions from gasoline consumption relative to the rest of Canada	-0.26% to 10.3% (5 years)	<-2.0%
Pretis (2020)	British Columbia	1990-2016	DiD, synthetic control, and break detection	Carbon tax	Aggregate and sectoral CO_2 emissions	Average long-run decrease of 19% (diff-in-diff) and 3-15% decrease (synth.) in road transport CO_2 emissions (2008-2016)	+$5/$tCO_2$ increase → -1% reduction in road transport emissions
Best *et al.* (2020)	42 countries	2012-2017	Cross-sectional and panel models of emissions growth rates with many controls	'Effective carbon rate' including taxes and ETSs	Growth rate of road transport CO_2 emissions and aggregate emissions of all non-road sectors	Average annual decrease of -2% relative to countries without a price	Marginal decrease of -.03% for a €1/tCO_2 price increase

Note: DiD = Difference-in-differences; IV = instrumental variable; LP = local projection; SVAR = structural vector autoregression; GSC = generalised synthetic control; IFE = interactive fixed effects; ML = machine learning; RDiT = regression-discontinuity-in-time.

The available evidence from ex post carbon pricing evaluations, summarized in Table 1, has been mixed and inconclusive. Nevertheless, we can infer a few basic facts from the nascent literature: (i) no study, it is fair to say, has managed to identify an instrumental variable that can credibly isolate exogenous variation in the carbon price, and understandably so given that the significant correlates of observed carbon price levels, such as domestic coal dependencies, are also correlates of CO_2 emissions; (ii) therefore, researchers aspiring to make 'causal' inferences regarding the environmental efficacy of carbon pricing instruments have typically adopted a quasi-experimental approach usually based on difference-in-differences (DiD), synthetic control, and related matrix completion methods, generating policy-response estimates whose reliability depends largely on the similitude of treated and untreated units, as well as how one judges the 'verisimilitude' of imputed counterfactuals; (iii) only one study, Best *et al.* (2020), has attempted to estimate emissions elasticities in a cross-country panel setting using standardized carbon prices (based OECD (2016) data on 'effective carbon rates'), but the time horizon is short (2012-2017) and the authors do not estimate counterfactual emissions, relying instead on causal inference based on correlational evidence from panel regressions with many controls;[8] (iv) policy-response estimates are heterogeneous across regions and sectors, but it remains difficult to draw meaningful comparisons across space and time; and (v) generally speaking, ex post evaluations detect less CO_2 abatement than ex ante studies, but again we are precluded from making any systematic comparisons given the fragmentary nature of the available evidence.

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[8] Included controls include 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.

IV. DATA—EMISSIONS-WEIGHTED CARBON PRICE

Economic theory has long recommended using a single, uniform price signal to reduce CO_2 emissions at minimal cost,[9] provided that the public authority can credibly commit to an escalating price path (or declining emissions cap) and assuming the absence of transaction costs.[10] Contrary to 'first-best' theory, however, experience has shown that governments are routinely constrained by domestic political economy concerns that inhibit 'optimal' carbon pricing, while the transaction costs associated with implementing and sustaining carbon pricing instruments in some sectors are far from trivial.

Political economy constraints have largely been a function of the varied and largely effective lobbying strategies and tactical rent-seeking of the largest carbon-exposed companies whose success in stifling carbon pricing's ascent has been copiously documented (Grubb 2014; Helm 2010; Jenkins 2014; Dolphin *et al.* 2020). Notably, this includes the lobbied opposition of peak business associations representing industries exposed to carbon costs through extensive supply chain linkages (Cory *et al.* 2020). Beyond heeding the competitiveness 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 price.

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[9] The externality associated with each tonne of CO_2 emitted to the atmosphere is the same regardless of its source (i.e. country, sector, or technology of origin). Therefore, assuming a policymaker wants to set the carbon price equal to the monetized damages from emitting an additional tonne of CO_2, any departure from a single, economy-wide 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 economy-wide.

[10] If transaction costs (e.g. costs 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. Insofar as only CO_2 emissions are covered, there are various strategic points at which fossil fuels, for example, can be priced either upstream, midstream, or downstream to minimize transaction costs. There are, however, technical difficulties in implementing schemes covering other greenhouse gases and, hence, it might be sub-optimal to aim for 100 percent coverage of GHG emissions.

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,[11] often with the intention of achieving multiple policy objectives simultaneously. The pattern is consistent with the principle, popularized by Tinbergen (1952), that there ought to be at least as many policy instruments as there are market failures to be corrected.[12]

This has introduced a major impediment to economy-wide (let alone cross-national) empirical evaluations of price-induced CO_2 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.[13] The problem is compounded by the relatively short timeframe (<5 years) covered by available carbon price data sources (OECD 2018; World Bank *et al.* 2018).

We overcome this impediment by compiling 'emissions-weighted carbon price' (ECP) data at a sector level for a panel of 35 countries from 1990-2016. The ECP data have been updated from the original aggregate (economy-wide) CO_2 prices presented in Dolphin *et al.* (2020). Here we have applied the same computation methodology not only to obtain the economy-wide 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

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[11] Examples include a panoply market-based and non-market policy instruments including, *inter alia*, product standards, building regulations, emission limits for power plants, renewable energy auctions, RD&D, grants and subsidies, public infrastructure investments, and product bans.

[12] In the hypothetical situation of a policymaker desiring to achieve only the singular 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.

[13] 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.

coverage and price information at the sector-fuel level, in combination with sector-fuel CO_2 emissions data. A summary of the computation methodology 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 carbon price-induced incentive to reduce aggregate CO_2 emissions cross-nationally, making carbon prices truly comparable for panel econometric purposes.[14] 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.[15] This study goes one step further and estimates not only the generic treatment effect but also emissions elasticities with respect to the level and interannual change of price.

Table 2 highlights the disparity between nominal and emissions-weighted carbon prices. For example, Sweden's nominal carbon price was \$130/tCO_2 in 2015, but its average emissions-weighted carbon price (accounting for exemptions and coverage restrictions) was approximately \$76/tCO_2. Likewise, Switzerland's highest nominal carbon price in 2015 was \$50/tCO_2, but its average emissions-weighted price 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 heat maps in Figure 1.

Equipped with the ECP data, we proceed in §V to describe our model specification and identification approach.

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[14] While 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), here we use the ECP for the first time as an independent variable.

[15] The few studies that have incorporated empirical information on carbon price levels within a quasi-experimental evaluation framework have been confined to one (or a small number of) jurisdictions (e.g. Andersson 2019; Pretis 2020).

Table 2.

Nominal vs. emissions-weighted carbon prices in selected jurisdictions, 2015 (US$/tCO₂)

	Nominal CO_2 price	Emissions-weighted CO_2 price	% 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 carbon price levied within the jurisdiction in 2015, without accounting for sectoral, industrial, or fuel-specific exemptions. The ECP values are based on the average (economy-wide) CO_2 price level.

Figure 1.

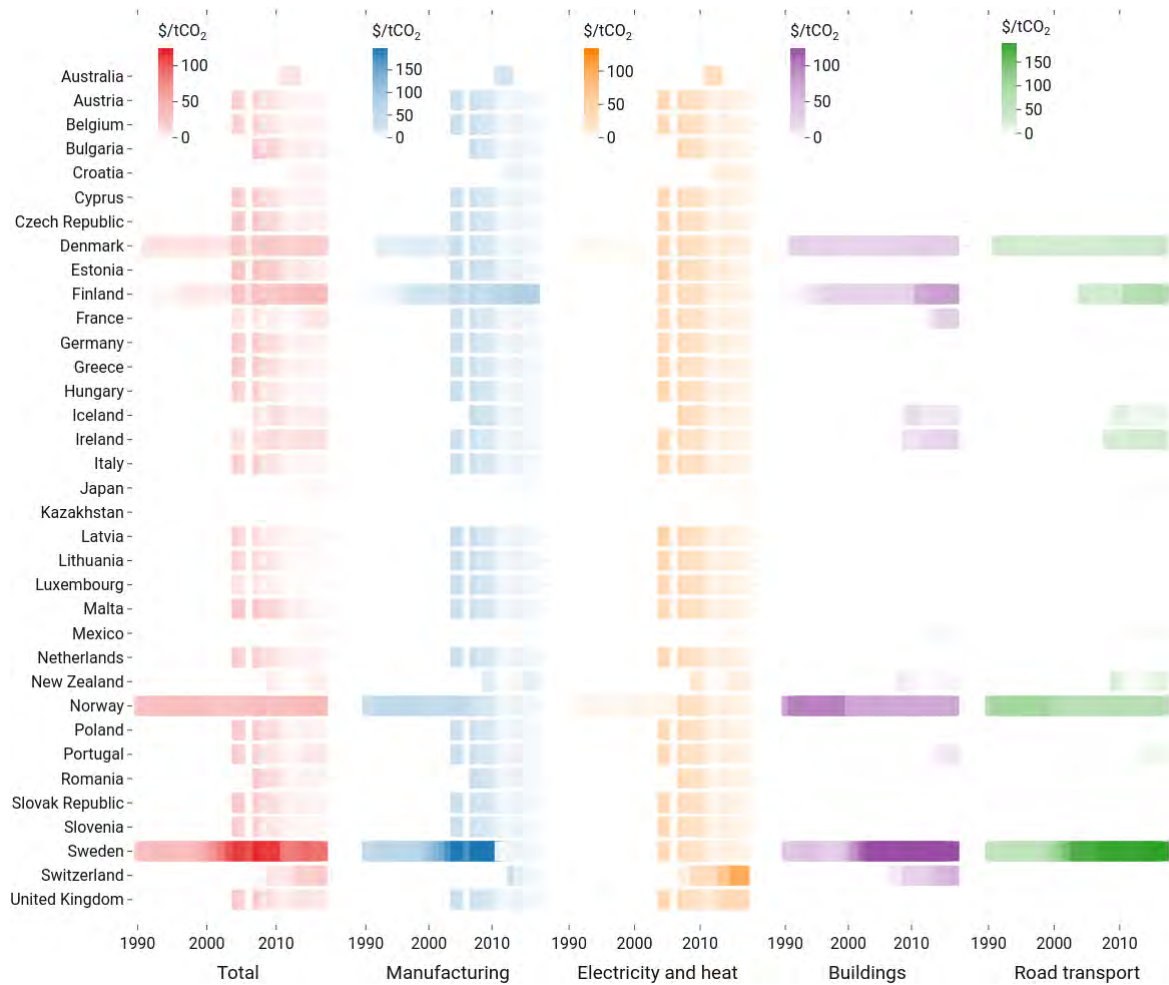**Carbon Price Coverage and Stringency Across Countries and Sectors (1990-2016)**

Note: Colour-coded tiles indicate the presence of a carbon pricing initiative (tax and/or ETS) in a given year, with higher opacity (darker tiles) reflecting higher carbon price levels (2015 US$/tCO₂). Based on emissions-weighted carbon price (ECP) data updated from Dolphin *et al.* (2020) for purposes of sector-level analysis.

V. ESTIMATING THE IMPACTS OF CARBON PRICING

Using sector-level observations on emissions we first estimate the average treatment effect of the introduction of carbon pricing on the growth rate of CO_2 emissions irrespective of the price level using generalised synthetic control methods for policy evaluation under staggered adoption, i.e. in which multiple treated units introduce the policy at varying points in time. (§V.1 and §V.1.1). Using emissions-weighted carbon pricing, we then quantify the implementation semi-elasticity of CO_2 emissions with respect to the carbon price level by proposing a new approach to decompose variation in the treatment effect using variation in the treatment intensity provided by different levels of carbon pricing (§V.2 and §V.2.1). Subsequently, we estimate the marginal (semi-) elasticity of carbon pricing using interactive fixed effects to quantify the effect of additional price changes conditional on having implemented a carbon price (§V.3 and §V.3.1).

V.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 growth of CO_2 emissions. We are faced with multiple treated units, many of which implemented carbon pricing schemes at different points in time and potentially exhibit pre-treatment trends. Conventional difference-in-differences estimators rely on the restrictive assumption of parallel trends in the outcomes of treated and control units, while standard synthetic control estimates were designed for a single treated unit. We therefore employ recent developments in the treatment evaluation and matrix completion literature on staggered adoption. Specifically, we apply the generalised synthetic control estimator proposed by Xu (2017) based on panel interactive fixed effects (IFE) models (Bai 2009). 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:

$$\Delta \log(CO2)_{i,k,t} = \delta_{i,k,t} D_{i,k,t} + x'_{i,k,t} \beta + \xi_{i,k} + \tau_t + \lambda'_{i,k} F_t + \epsilon_{i,k,t} \qquad (1)$$

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

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

where $D_{i,k,t}$ is a treatment dummy denoting the presence or absence of a carbon price at time, t, and $\delta_{i,k,t}$ denotes the parameter of interest – the (potentially) heterogeneous sector-specific treatment effect, capturing the change in emissions attributed to the carbon price conditional on its introduction. We control for q observed time-varying covariates $x' = [x'_1, \dots, x'_q]'$, including the country-level population growth rate, growth in real aggregate GDP (and its square), as well as growth in sector-level GDP (and its square) where available.[16] We investigate a wide range of specifications in robustness checks (§V.4), several of which include population-weighted heating degree days and cooling degree days as additional control variables to capture the impact of weather – unusually cold winters or hot summers – on changes in energy demand and emissions (Mistry 2019). The baseline model specification includes unit fixed effects, $\xi_{i,k}$, and time fixed effects, τ_t, which enter the model additively. 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; and $\epsilon_{i,k,t}$ are unobserved idiosyncratic mean zero shocks.

We treat the latent common factors F_t and factor loadings $\lambda'_{i,k}$ as interactive fixed effects parameters to be estimated as a means of controlling for unobserved heterogeneity. In our context, 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

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[16] 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 2020). See Appendix B for a summary of all observed covariates included in the model specifications.

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).

Bai (2009) shows that when T is large and of comparable size to N, as is the case in the present study, least squares estimation of model (1) is robust to serial correlation and heteroskedasticities of an unknown form in the idiosyncratic errors.[17] As in Bai (2009), we make no assumption about whether F_t and $\lambda'_{i,k}$ have a zero mean or whether they are independent over time. In some cases F_t may affect CO_2 emissions only, but in other cases may be correlated 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 the common factors, F, are unobserved and their true number, r, is unknown when 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.[18] To capture the (potential) multi-dimensionality of the factor structure without overfitting, we use an algorithm to select the optimal number of factors (up to a maximum of five) for each model iteration using the 'leave-one-out' cross-validation procedure described in Xu (2017).

Model (1) can accommodate the theoretical schema described in §II, 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, $\mathbb{E}[\varepsilon_{i,k,t}|D_{i,k,t}, x_{i,j,t}, f_t, \lambda_{i,k}] = \mathbb{E}[\varepsilon_{i,k,t}|x_{i,k,t}, f_t, \lambda_{i,k}] = 0$.

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

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

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

ASSUMPTION 2. Transitory shocks in $\epsilon_{i,k,t}$ are cross-sectionally independent, such that any common factors and unobservable heterogeneities that have a substantive bearing on emissions in model (1) are captured or closely approximately 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 the (potential) presence of common factors and time-varying unobservable heterogeneity that may be correlated with the observed covariates (Bai 2019). 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 the effects of divorce law reforms on divorce rates (Kim and Oka 2014), among others. Gobillon and Magnac (2016) provide Monte Carlo evidence showing that in the presence of common error components, the conventional difference-in-differences estimator is generically biased, the synthetic control method performs relatively well under specific conditions, and the IFE estimator produces the least bias in most cases.

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 (as well as their squares to allow for non-linear 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 the introduction of the carbon price as well as the price level itself. There is little evidence of existing carbon prices having 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; 2020*b*) 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 carbon price levels considered herein. While this assumption is plausible for the period under consideration, 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 (§V.4).

> 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 $\lambda'_{i,k} f_t$.

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 generalised synthetic control (GSC) 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 interactive fixed effects model is estimated using only control group data. Having obtained a fixed number of latent factors, factor loadings are then estimated for each treated country by linearly projecting their pre-treatment 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 pre-treatment outcomes in the treated country as the benchmark. The estimated

counterfactuals for treated countries are estimated using cross-sectional correlations between treated and control group countries.[19]

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 pre-treatment period, we can have greater confidence that the posttreatment counterfactual can serve as a credible baseline against which to assess the effect of the carbon-pricing intervention. Since tests of 'no treatment effect' based on synthetic controls can be extremely oversized if non-stationarity is ignored (Carvalho *et al.* 2016), we focus on specifications in first differences. Unit root tests confirm that observed CO_2 emission levels for our panel of countries/sectors are $I(1)$ non-stationary but become stationary in first differences (see Appendix C).

Estimating our baseline model (1) using the interactive fixed effects estimator (Bai 2009) in a generalised 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 time period as:

$$\widehat{ATT_{t,k}} = \frac{1}{n_{Tr}} \sum_{i \in Tr} \hat{\delta}_{i,k,t} \tag{2}$$

with the overall average treatment effect for each sector given by the weighted average of $\widehat{ATT_{t,k}}$ over all treated time periods. We conduct inference on $ATT_{t,k}$ and $\hat{\delta}_{i,k,t}$ using the non-parametric bootstrap.[20] The 'base' specification reported in the main text includes both additive (individual and time) fixed effects as well as allows for interactive fixed effects, and restricts the treated countries to those with pre-treatment data spanning a minimum of 15 years, requires countries in the control group to have average population, real GDP, and emissions levels that are at least as high as the lowest average in the treatment group, and imposes no restrictions on the

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[19] 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).

[20] All models are estimated using the 'gsynth' package in R for a range of specifications to assess the robustness of the results (see §V.4 for robustness checks).

minimum of the number of common factors, or the maximum/minimum number of treated years. We investigate a wide range of specifications in our robustness checks (§V.4).

V.1.1 Results: The Average Effect of Introducing a Carbon Price
(Average Treatment Effect)

Estimation results using the generalised synthetic control model show that the introduction of carbon pricing has resulted in a significant decrease in the growth rate of CO_2 emissions (Table 3 and Figure 2) relative to the estimated counterfactual. The average treatment effect over treated countries and time periods suggests that growth in total CO_2 emissions is roughly 1.5% (se=0.7%) lower compared to the estimated counterfactual. Results at the sector level indicate that growth in CO_2 emissions is 2.5% (se=1.2%) lower for electricity and heat, 0.8% (se=1.5%) lower for manufacturing, 1.7% (se=0.9%) lower for road transport, and 1.2% (se=2.2%) lower for buildings. These results are robust across a wide range of model specifications (see §V.4 for robustness checks).

Table 3: 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	-0.014	-0.027	-0.008	-0.017	-0.012
	(0.007)	(0.012)	(0.015)	(0.008)	(0.019)
	[p=0.04]	[p=0.01]	[p=0.59]	[p=0.01]	[p=0.43]
Δlog(GDP)	0.6824 (0.9842)	-0.4711	-0.3985	-0.172	1.2378
		(1.0416)	(1.2797)	(0.9788)	(1.2899)
Δlog(GDP)^2	-0.0086	0.0395	0.0286	0.0206	-0.0366
	(0.0374)	(0.0428)	(0.0573)	(0.0425)	(0.0515)
Δlog(population)	0.3874	0.2243	-0.0261	0.511	0.9564
	(0.1624)	(0.2555)	(0.4233)	(0.2063)	(0.767)
Δlog(servicesGDP)	-	-	-	-	-0.6551
					(0.824)
Δlog(servicesGDP)^2	-	-	-	-	0.0444
					(0.0403)
Δlog(manfacturingGDP)	-	-	1.2974	-	-
			(0.6056)		
Δlog(manfacturingGDP)^2	-	-	-0.0461	-	-
			(0.0323)		
Δlog(transportGDP)	-	-	-	0.6356	-
				(0.5067)	
Δlog(transportGDP)^2	-	-	-	-0.0258	-
				(0.0282)	
Δlog(heatingdegreedays)	-	-	-	-	-
Δlog(coolingdegreedays)	-	-	-	-	-
r	0	1	0	0	2
N_{TR}	21	21	20	7	7
N_{CO}	29	27	31	43	27
Specification #	1	1	1	1	1

Note: Bootstrap standard errors shown are shown in parentheses, with the bootstrap *p*-value for the ATT reported in square brackets. Results including heating and cooling degree days are shown in §V.4. Specifications are described in Table 6.
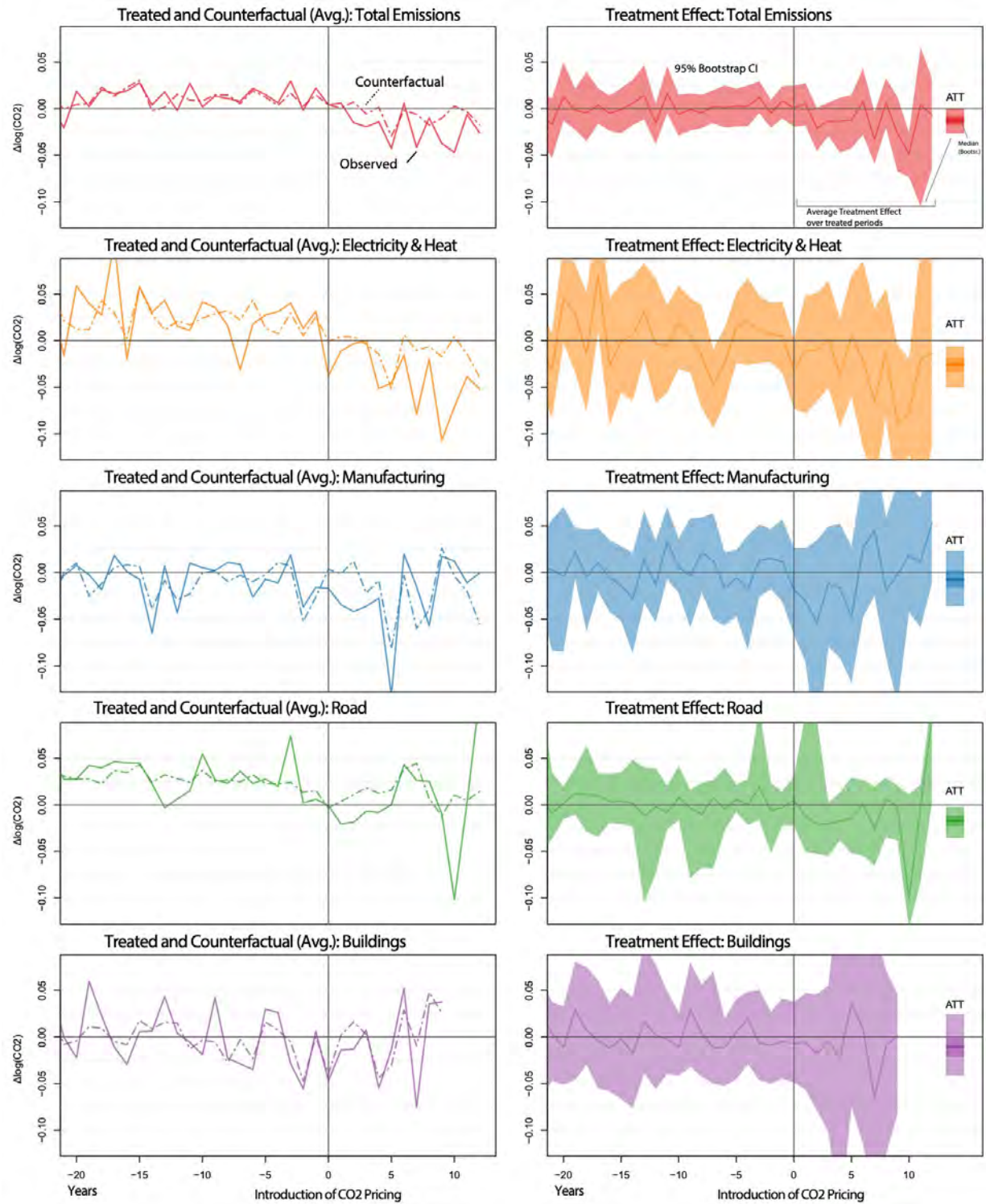
Figure 2.

Generalised 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, together with the estimate of the Average Treatment Effects (ATT) and its 95% bootstrap confidence interval (shaded).
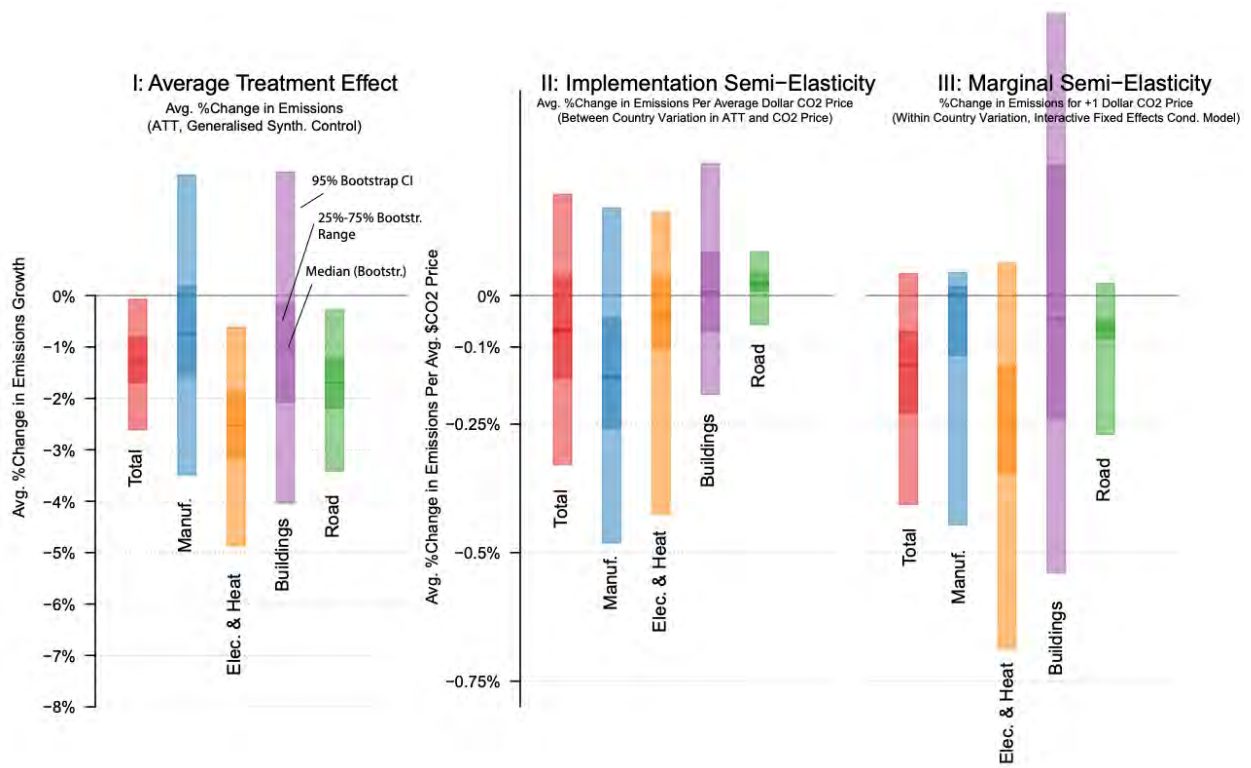
Figure 3.

Average treatment effects, implementation semi-elasticity, and
marginal semi-elasticity

V.2 The Effect of the Price Level (Implementation Semi-Elasticity)

The estimated average treatment effects show that the introduction of carbon pricing has resulted in a decrease in the growth of CO_2 emissions. However, it is not clear whether higher price levels at the time of introduction result in larger emission reductions. Merely the act of introducing any non-zero carbon price might drive the apparent reductions by altering expectations (see e.g. Fried *et al.* 2020). 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. Estimating simple panel model regressions of emissions growth on the carbon price level while including the pre-treatment sample risks confounding the effect associated with introducing the carbon price from the effect which is specific to the price level itself. We therefore propose a new approach to estimate elasticities using synthetic control methods.

Few existing studies have attempted to estimate elasticities from treatment effects obtained via synthetic controls. Dube and Zipperer (2015) is a notable exception. The authors estimate elasticities using multiple synthetic control estimates - one for each treated unit[21] - to assess whether changes in unemployment can be attributed to the magnitude of changes in minimum wages. Their application focuses solely on already implemented minimum wage policies, thus avoiding the challenge of separating introduction and price effects.

Our proposed approach is to model variation in the country-specific treatment effects using observed variation in the carbon price within and between countries over time. Specifically, we assess whether heterogeneity over i (and t) in the treatment effect $\delta_{i,k,t}$ can be attributed to variation in the carbon price levels and their trajectories over time. We model the treatment effect as

$$\delta_{i,k,t} = f(a_{i,k}, b_k, p_{i,k,t}) \tag{3}$$

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

where $a_{i,k}$ denotes the (potentially heterogeneous over i) effect of introducing *any* carbon price in sector k. For example, this 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.

A concern is that the generalised synthetic control approach does not differentiate between the introduction effect, $a_{i,k}$, and the price effect, b_k. Using only the treatment effect, $\hat{\delta}_{i,k}$, we cannot differentiate between the emission reductions stemming from the introduction of *any* carbon price, or because of a particularly high carbon price. We estimate this elasticity using both between country and within country variation.

V.2.1 Implementation Elasticity Using Between-Country Variation

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

$$\bar{\hat{\delta}}_{i,k} = a_k + b_k \bar{p}_{i,k} \tag{4}$$

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

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

with b_k denoting the parameter of interest - the change in the average sector-level treatment effect (change in the growth rate of CO_2 emissions) in response to a one dollar increase in the average emission-weighted carbon price. This implicitly assumes that the introduction effect $a_{i,k}$ is identical for all countries. We relax this assumption

when considering the within-estimator of the implementation elasticity. As there is variation in the treatment length (the number of years carbon prices have been implemented), countries with shorter treatment might exhibit higher variance in the treatment effect. To account for this potential heteroskedasticity we estimate (4) using a weighted estimator:

$$\bar{\bar{\delta}}^*_{i,k} = a_k x^*_{0,i,k} + b_k \bar{p}^*_{i,k}, \tag{5}$$

where the weighted variables are given by:

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

with $l_{i,k}$ denoting the treatment length for treated unit i and sector k. To alleviate concerns about single outlying countries distorting the estimates, we estimate (5) using an outlier-robust MM estimator (Koller and Stahel 2011).[22] To conduct inference on b_k we bootstrap (5) 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 generalised synthetic control estimator from §V.1. For example, in a sample of 22 treated countries (n_{treat}=22) we sample 22 treatment effects (one for each country) 1,000 times from the original bootstrap draws and estimate the above robust weighted regression with 22 observations 1,000 times to approximate the distribution of b_k.

V.2.2   Implementation Elasticity Using Within-Country Variation

Using between country variation to estimate the semi-elasticity of CO_2 emissions with respect to the carbon price does not control for country-specific characteristics that might lead to heterogeneous treatment effects. In particular, the above between model assumes that the pure introduction effect captured by $a_{i,k}$ is the same for all countries

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[22] Implemented using the R package 'lmrobust'.

i. We therefore also estimate the effect of the carbon price on the estimated treatment effect using within-country variation of the carbon price 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 (6):

$$\hat{\delta}_{i,k,t} = a_{i,k} + b_k p_{i,k,t} \tag{6}$$

where $a_{i,k}$ are country specific fixed effects (allowing for heterogeneous introduction effects of carbon pricing). We also estimate (6) including the first lag of the carbon price to test whether any price effect works through first differences. 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 (6) by estimating the panel model 1,000 times using each bootstrap draw of the treatment effect $\hat{\delta}_{i,k,t}$ from the generalised synthetic control estimator in §V.1.

V.2.3 Results: The Effect of the Price Level
(Implementation Semi-Elasticity)

The point estimate of the implementation semi-elasticity is negative for most sectors, but imprecisely estimated. Table 4 shows the between-country and within-country estimates of the implementation semi-elasticity, with Figure 4 plotting the country-level average treatment effects against average carbon price levels used to derive the between-country estimates of the implementation semi-elasticity. The results suggest a 0.07% reduction in the growth rate of total CO_2 emissions for a \$1/tCO_2 increase in the average carbon price, however the 95% bootstrap confidence interval includes zero, ranging from -0.4% to +0.2% per dollar. Model results assessing level vs. growth rate effects using lagged prices in the within-country model are reported in Appendix D, supporting primarily an effect of the level of the price instead of the change in the price.

The null hypotheses that the carbon price coefficient and fixed effects are homogeneous over countries and therefore poolable are each rejected only in the case of the model of manufacturing CO_2 emissions, suggesting that there is unobserved heterogeneity that may be confounding estimates for this sector. Furthermore, the model of manufacturing CO_2 emissions is the only one with considerably large estimates for the implementation semi-elasticity, suggesting that there may be a small number of countries driving the results. We explore this possibility further in robustness checks in §V.4 by estimating the model of manufacturing emissions in equilibrium correction form, the results of which are presented in Appendix E.

Table 4. Implementation Semi-Elasticity

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

	Total	Electricity and heat	Manufacturing	Buildings	Road transport
Semi-elasticity (between-country)	-0.069% (-0.329%, 0.197%)	-0.041% (-0.426%, 0.163%)	-0.159% (-0.481%, 0.171%)	0.006% (-0.192%, 0.257%)	0.023% (-0.057%, 0.087%)
Semi-elasticity (within-country)	0.011% (-0.162%, 0.194%)	0.059% (-0.119%, 0.187%)	-0.153% (-0.424%, 0.058%)	-0.044% (-0.222%, 0.121%)	-0.023% (-0.084%, 0.045%)
N_{Tr}	21	21	20	7	7
F test for poolability of carbon price coefficient	p=0.577	p=0.982	p=0.004	p=0.898	p=0.679
F test for poolability of fixed effects	p=0.032	p=0.72	p=0.002	p=0.634	p=0.363
Specification #	1	1	1	1	1

Note: 95% bootstrap confidence interval shown in parentheses.
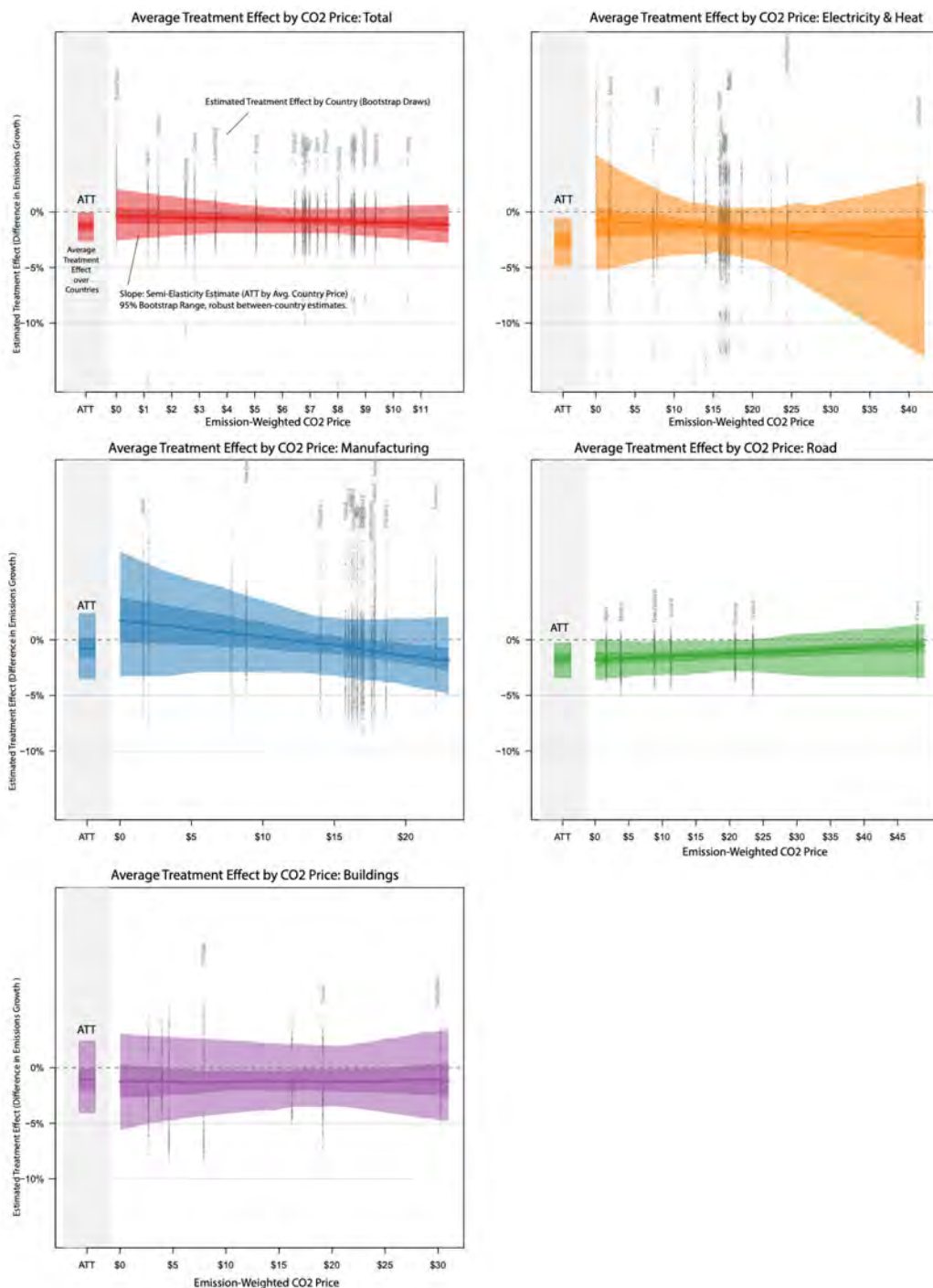
Figure 4.

Average overall treatment effects and between-country variation in treatment effects across average 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. The slope of the regression line denotes the estimate of the implementation (semi-)elasticity. Average treatment effects (across treated units) obtained from the generalised synthetic control analysis are shown as bars with the 95% bootstrap confidence interval (shaded).

V.3 The Effect of Price Changes Conditional on Having a Carbon Pricing Scheme (Marginal Semi-Elasticity)

To assess the impact of price changes conditional on having already implemented a carbon price, we estimate marginal elasticities by exploiting within-country variation in carbon prices post-introduction. We restrict the sample to countries and years with non-zero carbon pricing and estimate the interactive fixed effects model

$$\Delta \log(CO2)_{i,k,t} = \beta_k \Delta p_{i,k,t} + x'_{i,k,t}\beta + \lambda'_{i,k}F_{k,t} + \epsilon_{i,k,t} \tag{7}$$

where $\Delta p_{i,k,t}$ refers to the year-on-year change in the carbon price for each country i. The number r of factors F_k is chosen using the cross-validation results from the generalised synthetic control analysis in §V.1. To assess whether any price effects may enter the model in levels or first differences, we also estimate a more general model,

$$\Delta\log(CO2)_{i,k,t} = \beta_{0,k}p_{i,k,t} + \beta_{1,k}p_{i,k,t-1} + x'_{i,k,t}\beta + \lambda'_{i,k}F_{k,t} + \epsilon_{i,k,t}, \tag{8}$$

and compare the signs on the contemporaneous and lagged price. For most sectors the coefficients on the contemporaneous and lagged price level have opposite signs, $\beta_{0,k} < 0$ and $\beta_{1,k} > 1$, supporting an analysis in first differences as in (7).

The above models assume that changes in the carbon price are strictly exogenous. Conditional on having implemented a carbon price, we argue this assumption is reasonable, since many carbon pricing schemes have committed changes in advance, and changes to prices are unlikely to be driven by contemporaneous growth in CO₂ emissions.[23] Several additional considerations support our assumption of strict exogeneity. First, economists have explicitly recognized the long lag times between (uncertain) CO₂ emissions outcomes and politically initiated adjustments to the carbon tax rate (or the emissions cap in the case of carbon markets). In response, Hafstead *et al.* (2017), Metcalf (2020) and other recent studies propose methods of redesigning carbon pricing schemes so that they include built-in price adjustment

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

mechanisms, thereby providing assurance that carbon price levels can be preemptively adjusted in accordance with specific emission reduction targets under conditions of mitigation uncertainty. To the best of our knowledge, autonomous CO_2 price-adjustment mechanisms of this kind have yet to be adopted in any jurisdiction thus far.[24] Furthermore, we have not been able to identify a single case where policymakers have manually adjusted the carbon tax rate (or emissions cap) as a *contemporaneous* response to unanticipated changes in emissions.[25] With respect to emissions trading systems, the issue of simultaneity is more complex, as economic theory would suggest *a priori* that the CO_2 permit price should respond to 'overachievement' or 'underachievement' of emissions abatement with respect to the cap set by regulators. However, a compelling body of empirical evidence has shown that occasional bouts of volatility and non-stationarity in CO_2 permit prices in the EU ETS since 2005 have predominantly been a function of exogenous events – owing to the altered expectations of market participants induced by unanticipated regulatory changes and policy design announcements regarding the allocation and banking of allowances – whereas the CO_2 permit price is poorly predicted by market fundamentals, negative demand shocks, or lagged emissions levels (Koch *et al.* 2014, 2016; Friedrich *et al.* 2019). Importantly, these regulatory events[26] themselves are best understood as the product of often intense and protracted negotiations with emissions-intensive and trade-exposed industries – often resulting in substantial overcompensation (Grubb 2014; Martin *et al.* 2014*b*) – rather than contemporaneous responses to 'over-achievement' or 'under-achievement' of emissions reductions under the cap. For extended periods, the EU

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[24] We note that the 'Market Stability Reserve' in the EU ETS comes close to an autonomous price adjustment mechanism, but this is scheduled for implementation from 2023 onward and does not affect the time period considered in this study.

[25] 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, since the year of the tax repeal simply marks the end of the treatment period for Australia.

[26] 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 modelled in Bocklet *et al.* (2019).

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 with respect to the time period considered in our study.[27]

V.3.1 Results: The Effect of Price Changes Conditional on Having a Carbon Pricing Scheme (Marginal Semi-Elasticity)

The point estimate of the marginal semi-elasticity of emissions with respect to the carbon price is negative for most sectors (Table 5). The results are robust across specifications (see §V.4), though similarly to the implementation elasticity, the estimates of the marginal elasticity are imprecise. The point estimates suggest that a $1/tCO_2 increase in the carbon price – conditional on having already implemented a pricing scheme – results in a 0.16% reduction in the growth of aggregate CO_2 emissions. At the sector level, a $1/tCO_2 increase in the carbon price results in a 0.26% reduction in the growth of electricity and heat emissions, while estimates for manufacturing, road transport, and buildings are difficult to ascertain due to large standard errors (see Table 5).

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[27] Our argument here relates to a key point made 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".

Table 5. Marginal Semi-Elasticity

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

	Total	Electricity and heat	Manufacturing	Road transport	Buildings
Marginal	-0.16%	-0.264%	0.003%	-0.065%	-0.185%
semi-elasticity	(0.119)	(0.182)	(0.129)	(0.065)	(0.346)
N_{TR}	34	33	36	11	12
N_{sample}	392	327	396	109	139
r	0	1	0	0	2
Specification	1	1	1	1	1

Note: Bootstrap standard errors shown in parentheses. The number of factors selected via cross-validation is denoted by r.

V.4 Robustness of the Results

Figure 5 shows estimates of the average treatment effect, the implementation semi-elasticity, and marginal elasticity for each of the five sectors across 24 model specifications summarized in Table 6. We vary the minimum number of pre-treatment and post-treatment observations, the criteria for control variables used to select the units in the donor pool (average level of emissions and all observed control variables to be at least as large as the minimum, or 25th percentile, for treated units), the minimum and maximum number of common factors r, as well as the forced additive fixed effect specifications in the interactive fixed effects (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, as well as including heating and cooling degree days (HDD, 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 (MC) estimator developed in Athey *et al.* (2018).[28]

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[28] Athey *et al.* (2018) show that the generalised 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. But whereas the synthetic control approach focuses on minimizing the sum of squared errors given a fixed number of latent factors, their proposed MC estimator implicitly 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 IFE approach to selecting the rank of common factors (Xu 2017). Moreover, both estimators accommodate staggered policy adoption across multiple treated units.

Table 6. Model specifications for robustness analysis

Spec. ID #	Min. treated years	Min. pre-treatment years	Donor pool quantiles	Start year	Max. r	Min. r	Fixed effects	Estimator	Observed control variables
1 (base)	0	15	0	1980	5	0	Two-way	IFE	Socio-economic
2	5	15	0	1980	5	0	Two-way	IFE	Socio-economic
3	5	20	0	1980	5	0	Two-way	IFE	Socio-economic
4	0	15	no min.	1980	5	0	Two-way	IFE	Socio-economic
5	0	15	0.25	1980	5	0	Two-way	IFE	Socio-economic
6	0	15	no min.	1980	5	0	Unit	IFE	Socio-economic
7	0	15	0.25	1980	5	0	Unit	IFE	Socio-economic
8	0	15	0	1975	5	0	Two-way	IFE	Socio-economic
9	0	15	0	1980	5	0	Unit	IFE	Socio-economic
10	0	15	0	1980	5	0	None	IFE	Socio-economic
11	0	15	0	1980	2	0	Two-way	IFE	Socio-economic
12	0	15	0	1980	5	1	Two-way	IFE	Socio-economic
13	2	15	0	1980	5	0	Two-way	IFE	Socio-economic
14	0	15	0	1980	5	0	Unit	IFE	Population only
15	0	15	0	1980	5	0	Unit	IFE	Socio-economic, weather
16	0	15	0	1980	5	0	Two-way	IFE	Population only
17	0	15	0	1980	5	0	Two-way	IFE	Socio-economic, weather
18	0	15	0	1980	5	0	Two-way	MC	Socio-economic
19	0	15	no min.	1980	5	0	Two-way	MC	Socio-economic
20	0	15	0.25	1980	5	0	Two-way	MC	Socio-economic
21	0	15	no min.	1980	5	0	Unit	MC	Socio-economic
22	0	15	0	1980	5	1	Two-way	MC	Socio-economic
23	0	15	0	1980	5	0	Unit	MC	Population only
24	0	15	0	1980	5	0	Unit	MC	Socio-economic, weather

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. 'Max. r' and 'Min. r' refer to restrictions on the number of common factors selected via cross-validation. 'Socio-economic' refers to inclusion of GDP, sector-level GDP, and population control variables; 'weather' refers to inclusion of population-weighted heating degree days and cooling degree days.

Estimates of the average treatment effects, the implementation elasticity, and the marginal elasticity are robust across specifications. With respect to total (economy-wide) emissions, the average treatment effect is centred around a -1.5% change in the growth rate of aggregate CO_2 emissions; the implementation elasticity is around -0.07% per average emissions-weighted dollar of CO_2 pricing; and the marginal elasticity is around -0.15% change in emission growth for an additional dollar of CO_2 pricing conditional on having an implemented pricing scheme. Taken in turn [ATT, implementation, marginal], our sector level estimates centre around -1.2%, -0.2%, and -0.01% for manufacturing emissions; -3.5%, -0.04%, and -0.2% for electricity and heat emissions; -1%, 0.01%, and -0.15% for buildings emissions; and -2%, 0%, and -0.75% for road transport emissions. Note that many of the control specifications in Figure 5 (such as number of pre-treatment periods) only apply to the model specification used to estimate the ATT and implementation elasticity, thus yielding identical estimates for the marginal semi-elasticity.

Several aspects of the robustness analysis presented in Figure 5 are noteworthy. First, our estimates are robust to the exclusion of GDP as a control variable, which greatly diminishes the concern – discussed in §V.1 – that the carbon price might affect emissions vis-à-vis its potential impact on economic output. Second, inclusion of weather-related control variables – heating degree days and cooling degree days – yields a significant increase in the ATT and marginal semi-elasticity point estimates for the buildings sector, with a considerable narrowing of the 95% bootstrap confidence intervals; this finding is consistent with the well-established empirical literature demonstrating the significant impact of weather variation on energy demand (Mistry 2019), and it also indicates that our preferred specifications for the buildings sector should be numbers 15, 17, and 24. Third, our estimates are robust to the choice of estimator: interactive fixed effects or matrix completion. Fourth, while marginal and implementation 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 equilibrium correction (EC) models that include any treated country $i_{TR} \in 1, 2, \dots N_{TR}$, that has had a sufficiently

long treated period $t_{TR} \in t_1, \ldots \geq t_{23}$ with respect to carbon pricing in sector k. A summary of these specifications and results are provided in Appendix E. Estimating these additional 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 §V.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, since the relatively large implementation semi-elasticities estimated in the manufacturing sector may be driven by a small number of countries, we can use the equilibrium correction specification to check if any of the countries with a relatively long treatment period of CO_2 pricing in manufacturing are driving this result (namely, Finland, Sweden, and Poland). This intuition is confirmed in Appendix E, where it is found that Finland accounts for the large semi-elasticity of manufacturing emissions. We reject the null hypothesis of 'no cointegration' for the models of total emissions and manufacturing emissions, but cannot reject the null for other sectors. As shown in Table E.1 of Appendix E, the average long run effects of an additional $1/tCO_2$ range from a 0.2% to 0.6% reduction in the growth rate of total CO_2 emissions and manufacturing emissions, respectively.
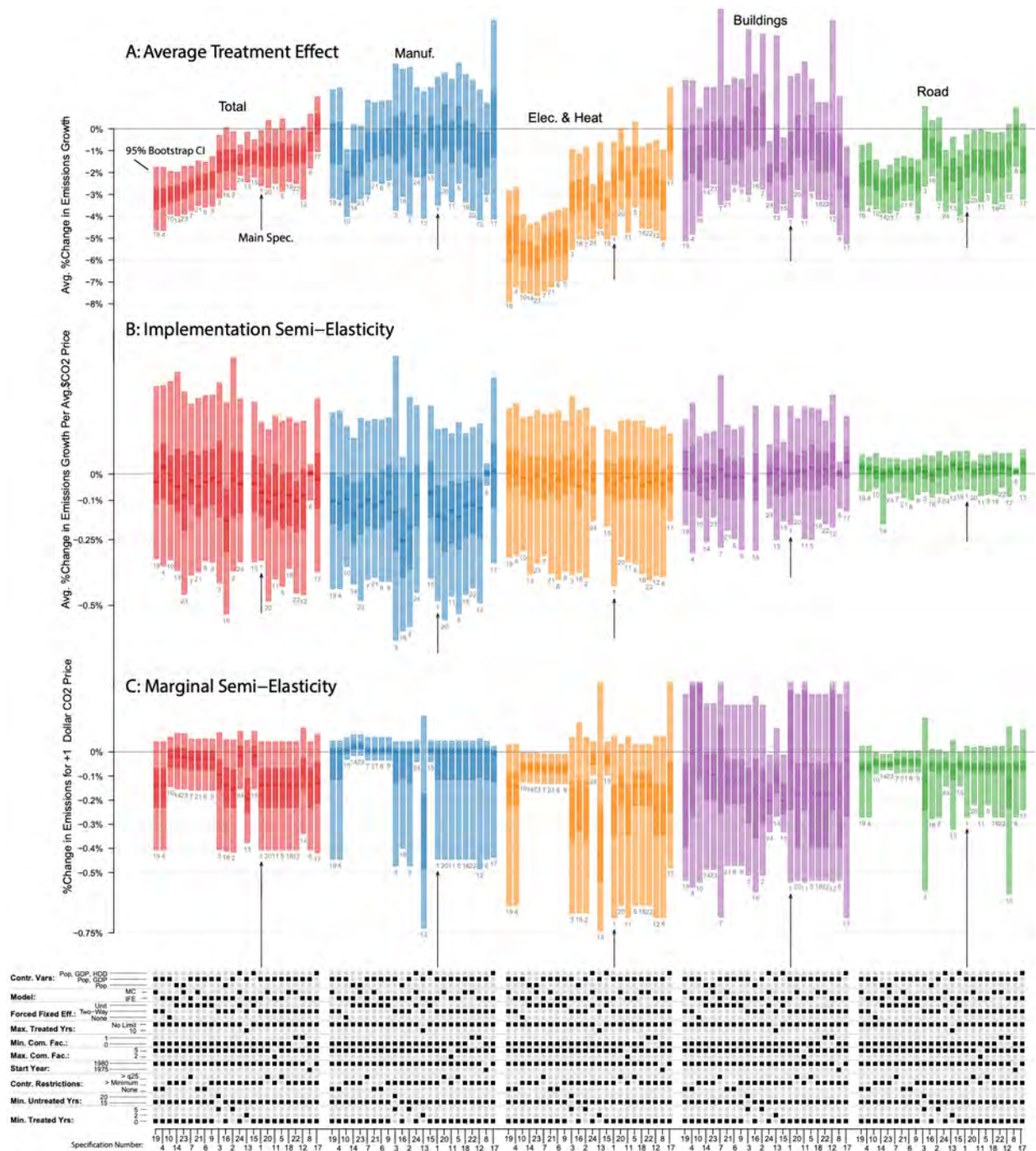
Figure 5.

Average treatment effects, implementation semi-elasticities (using between-country variation), and marginal semi-elasticities across 24 model specifications.

VI. SIMULATING THE EMISSIONS IMPACTS OF FUTURE PRICE PATHS

What changes in future emissions can we expect in response to a specific carbon pricing scheme? Policymakers have long sought the answer to this question, which is particularly pressing due to the international commitments under the 2015 Paris Agreement. The agreement signed by 195 nations requires global emission reductions of approximately 50% percent relative to 2020 by 2030 to maintain global average surface temperatures below 1.5C relative to pre-industrial conditions (UNEP 2019).

Carbon prices have been hailed by many economists as the tool of choice to implement such emission reductions at the '*scale and speed that is necessary*' (Economists' Statement on Carbon Dividends 2019).[29] However, these claims were made with little empirical evidence to support them. Using our estimates of the implementation and marginal semi-elasticities we simulate the impact of carbon pricing on projected future emissions to assess whether pricing is likely to be sufficient to achieve emission reductions at the required scale and speed. We compare emissions under carbon pricing to no-pricing scenarios using projected future CO_2 emissions from the Shared Socioeconomic Pathways (SSPs), which serves as a set of reference scenarios from 2005 to 2050 (Riahi *et al.* 2017).[30] We consider a hypothetical global carbon price introduced in 2021. We simulate projected total (*tot*) emissions as

$$\log(\widehat{CO2})_{tot,t} = \log(\widehat{CO2})_{tot,t-1} + \Delta\log(\widehat{CO2})_{tot,t}, \tag{9}$$
$$\text{for } t = 2006, \dots ,2100,$$

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:

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[29] Emphasis in italics is ours. Economists' Statement on Carbon Dividends (2019) includes among its signatories 3,589 US-based economists, 4 former chairs of the Federal Reserve, 27 Nobel Laureate economists, and 15 former chairs of the Council of Economic Advisers.

[30] 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 semi-elasticities.

$$\Delta \log\left(\widehat{CO2}\right)_{tot,t} = \Delta \log(CO2)_{tot,t,Base} + \Delta \log(CO2)_{tot,t,Price} \tag{10}$$

where $\Delta \log(CO2)_{tot,t,Base}$ is the CO_2 emissions growth rate given in the SSP reference scenario. The change in emissions implied by carbon pricing is specified as

$$\Delta \log(CO2)_{tot,t,Price} = \hat{a}_{tot} + \hat{b}_{tot}\bar{p} + \hat{\beta}_{tot}\Delta p_t \tag{11}$$

where $\hat{b}_{tot}$ is the estimated implementation semi-elasticity from §V.2, $\bar{p}$ denotes the average carbon price over the treated period (2021-2050) and $\hat{a}_{tot}$ is the intercept in our model used to estimate the implementation elasticity. Taken together, $\hat{a}_{tot} + \hat{b}_{tot}\bar{p}$ correspond to our model of the average treatment effect. The transitory emission impacts of marginal price changes post-implementation are captured by the marginal semi-elasticity, $\hat{\beta}_{tot}$, from §V.3, where Δp_t is the change in the hypothetical carbon price in year *t*. Both elasticity estimates are taken from the baseline model specification (=1) summarized in Table 6.

Our simulation combines parameter estimates from two sets of models (the implementation semi-elasticity and marginal semi-elasticity). However, two considerations support the conclusion that any biases in estimated effects due to omission of marginal impacts in implementation estimates (and vice versa) are likely to be small. First, the impact of marginal changes is transitory in the model and thus expected to have little impact on implementation estimates. Second, individual fixed effects in the model used to estimate the marginal elasticity can account for constant implementation effects.

We simulate the uncertainty range around projected emissions by sampling over the bootstrap draws of the implementation coefficient, $\hat{b}_{tot}$, intercept, $\hat{a}_{tot}$, and marginal coefficient, $\hat{\beta}_{tot}$. We implicitly assume that these parameters remain constant over the projected period and, therefore, that there is no gradual phase-in of effects or non-linearities. Granted, there is no guarantee that the emissions elasticity will be constant or that the demand function will be smooth and continuous into the future;

as renewable energy sources 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. But although parameter constancy is a strong simplifying assumption, we emphasize that any variation in emissions that results due to time-dependency of policy effects likely falls well within the already wide range of simulated outcomes. In other words, uncertainty about the phasing-in of treatment effects is likely dwarfed by the uncertainty present in the parameter estimates. The simulations below are thus perhaps optimistic in the short run, assuming prices affect emissions immediately without delay.

Figure 6 shows the projected emissions for the SSP2 reference scenario (commonly referred to as the 'middle of the road' scenario) together with hypothetical carbon pricing schemes.[31] The first scheme (blue-dashed) introduces a constant emission-weighted carbon price of $8/tCO_2$ (the median across all currently existing pricing schemes). A second scheme simulates a constant $30/tCO_2$ carbon price (purple solid), mirrored by a third scheme that achieves a carbon price with a $30/tCO_2$ average over the simulated time frame, but that starts at $1/tCO_2$ and is ramped up by $5/tCO_2$ each year until stabilizing at $34/tCO_2$ (purple dashed). Since the average price for both these schemes is identical, this permits a comparison of a constant vs. ramped pricing scheme. Finally, a fourth scheme considers a $110/tCO_2$ constant carbon price (roughly 50% higher than the highest current existing emission-weighted carbon price implemented in Sweden).

Even though both the implementation and marginal elasticities are imprecisely estimated, the median projected difference in emissions suggests a 20% 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 20% reduction in the emissions level relative to the SSP2 baseline corresponds to roughly 'no change' in emissions relative to 2020 (bottom panel in Figure 6). Notably, 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%

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[31] A full description of the SSP2 scenario and its underlying assumptions is available in Fricko *et al.* (2017).

interquartile bootstrap ranges are shown as shaded for the first and second pricing scheme: constant $8 and constant $30).

To achieve median projected emission reductions of 50% by 2030 relative to 2020 consistent with the Paris Agreement using only carbon pricing seems all but impossible. Projected median emission changes in response to a $30/tCO$_2$ price results in a 15% reduction by 2030 (relative to 2020), rising to a 20% reduction at the median if pricing is instead ramped up over time (dashed purple). In the absence of persistence in emissions, to achieve a projected median reduction of 50% by 2030 (relative to 2020) consistent with the Paris agreement, requires a global emission-weighted economy-wide carbon price in excess of $110/tCO$_2$ (green pricing scheme in Figure 6). This seems far outside the realm of political feasibility, particularly as the simulation is optimistic in the assumption of immediate emission reductions in response to the introduction of carbon pricing.
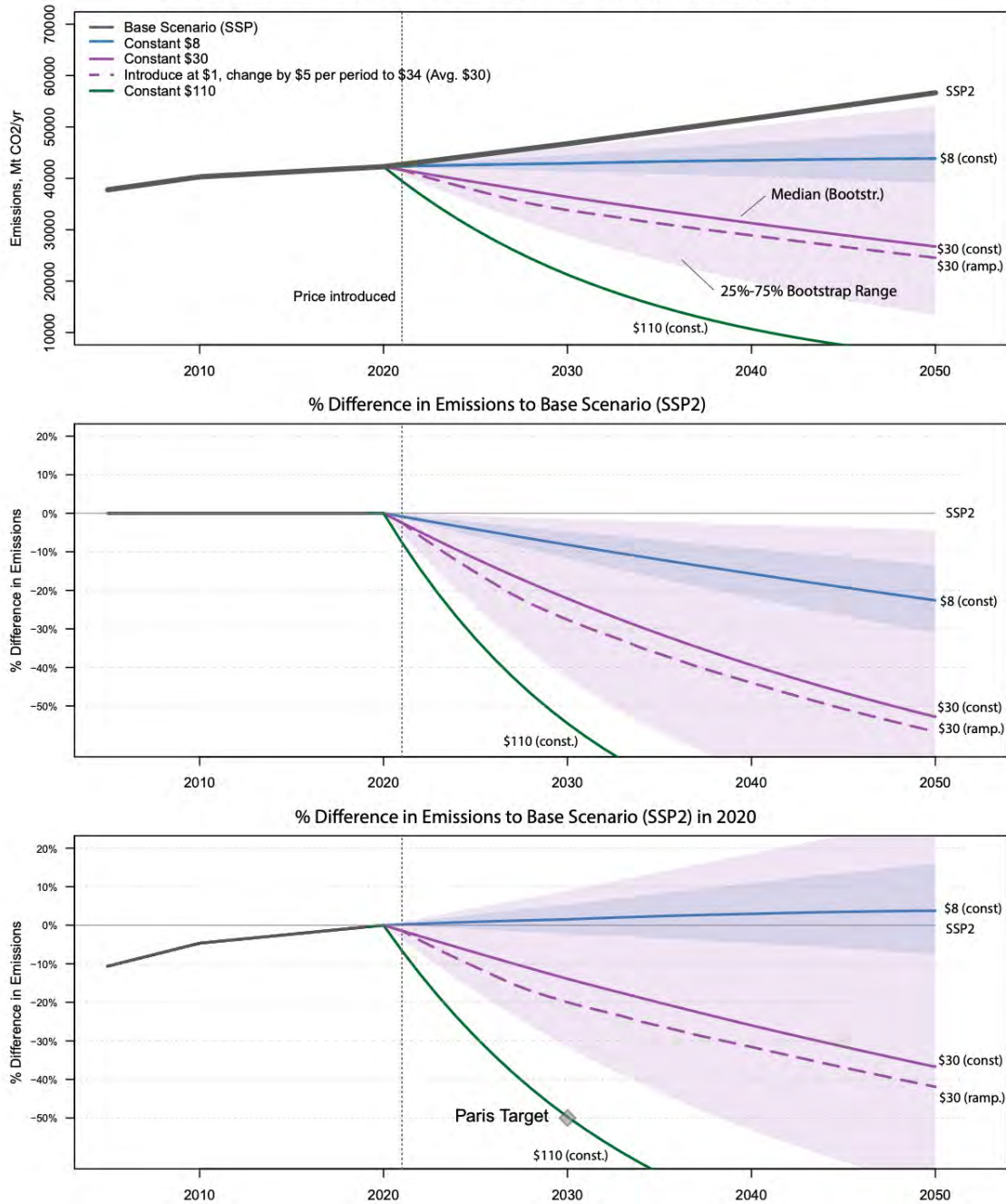
Figure 6.
Global CO₂ Emissions under Carbon Pricing and Reference Scenario using empirical estimates of the emission response to CO₂ pricing
(SSP2, 'Middle of the Road').

Note: Top panel shows the projected emissions, with the reference scenario in black and median projected hypothetical emissions for different pricing schemes: constant $8 (blue); constant $30 (purple); initial $1 and increasing $5 per period until reaching $34 increase, matching a $30 average (purple dashed); and constant $110 (green). The middle panel shows the percentage difference to baseline in each year, and bottom panel shows the percentage difference to the reference scenario in 2020. Shaded bands denote a 25%-75% bootstrap interquartile range for the constant $8 and constant $30 schemes. The Paris target of a -50% reduction relative to 2020 by 2030 is indicated by the grey diamond.

VII. Concluding Remarks

Few questions are as pressing today in the arena of climate change policy as the effectiveness of CO_2 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, the OECD, the IMF, the World Bank, and the UNFCCC. Our retrospective evaluation has contributed 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 implementation effect of carbon pricing conditional on the price level; and (iii) marginal changes in the price per tonne of CO_2 after the pricing instrument has been introduced.

Consistent across a range of model specifications, we find that carbon pricing instruments have reduced the growth rate of CO_2 emissions by 1% to 2.5% on average relative to counterfactual emissions, with most abatement occurring in the electricity and heat sector (where estimates of the ATT reach up to -6% 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 implementation semi-elasticity are centred around a 0.1% reduction in the growth rate of total emissions for each additional $1/tCO_2$, and roughly 0.2% in the manufacturing sector.

After the carbon price has been introduced, each marginal price increase of $1/tCO_2$ has altered the growth rate of CO_2 emissions by -0.01% in the manufacturing sector, -0.2% for electricity and heat generation, -0.15% in buildings, -0.75% in road transport, and -0.15% for the economy as a whole. Based on our simulations of potential future emissions reductions in response to alternative carbon price paths up to 2050, we conclude that CO_2 emissions are unlikely to decline to levels consistent with Paris climate targets in response to plausible levels of carbon pricing in the decades ahead, absent additional non-pricing policies and substantial complementary investments to deploy low-carbon technology and infrastructure.

Our estimates of emissions (semi-)elasticities with respect to carbon pricing indicate that emissions may be substantially more inelastic than suggested by previous empirical studies. 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 centre around –0.18 for electricity and heat, buildings, and the economy as a whole.[32] Furthermore, for the road transport sector, Sterner (2007) reports globally averaged gasoline price elasticities of around -0.7 based on estimates from Europe and the US, while the estimates in Dahl (2012) are closer to about -0.25 on average. By contrast, our (implied) gasoline price elasticity estimates centre around -0.25 (relative to an average carbon price of $8/tCO_2$ in sample). Our implied elasticity estimates here assume that the (CO_2)-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 (see e.g. Andersson (2019)), then the disparity between our elasticity estimates and those of previous empirical evaluations 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. First, estimates of empirical energy demand elasticities based on data from the 1980s and earlier may be underestimating the extent to which energy demand has been shifting towards relatively fast-growing and less price responsive products and regions (see e.g. the evidence for world oil demand in Daragay and Gately (2010)). Second, policy response models of CO_2 emissions (both ex ante and ex post) have tended to poorly capture the inertia of carbon-/infrastructure lock-in, as suggested by analysis in Avner *et al.* (2014) of urban vs. rural responses to carbon pricing under varying densities of available mass public transport. Third, and related to the previous points, our empirical evaluation is the first to explicitly account for cross-country and temporal variation in carbon price exemptions across different

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[32] This is calculated from our average marginal semi-elasticity estimates based on computing the effect of a $1/tCO_2$ price increase relative to an average CO_2 price of $8/tCO_2$ in sample. The same holds for the subsequent estimate made in this paragraph for the price elasticity of gasoline demand.

sectors and industries. This latter point is relevant to the extent that governments may 'offload' higher carbon price levels onto sectors and industries that are either (a) relatively less price responsive but able to bear the policy costs due to relatively less carbon exposure; or (b) highly price responsive but have already undergone critical processes of decarbonization in the years preceding the introduction of carbon pricing (e.g. in response to the oil price shocks of the 1970s, as discussed in Grubb *et al.* (2017)). Taken together, these three points cast doubt on the implicit assumption that the price elasticity of energy demand should elicit coefficient stability across time. Instead, the elasticity of emissions with respect to carbon pricing is likely to be a function not only of the price level, but of 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 the findings of our simulation exercise, although based on empirically grounded and up-to-date elasticity estimates, are limited by an irreducible element of uncertainty.

As a final note, our assessment strongly corroborates several 'best practices' for optimizing carbon pricing reforms that have been identified elsewhere. First, carbon prices are undermined the more they are volatile inter-annually; their effectiveness generally tends to be enhanced when they are on a credible upward trajectory, which has been rare hitherto. Second, while there are (in limited cases) compelling arguments that strategically targeted carbon pricing reforms in with limited sectoral coverage can generate   valid efficiency-based arguments for limiting the sectoral coverage of carbon pricing (King *et al.* 2019), the discrepancy between current coverage levels and those that are likely needed to comply with Paris climate targets remains stark. We therefore recommend the elimination of carbon tax exemptions and free allowance allocations in carbon markets for most emissions-intensive industries. Finally, our analysis corroborates the findings of Grubb (2014), Mercure *et al.* (2014), and others suggesting that climate change mitigation policies, when strategically combined, may be highly synergistic. Carbon pricing still has the potential to be a powerful tool contributing to economy-wide emission reductions, but it is clearly no panacea.

APPENDIX A.

COMPUTING EMISSIONS-WEIGHTED CARBON PRICES

In order to compute the emissions-weighted carbon price (ECP), the following information is required: (1) the coverage of the carbon pricing policy, namely the volume of CO_2 emissions to which the price applies; (2) verified total CO_2 emissions in each jurisdiction; and (3) 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) CO2 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 (IPPU), Including Cement' (category 2). CO_2 emissions from the three source categories accounted for 92 percent of total global CO_2 emissions and 72 percent of total global GHG emissions 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 has been retrieved from various source, including but not limited to: primary legislation; the OECD's Database on Instruments Used for Environmental Policy (OECD 2016); customs agencies' documentation, academic journal articles, and policy assessment reports. A full list of sources and references is available upon request.

Verified data on total CO_2 emissions in each jurisdiction is derived from several different sources depending on the emissions category, as summarized in Table A.1. Furthermore, information about nominal emission prices is gathered from different sources depending on the type of policy instrument and the 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 emissions trading systems, we rely on the sources described in Table A.A.2.

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

$$ECP_{i,t,j} = \frac{\sum_k \left[\tau_{i,t,j,k} \left(q_{i,t,j,k}^{tax} + q_{i,t,j,k}^{ets,tax} \right) + p_{i,t,j,k} \left(q_{i,t,j,k}^{ets} + q_{i,t,j,k}^{ets,tax} \right) \right]}{q_{i,t,j}^{CO2}} \tag{A.E1}$$

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;

$q_{i,t,j}^{CO2}$ 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 fuels) of the sector are covered, the $ECP_{i,t,j}$ would collapse to either $\tau_{i,t,j}$ or $p_{i,t,j}$.

An economy-wide ECP is then computed as a weighted average of the sectoral carbon rates. The weights correspond to the quantities of CO_2 emissions subject to each individual carbon rate, such that

$$ECP_{i,t} = \sum_j ECP_{i,t,j} \gamma_{i,t,j} \tag{A.E2}$$

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. $q_{i,t,j}^{CO2} / q_{i,t}^{CO2}$. To ensure that the computed ECP levels are not biased by interannual changes in CO_2 emissions that may be a consequence of the carbon pricing policy itself, all years are weighted to 2013 emissions data. All prices are expressed in US dollars at constant 2015 prices.

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Table A.1

Data Sources for CO$_2$ Emissions by Emission Source

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Emission source	Data source
1A. Fuel combustion	IEA (2018)
1B. Fugitive emissions	Carbon Dioxide Information Analysis Center (2017)
2. Industrial processes and product use	US Energy Information Administration

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Table A.2

Data Sources for CO$_2$ Prices in Emissions Trading Systems

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Jurisdiction	Price information
EU-ETS	The price of European Union Allowances (EUA) is the annual average of daily EUA futures prices, based on data from Bloomberg.
Switzerland	During the time period covered, no transactions of Swiss emissions allowances (CHU) had taken place over a centralized platform. Transactions had either not taken place or occurred over-the-counter outside of that transaction platform. Hence, no secondary market data is available. Consequently, the price quoted in this study is the volume-weighted average price at auction, based on data from the Swiss Emissions Registry.

APPENDIX B.

DATA SUMMARY

Table B.1

Observed Covariates: Summary Statistics and Data Sources

Covariates	Unit	Source
CO$_2$ emissions: total (economy-wide) electricity and heat, manufacturing, road transport, and buildings (commercial and residential)	Million tonnes of CO$_2$ (MtCO$_2$)	IEA (2018)
Emissions-weighted carbon price: total (economy-wide), electricity and heat, manufacturing, road transport, and buildings (commercial and residential)	US dollars per tonne CO$_2$ (constant 2015 prices)	Updated from Dolphin *et al.* (2020)
GDP: total, manufacturing, transport, and services	US dollars (millions, constant 2015 prices)	UNCTAD (2020), based on United Nations DESA Statistics Division, *National Accounts Main Aggregates Database*
Population size	Absolute value in thousands	UNCTAD (2020), 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 MIS-SPECIFICATION TESTS

Our model specification is informed by diagnostic tests for cross-section dependence, common factors, unit roots, panel cointegration.

First, we strongly reject the null hypothesis of cross-section independence (as well as '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, not only does differencing eliminate serial correlation the errors, it 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. In other words, 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% level using the Hausman-type test in Bai (2009). Moreover, the null hypothesis that the dimensionality of common factors is equal to zero is strongly rejected at the 1% 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 between two and five 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).[33]

To distinguish between common and idiosyncratic components of the residuals (Bai and Ng 2004, 2010), we apply the PANNICA testing procedure described by Reese and Westerlund (2016) with results presented in Table C.2. The procedure combines the strong small sample performance of the tests developed by Pesaran (2006) with flexibility regarding the orders of integration for common and idiosyncratic error components, as in Bai and Ng (2004, 2010). PANNICA corroborates the presence of multiple common factors. When variables are entered in

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[33] All tests are computed using 'phtt' in *R*.

levels, we fail to reject the null hypothesis that there are fewer unit roots than common factors, suggesting the presence of 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 tests developed in Bai and Ng (2010). Hence, all tests consistently suggest that non-stationarity is driven entirely by the common error components, while stationarity is attained in the first-differenced model conditional on the observed regressors.

This naturally leads to tests for cointegration, and we apply those proposed by Westerlund (2007). Bootstrap critical values of these tests are robust in the presence of common factors. Based on these tests, we strongly reject the null hypothesis of no cointegration at the 1% level.

Table C.1

Panel Analysis of Non-stationarity 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 described 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 that there are k unit roots in the common factors; here we report only the test statistics for iterations where the null hypothesis cannot be rejected. For the idiosyncratic (unit-specific) component, we compute the three test statistics developed in Bai and Ng (2010), where $PMSB$ 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 non-stationary, which we strongly reject. All test statistics are computed using 'xtpanicca' in Stata, with thanks to Simon Reese for helpful input.

Table C.3

Tests for Panel Cointegration

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

G_τ	G_α	P_τ	P_α
-6.127	6.067	2.458	2.384
(0.0000)	(1.000)	(0.993)	(0.991)

Note: Bootstrap p-values based on 1000 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.

DIFFERENTIATING BETWEEN LEVEL AND GROWTH EFFECTS

Within-country estimation results of the panel model allowing for level or growth effects.

Table D.1.

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)	(0.005)	(0)	(0.001)
P_{t-1}	0.002	0.001	-0.006	0	0.003
	(0.002)	(0.001)	(0.005)	(0)	(0.002)
N_{sample}	171	162	162	38	21
Specification #	1	1	1	1	1

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Table D.2.

Marginal elasticity estimates from panel model allowing for level or growth effects

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	Total		Electricity and heat		Manufacturing		Road transport		Buildings	
ΔP_t	-0.0016 (0.001)	-	-0.0026 (0.002)	-	0 (0.001)	-	-7e-04 (0.001)	-	-0.0018 (0.003)	-
P_t	-	-0.0018 (0.0013)	-	-0.0028 (0.0024)	-	1e-04 (0.0015)	-	-6e-04 (7e-04)	-	-0.0017 (0.0032)
P_{t-1}	-	0.0015 (0.0013)	-	0.0019 (0.0027)	-	0 (0.0017)	-	7e-04 (8e-04)	-	0.002 (0.0033)
$\Delta\log(\text{GDP})$	-0.2292 (0.98)	-0.2318 (0.9393)	-1.7357 (1.878)	-1.7375 (2.0041)	0.9005 (1.981)	0.87 (2.0934)	-1.6426 (4.697)	-1.6356 (4.6345)	3.6967 (34.762)	4.8632 (38.9238)
$\Delta\log(\text{GDP})^2$	0.0249 (0.04)	0.0252 (0.0388)	0.0805 (0.078)	0.0804 (0.0822)	-0.0121 (0.085)	-0.0106 (0.0901)	0.0686 (0.184)	0.0682 (0.181)	-0.1493 (1.391)	-0.1946 (1.5548)
$\Delta\log(\text{population})$	-1.8347 (1.095)	-1.8022 (1.2152)	-3.0071 (2.648)	-3.2801 (2.5563)	-3.2967 (1.801)	-3.2891 (1.9519)	-2.7794 (2.779)	-2.8632 (3.1754)	-8.0507 (5.092)	-7.2638 (6.0155)
$\Delta\log(\text{servicesGDP})$	-	-	-	-	-	-	-	-	-4.2258 (18.459)	-5.0843 (21.1748)
$\Delta\log(\text{servicesGDP})^2$	-	-	-	-	-	-	-	-	0.2309 (0.916)	0.2722 (1.0464)
$\Delta\log(\text{manufacturingGDP})$	-	-	-	-	0.3878 (0.558)	0.3943 (0.5335)	-	-	-	-
$\Delta\log(\text{manufacturingGDP})^2$	-	-	-	-	-0.0206 (0.027)	-0.0211 (0.0264)	-	-	-	-
$\Delta\log(\text{transportGDP})$	-	-	-	-	-	-	2.0795 (2.342)	2.0822 (2.4152)	-	-
$\Delta\log(\text{transportGDP})^2$	-	-	-	-	-	-	-0.0902 (0.117)	-0.0903 (0.1212)	-	-
$\Delta\log(\text{heatingdegreedays})$	-	-	-	-	-	-	-	-	-	-
$\Delta\log(\text{coolingdegreedays})$	-	-	-	-	-	-	-	-	-	-
N_{treat}	34	34	33	33	36	36	11	11	12	12
N_{sample}	392	392	327	327	396	396	109	109	139	139
r	0	0	1	1	0	0	0	0	2	2
Specification #	1	1	1	1	1	1	1	1	1	1

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:

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

where p is the emissions-weighted carbon price, the bars indicate cross-section averages of the variables, $\omega_{1,i,k}^{CA}, \dots, \omega_{4,i,k}^{CA}$ 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 (Campos *et al.* 2005).[34] We investigate cointegration between the variables by assessing the equilibrium-correction (EC) coefficient $\beta_{0,i,k}$ in (E.E1). Specifically, we compute the unweighted mean-group EC coefficient as $\sum_i (\beta_{0,i,\dots N,k,\dots K})/N$ and obtain the average t-statistic and corresponding p-value based on the critical values in Gengenbach *et al.* (2015). To determine whether the long run average emissions semi-elasticity

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[34] 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% level, and then the selection procedure is repeated until the largest lags of the variables in first differences are significant (if any).

(with respect to the carbon price) is significantly different from zero, we compute the long run average coefficient as

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

where the standard error, $\bar{T}$ statistic, and p-value are computed using the Delta method.[35] 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 as well as the model residuals. Consistent with the findings in Kapetanios *et al.* (2011) and Chudik and Pesaran (2015), we find that adding a sufficient number of lags of cross-section averages, $L^{CA} = T^{\hat{}1/3} - 1$, in model (9) is a powerful means of resolving cross-sectional dependence (see CD tests in Table E.1, which confirm 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% level. The average long run coefficient is significant.

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[35] The equilibrium correction models and mis-specification tests are computed using 'xtcaec' in Stata.

Table E.1. Average long run semi-elasticity

Dependent variable: $\Delta\log(CO2)_{i,k,t}$, in panel mean-group equilibrium correction model.

	Total	Manufacturing	Road transport
Average long run semi-elasticity	-1.57%	-0.6%	-2.55% (1.39)
	(0.4)	(0.2)	[-.0529, 0.0018]
$\beta^{EC}_{0,i.k}$	-1.058	-1.104	-.6107
	(.432)	(.372)	(0.283)
Short run marginal semi-elasticity	-1.06%	-0.32%	-0.76%
	(0.82)	(0.15)	(0.77)
$\bar{p}$	$10.6/tCO_2		
log(oil_price)			-.0176
			(.0293)
Treated countries	2	5	3
Treated observations	50	119	
Total observations			129
Countries used to compute ^{CA}	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≤0.01]	[p≤0.01]	[p≤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 non-parametrically following Pesaran and Smith (1995). 95% confidence intervals for elasticity estimates are in brackets/ $\beta^{EC}_{0,i.k}$ 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.

SUPPLEMENTARY MATERIAL

Data and code for replicating the model results in this study are available upon request and will be accessible online upon final publication of the manuscript.

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