Carbon Pricing and the Elasticity of CO$_2$ Emissions[†]

Ryan Rafaty,[*] Geoffroy Dolphin[**] and Felix Pretis[***]

Working Paper No. 140

October 21, 2020

ABSTRACT

We study the impact of carbon pricing on CO$_2$ 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 CO$_2$ emissions 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 CO$_2$ price. We find that the introduction of carbon pricing has reduced growth in CO$_2$ 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%

[*] Climate Econometrics at Nuffield College, University of Oxford and Institute for New Economic Thinking at the Oxford Martin School. Corresponding author: ryan.rafaty@nuffield.ox.ac.uk.
[**] Judge Business School, University of Cambridge
[***] Department of Economics, University of Victoria, Climate Econometrics at Nuffield College, University of Oxford, and Institute for New Economic Thinking at the Oxford Martin School.

reduction in emissions growth per average \$1/metric ton (hereafter abbreviated as: ton) of CO_2. After the carbon price has been implemented, each marginal price increase of \$1/tCO_2 has temporarily lowered the growth rate of CO_2 emissions 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.

I. INTRODUCTION

Pricing carbon dioxide (CO_2) emissions – either via a carbon tax, emissions trading system, or some 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 ($GtCO_2e$) 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 there is a great uncertainty over its role in climate policy. Critics and endorsers alike concede that 'optimal' pricing schemes which are cost-efficient and environmentally effective in theory may be politically unfeasible in practice (Rosenbloom *et al.* 2020a; Stiglitz 2019). A 'clash of paradigms' persists regarding what this means in practical political terms (Rosenbloom *et al.* 2020b; van den Bergh and Botzen 2020). Under the 2015 Paris Agreement, more than 190 countries committed to preventing dangerous levels of climate change this century by maintaining global average surface temperatures below 1.5-2°C relative to pre-industrial conditions, but this would necessitate a 50% reduction in global emissions in 2030 relative to 2020 (UNEP 2019).[2] In the Economists' Statement on Carbon

---

[1] The 'optimal' carbon price is typically defined in relation to an 'ideal' objective function that sets the carbon tax rate equal to the monetized damages associated with emitting an additional ton of CO_2, referred to as the 'social cost of carbon' (SCC) (Gillingham and Stock 2018). However, global SCC estimates can 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).

[2] This is a necessary but insufficient condition. A further requirement is that global emissions decline to net zero by around 2050-2070. Any irreducible positive emissions would need to be offset by a range of

Dividends (2019), which claims to be the largest public statement in the history of the economics profession, carbon pricing is hailed as the tool of choice to achieve these emissions reductions at the 'scale and speed that is necessary'. 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 relied on ex ante, speculative ('theory-laden') model 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 Section III. Our key takeaway from this burgeoning evaluation literature is that, thus far, the fragmentary nature of the evidence precludes systematic inference on the likely response of emissions to carbon pricing across space and time. As we describe in Section IV, the paucity of cross-country empirical assessments is partly a function of the lack of standardized carbon price data adjusted to account for variation in industry exemptions, rebates, and sectoral coverage. But the empirical neglect can also be attributed to the considerable identification challenges summarized succinctly by Mildenberger (2020):

> Carbon pollution levels are so overdetermined by diverse economic and social forces that retrospective causal identification of policy impacts remains difficult. Economists have offered evaluations of some policies, but these estimates are difficult to compare across countries and time. Nor can we reliably translate simple policy content metrics, like a

---

negative emissions technologies, none of which is a panacea and all of which face considerable biophysical limits, uncertain long-term costs, and political coordination challenges (Griscom *et al.* 2017; Hepburn *et al.* 2019; Chatterjee and Huang 2020; Smith *et al.* 2016).

[3] 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.

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, this paper presents a viable empirical modeling 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 is that policymakers and the public still know little about the environmental effectiveness 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, once a carbon price is set, 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 identifying treatment effects based on 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' – namely, 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 CO_2 emissions growth 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; these results are only statistically significant for the manufacturing sector (-0.16% for each additional \$1/tCO_2). Furthermore, the marginal (semi-)elasticity – the change in CO_2 emissions growth 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 mostly by the electricity and heat sector (-0.26%). Finally, we confirm that these results are robust across a range of model specifications, including additional equilibrium correction specifications that accommodate global stochastic trends affecting CO_2 emissions.

Combining our empirical estimates of the implementation and marginal elasticities with projected future emissions, we arrive at an important result: that carbon pricing

at current observed levels, even if implemented globally, is unlikely 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 near 100% emission coverage and 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 (Section II) and reviewing the prior evidence from the evaluation literature (Section III), we describe the standardized carbon price data used to estimate emissions elasticities (Section IV). We then explain our identification strategy, baseline model specifications, and estimation procedure (Section V). After summarizing the country-level and sector-level results across 24 model specifications (Section VI), we conclude with reflections on the policy implications of our findings (Section 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 for that sector.[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$_2$ 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

---

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 Section III, however, suggests that carbon prices have triggered at least some CO$_2$ abatement.

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 below Section IV). This has important implications for the empirical relationship to be expected between carbon prices and associated changes in CO_2 emission levels. We take this to suggest that, for the time period analyzed herein, the appropriate model specification may be linear. We return to the question of functional form in Appendix C with misspecification tests of our baseline model formulation; ultimately, the tests corroborate our initial conjecture that nonlinear relations are absent or non-detectable in the short sample and insignificant at hitherto observed carbon price levels. 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 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; 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 remain scarce and rarely present 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; 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.

---

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

Table 1.   Empirical evaluations of implemented carbon prices and associated CO$_2$ emission reductions

Study	Jurisdiction(s)	Period	Estimator	Policy instrument	Outcome variable	Change in emissions over entire period	Change in emissions per year
Abrell, *et al.* (2011)	EU	2005-2008	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 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	NA

Jaraite and DiMaria (2016)	Lithuania	2003-2010	DiD	EU ETS	CO_2 emissions (plant level)	Insignificant	Insignificant
Metcalf and Stock (2020b)	EU	1990-2018	Panel OLS with LP method and panel SVAR	Carbon taxes	Growth rate of total CO_2 emissions (country-level)	-4% to -6% 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

Study	Location	Period	Method	Policy	Outcome	Effect	Sensitivity
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)	+$5/t$CO_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 = generalized synthetic control; IFE = interactive fixed effects; ML = machine learning; RDiT = regression-discontinuity-in-time.

The available evidence summarized in Table 1 has been mixed and inconclusive. Nevertheless, we can infer a few basic facts from this literature: (i) no study, it is fair to say, has managed to identify an instrumental variable that can credibly isolate exogeneous 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 estimates whose reliability depends largely on the similarity 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 using standardized carbon prices (based on 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.

---

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

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, practical experience shows that governments are routinely constrained by domestic political economy constraints which inhibit 'optimal' carbon pricing, while the transaction costs associated with implementing and sustaining carbon pricing instruments in some sectors are far from trivial.

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

---

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

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.

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] Climate change need not be the only market failure.

This has introduced a major impediment to economy-wide (let alone cross-country) 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 39 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 apply the same methodology not only to obtain the economy-wide ECP series but also sector level CO_2 prices for (i) electricity and heat;

---

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

[12] In the hypothetical situation where a policymaker wants 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.

(ii) manufacturing; (iii) road transport; and (iv) commercial and residential buildings. The ECP in each sector k of each country i is computed using 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 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 yearly change of prices.

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.

---

[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).

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

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.

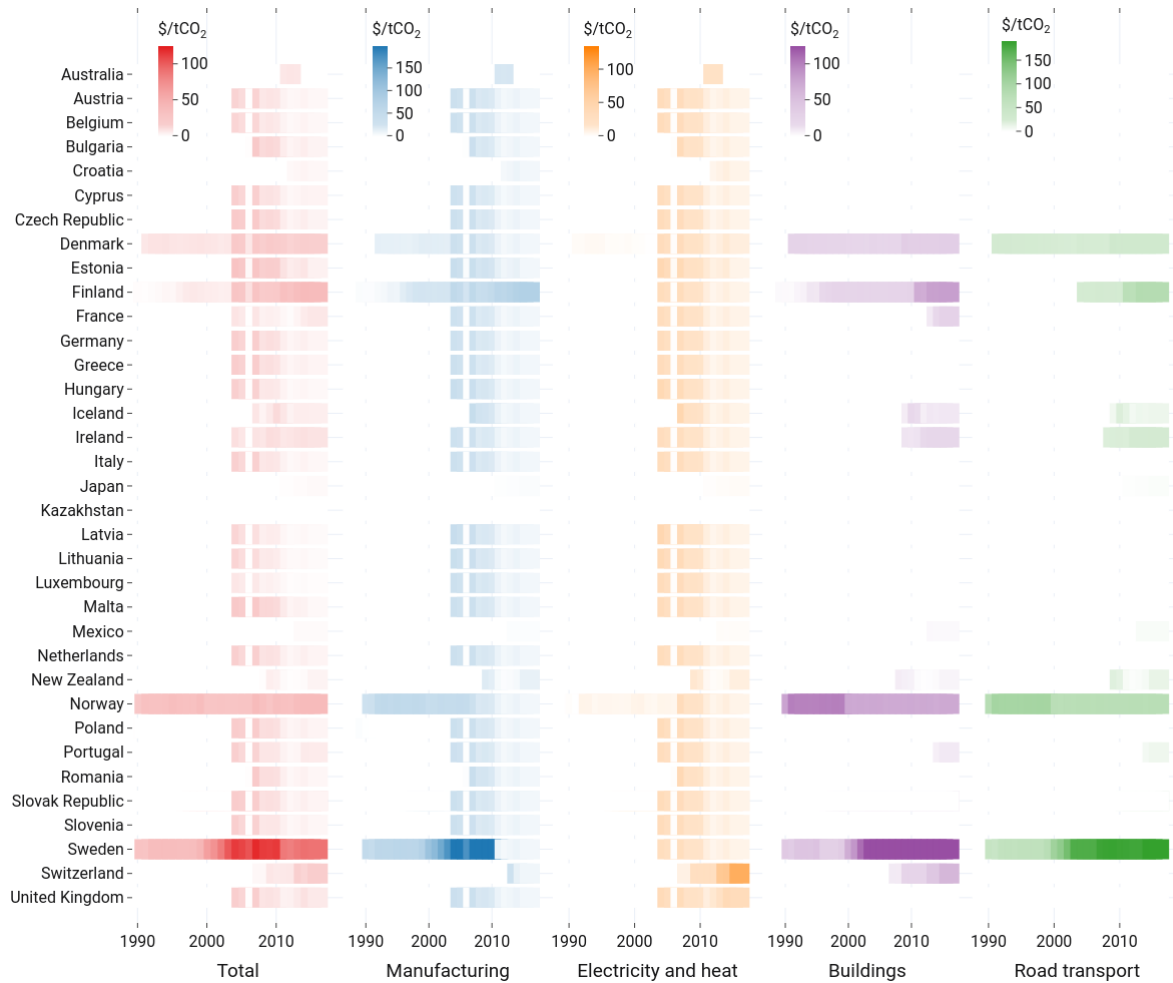**Carbon Price Coverage and Stringency Across Countries and Sectors (1990-2016)**

Note: Color-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$/tCO$_2$). 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 generalized synthetic control methods for policy evaluation under staggered adoption, i.e. in which multiple treated units introduce the policy at varying points in time (Xu 2017; Athey *et al.* 2019) (Section V.1 and Section V.1.1). Using emissions-weighted price data, we then quantify the implementation semi-elasticity of CO_2 emissions with respect to the carbon price level. We propose a new approach to decompose variation in the treatment effect using variation in the treatment intensity provided by different levels of carbon pricing (Section V.2 and Section 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 (Section V.3 and Section 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, which implemented carbon pricing schemes at different points in time and potentially exhibit distinct 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 generalized 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:

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

$$\text{for countries } i \in 1,2,...N_{co}, N_{co}+1,...,N,$$
$$\text{sectors } k \in \ k_{manufacturing}, k_{electricity\_heat}, k_{buildings}, k_{road}, k_{total},$$

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 $x' = [x'_1, \ldots, x'_q]'$, 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 (Section V.4), including population-weighted heating degree days and cooling degree days as additional control variables to capture the impact of weather on 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 $F_t = [F_{1t}, \ldots, F_{rt}]'$ denotes unobserved (latent) common factors that may be correlated with $\Delta\log(CO2)$, D, and x'; $\lambda_{i,k} = [\lambda_{i,k,1}, \ldots, \lambda_{i,k,r}]'$ 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 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

---

[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.

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

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 cross-validation procedure described in Xu (2017).

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

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

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

To the extent that this assumption holds, the IFE estimator effectively obviates endogeneity concerns related to the (potential) presence of unobserved common factors

---

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

and time-varying heterogeneity correlated with the observed covariates (Bai 2009). Under analogous assumptions, the IFE estimator has been used to mitigate cross-section dependence and endogeneity biases in studies estimating the effects of spillovers on private returns to R&D (Eberhardt *et al.* 2013) and 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 while 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; 2020b) reassure us that any inferable impact of a carbon price on GDP is likely to be negligible, at least with respect to the historically observed carbon price levels considered here. 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 Section V.4.

> ASSUMPTION 4. The level of the carbon price at time t is independent of $\Delta \log(CO2)_{i,k,t,\dots,t-L}$ 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 generalized 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 (and thus misleadingly rejected) 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 generalized synthetic control framework (Xu 2017) yields estimates of the sector-, country-, and time-specific treatment effects $\delta_{i,k,t}$. We report the average treatment effect over treated countries for each sector and each 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

---

[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 Section V.4 for robustness checks).

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 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 (Section V.4).

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

Estimation results using the generalized 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 Section 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]
$\Delta\log(\text{GDP})$	0.6824	-0.4711	-0.3985	-0.172	1.2378
	(0.9842)	(1.0416)	(1.2797)	(0.9788)	(1.2899)
$\Delta\log(\text{GDP})^2$	-0.0086	0.0395	0.0286	0.0206	-0.0366
	(0.0374)	(0.0428)	(0.0573)	(0.0425)	(0.0515)
$\Delta\log(\text{population})$	0.3874	0.2243	-0.0261	0.511	0.9564
	(0.1624)	(0.2555)	(0.4233)	(0.2063)	(0.767)
$\Delta\log(\text{servicesGDP})$	-	-	-	-	-0.6551
					(0.824)
$\Delta\log(\text{servicesGDP})^2$	-	-	-	-	0.0444
					(0.0403)
$\Delta\log(\text{manfacturingGDP})$	-	-	1.2974	-	-
			(0.6056)		
$\Delta\log(\text{manfacturingGDP})^2$	-	-	-0.0461	-	-
			(0.0323)		
$\Delta\log(\text{transportGDP})$	-	-	-	0.6356	-
				(0.5067)	
$\Delta\log(\text{transportGDP})^2$	-	-	-	-0.0258	-
				(0.0282)	
$\Delta\log(\text{heatingdegreedays})$	-	-	-	-	-
$\Delta\log(\text{coolingdegreedays})$	-	-	-	-	-
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 Section V.4. Specifications are described in Table 6.
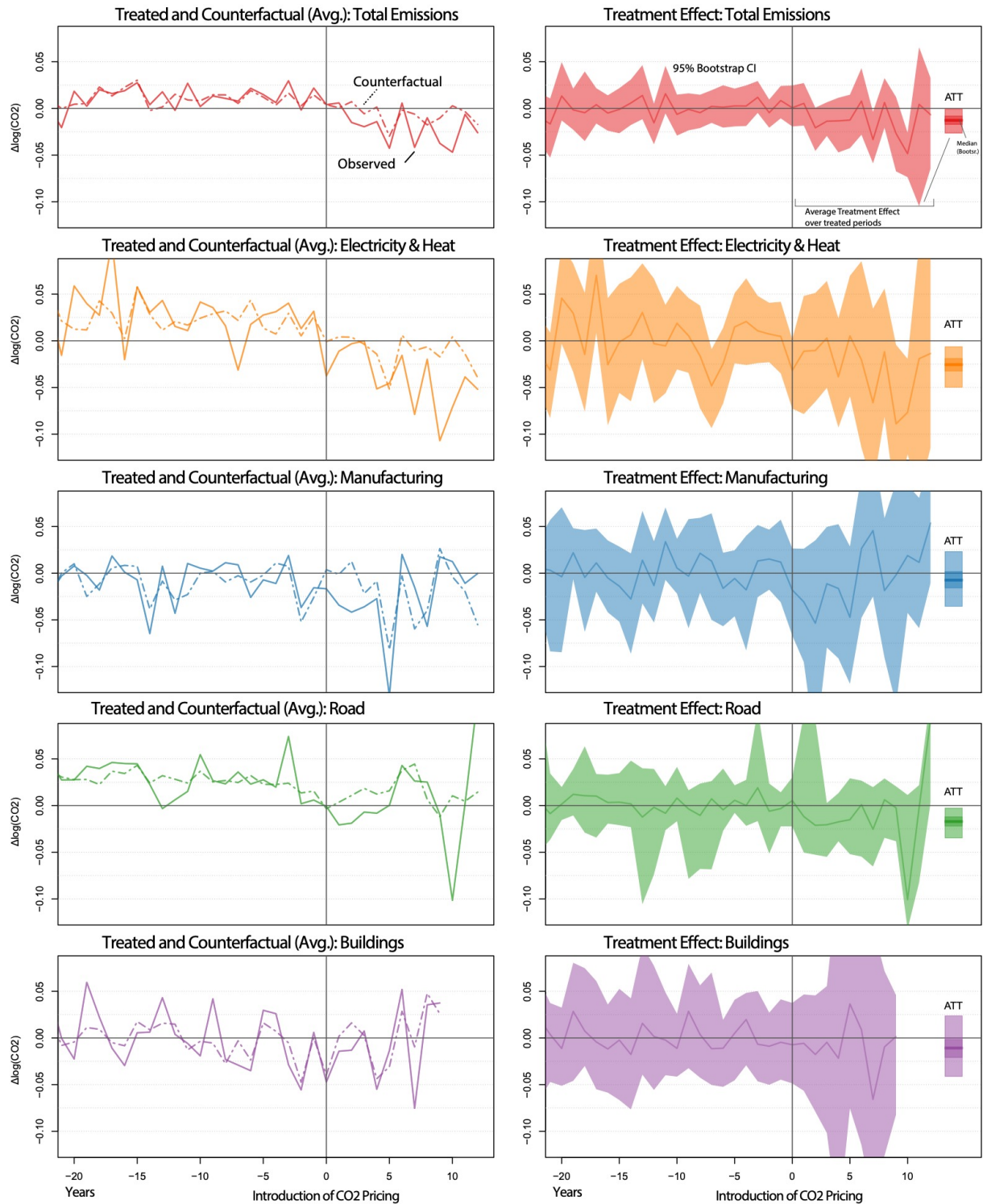
Figure 2.

Generalized synthetic control estimates of average treatment effects

Note: Left panels show observed (solid) and counterfactual (dashed) change in log emissions by sector. Right panels show the estimated treatment effects as the difference between observed and counterfactual, with the estimate of the Average Treatment Effects (ATT) and its 95% bootstrap confidence interval (shaded).
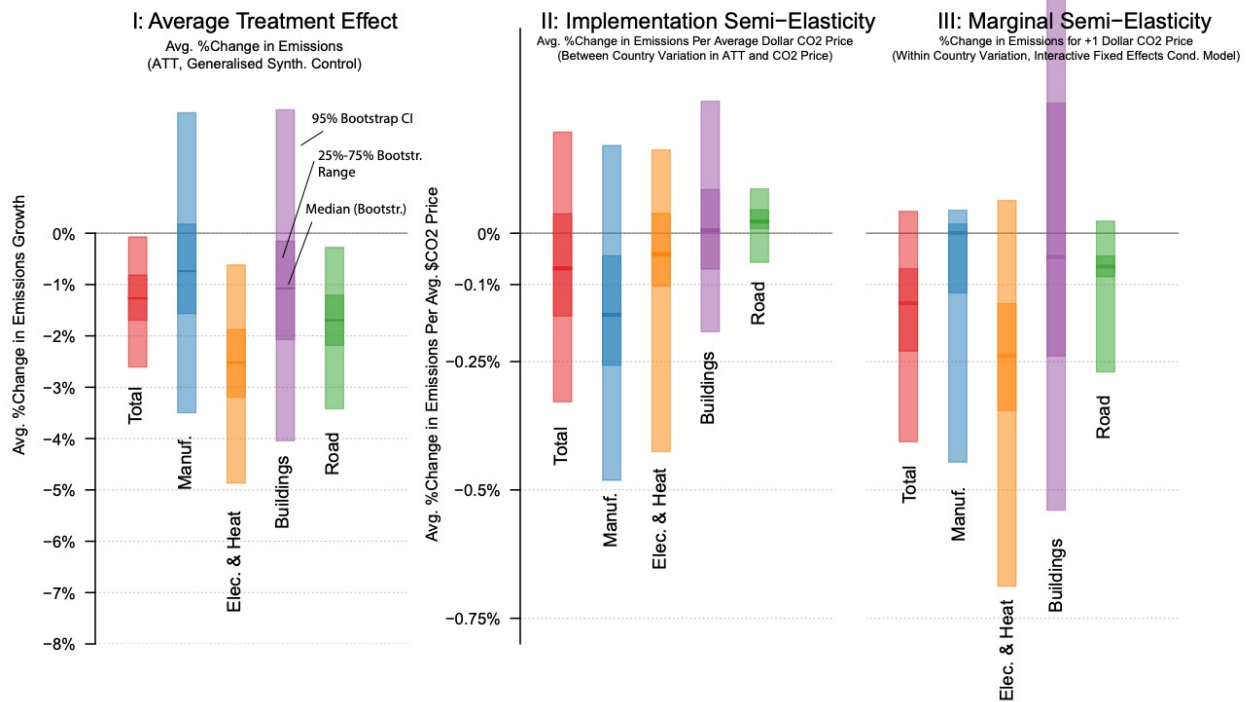
Figure 3.

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

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

---

[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 generalized 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 generalized synthetic control estimator from Section 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 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

---

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

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 generalized synthetic control estimator in Section 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 really large estimates for the implementation semi-elasticity, suggesting that a small number of countries may be driving the results. We explore this possibility further in robustness checks in Section 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: $\Delta\log(\text{CO2})_{i,k,t}$

	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.

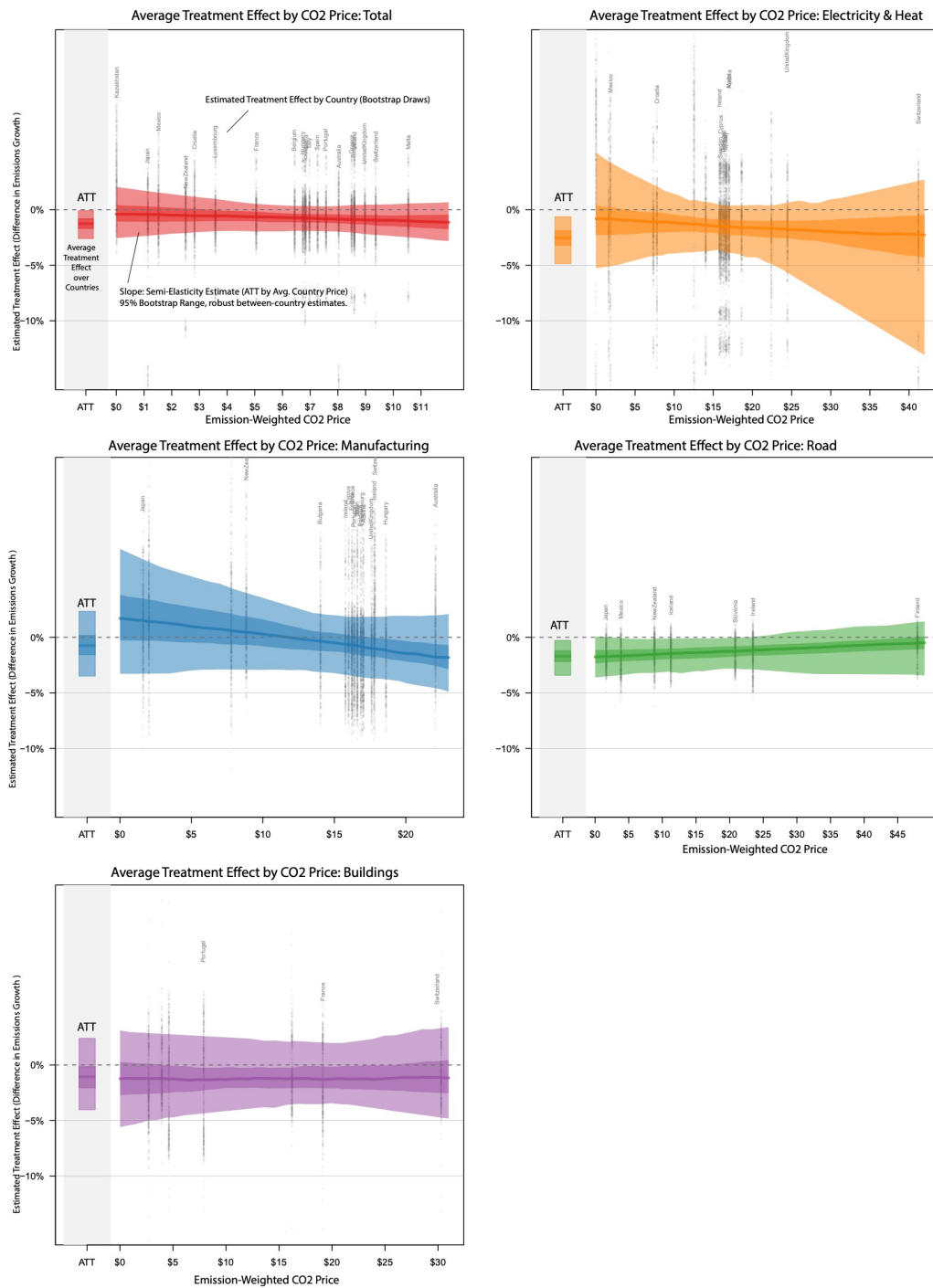**Figure 4.**

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 generalized 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 generalized synthetic control analysis in Section 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 pricing schemes have committed changes in advance, and changes to prices are unlikely to be driven by contemporaneous growth in CO_2 emissions.[23] Several considerations support this assumption. First, economists have explicitly recognized the long time lags between (uncertain) CO_2 emissions outcomes and politically initiated adjustments to the carbon tax rate (or the emissions cap in the case of carbon markets). To reduce the mitigation uncertainty this creates, 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

---

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

mechanisms, thereby providing assurance that carbon price levels can be preemptively adjusted in accordance with specific emission reduction targets. 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 managed 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] In emissions trading systems, the issue of simultaneity is more complex, since 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 indicates 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 – unanticipated regulatory changes and policy 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 (Koch *et al.* 2014, 2016; Friedrich *et al.* 2019). These regulatory events or 'shocks'[26] are best understood as the product of protracted negotiations with emissions-intensive and trade-exposed industries – often resulting in substantial overcompensation (Grubb 2014; Martin *et al.* 2014b) – rather than contemporaneous responses to 'over-achievement' or 'under-achievement' of emissions reductions under the cap. For extended periods, the EU carbon market has been stationary at low CO_2 prices, only occasionally undergoing periods of volatility in response to politically determined (rather than 'emissions determined') changes in the expectations of market participants, at least with respect to the time period considered in our study.[27]

---

[24] 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 modeled in Bocklet *et al.* (2019).

[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".

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 Section 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 there being a preexisting 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).

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]

---

[28] Athey *et al.* (2018) show that the generalized synthetic control estimator (based on the IFE model) and their proposed MC estimator belong to a general class of matrix completion methods based on matrix factorization. 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 centered 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 center 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 Section 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); 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 $i_{TR} \in 1,2,...N_{TR}$, that has had a sufficiently long treated period $t_{TR} \in t_1,... \geq t_{23}$ 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 Section 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 we find 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 it for other sectors. As shown in 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.
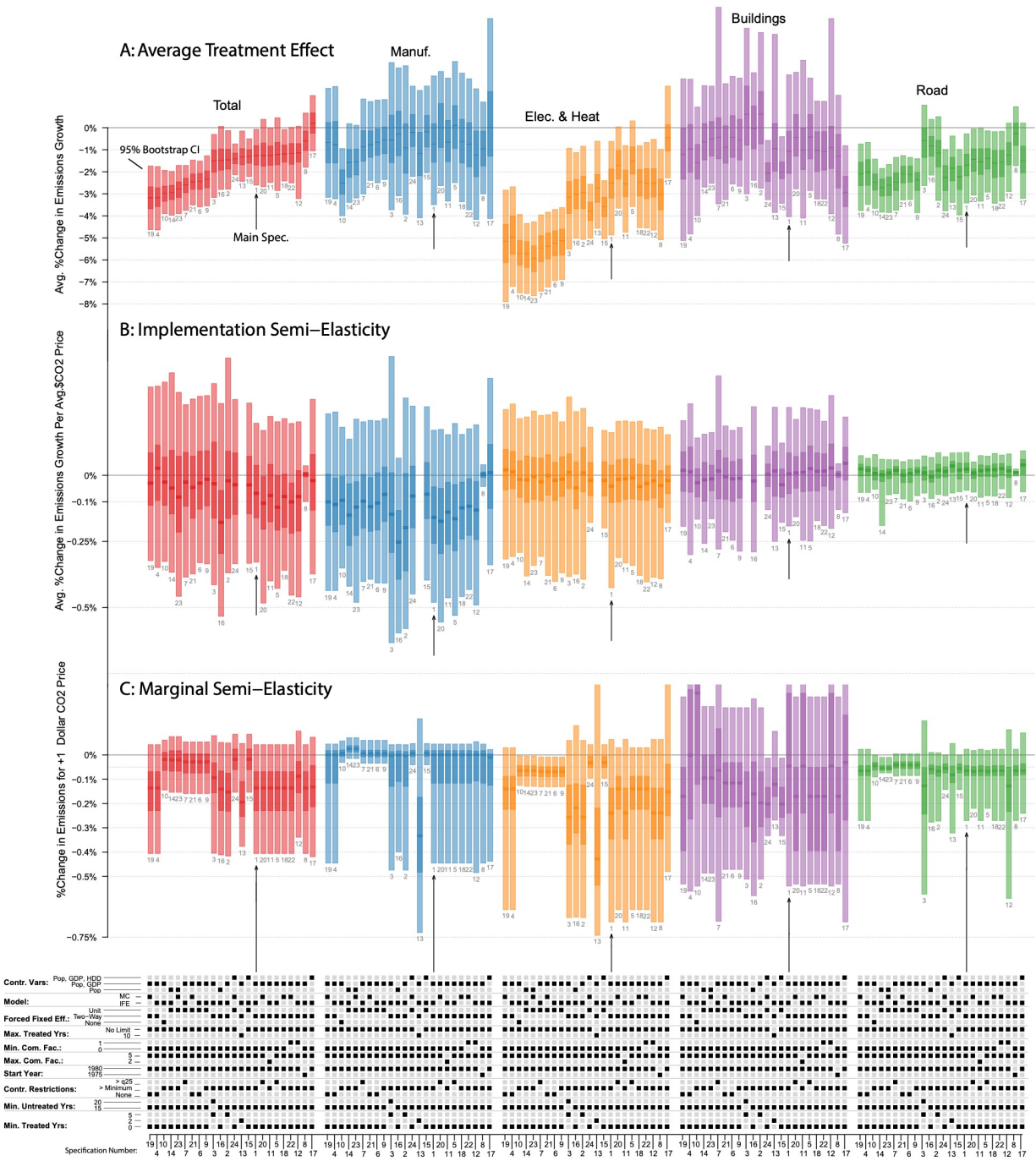
Figure 5.

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:

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

---

[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.

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

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

where $\hat{b}_{tot}$ is the estimated implementation semi-elasticity from Section 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 Section 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 renewables become cheaper than fossil fuels in a growing number of sectors and markets, economies may reach an inflection point where the price elasticity of emissions shifts upward as demand for fossil fuels plummets. Parameter constancy is a strong simplifying assumption, but on the other hand, any variation in emissions that occurs due to time-dependency of policy effects likely falls well within the already wide range of simulated outcomes. Uncertainty about the phasing-in of treatment effects is likely dwarfed by the uncertainty present in the parameter estimates. The simulations

below are perhaps optimistic in the short run, since they assume prices affect emissions immediately without delay.

Figure 6 shows projected emissions for the SSP2 reference scenario (commonly referred to as the 'middle of the road' scenario) together with hypothetical carbon price paths. [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% interquartile bootstrap ranges are shown as shaded for the first and second pricing scheme: constant $8 and constant $30).

Our conclusion is somber: 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

---

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

scheme in Figure 6). This seems far outside the realm of political feasibility, particularly since the simulation is optimistic in the assumption of immediate emission reductions in response to the introduction of carbon pricing.
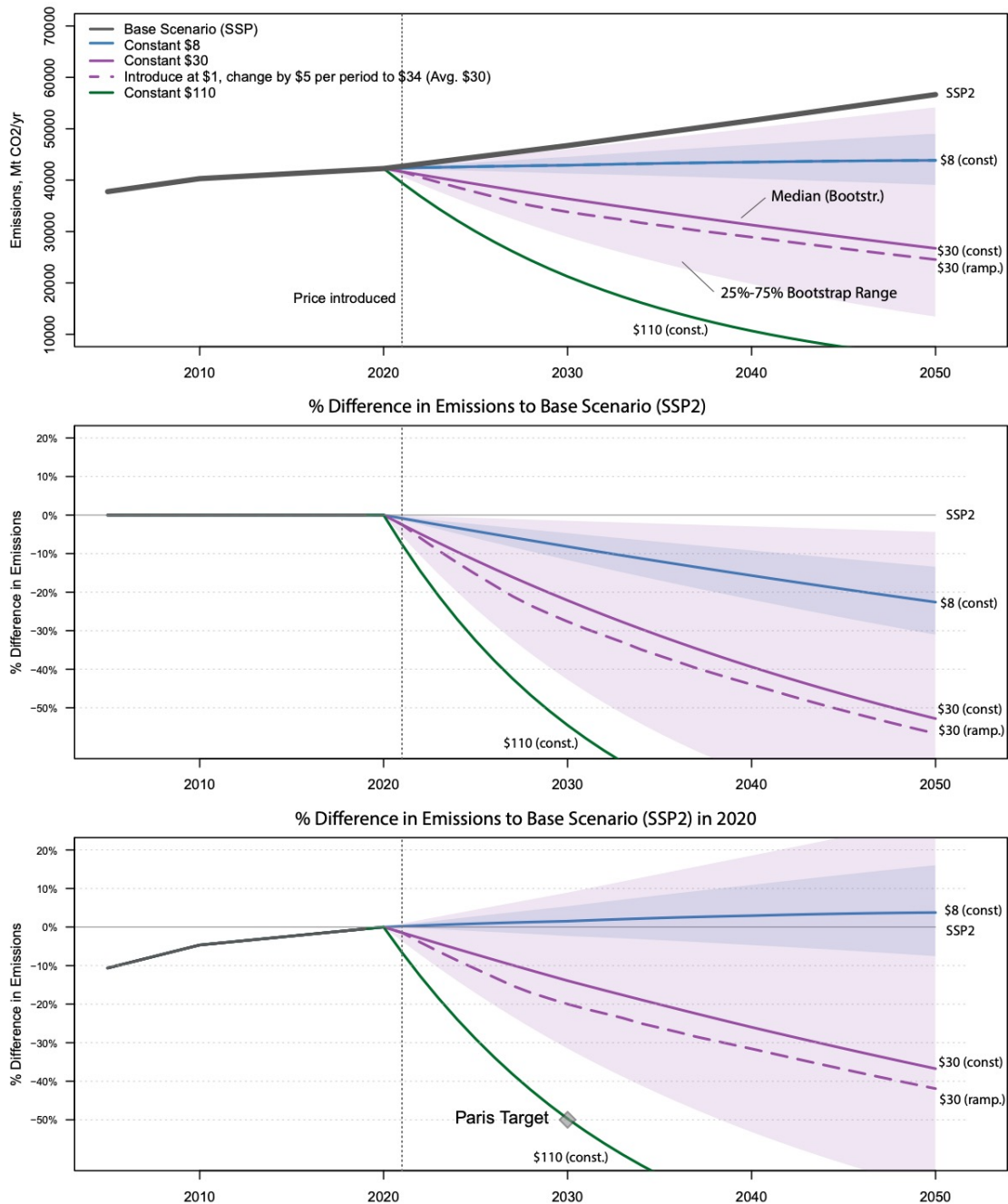
Figure 6.

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 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 contributes to a fuller understanding of this question, based on a novel approach to estimating changes in CO_2 emissions associated with (i) the introduction of carbon pricing irrespective of the price level; (ii) the implementation effect of carbon pricing conditional on the price level; and (iii) marginal changes in the price per ton of CO_2 after the pricing instrument has been introduced.

Consistent across a range of model specifications, 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 average treatment effect 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 centered 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 complementary (non-pricing) policies and substantial public investments to deploy green technologies 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 center around –0.18 for electricity and heat, buildings, and the economy as a whole.[32] 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. Our (implied) gasoline price elasticity estimates center around -0.25. We add a caveat: 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,[33] then the disparity between our estimates and those of previous empirical studies would be even greater.

Several considerations lead us to conclude that our significantly lower elasticity estimates are not mere artifacts of statistical noise but rather indicative of poignant empirical realities. First, relying on empirical estimates of energy demand elasticities based on data from the 1980s and earlier may lead researchers and policymakers to underestimate the extent to which energy demand has been shifting towards relatively fast-growing and less price responsive products and regions.[34] Second, policy response models of CO_2 emissions (both ex ante and ex post) have tended to poorly capture the inertia of infrastructure lock-in.[35] 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 sectors and industries. The importance of this can be seen when considering that governments may be incentivized

---

[32] This is calculated based on our estimate of the average marginal semi-elasticity by 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 reported in this paragraph for the price elasticity of gasoline demand.

[33] See, e.g., Andersson (2019).

[34] See, e.g., the evidence for world oil demand in Daragay and Gately (2010).

[35] See, e.g., the analysis in Avner *et al.* (2014) of urban vs. rural responses to carbon pricing under varying densities of mass public transport infrastructure.

to '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.[36] Taken together, these considerations should cast doubt on the notion that the price elasticity of energy demand should be stable over time, an implicit assumption of our simulation exercise. Instead, emissions elasticities are likely to be a function not only of the price of emissions, but also the initial state in the evolutionary process of complex energy-technological systems to which the price is applied (Mercure *et al.* 2014; Grubb 2014). As a consequence, we emphasize that any conclusions drawn from our simulation exercise, although they are based on empirically grounded and up-to-date elasticity estimates, are limited by an irreducible element of uncertainty.

As a final note, our assessment corroborates several 'best practices' for optimizing carbon pricing reforms that have been identified elsewhere. First, carbon prices are undermined the more they are volatile inter-annually; their environmental efficacy tends to be enhanced when they are on a credible upward trajectory, which has been rare hitherto, but which can be reinforced through built-in price adjustment mechanisms (Hafstead *et al.* 2017; Metcalf 2020). Alternatively, policymakers may attempt to price CO_2 emissions at very high levels initially to better capture climate externalities under conditions of uncertainty, which may counterintuitively imply a declining CO_2 price path over time (Daniel *et al.* 2019). Such an experiment would be intriguing, but it seems unlikely to pass muster without substantial revenue recycling in the early phase to counteract any regressive impacts on individuals whose carbon cost exposure comprises a salient share of their household income (Klenert *et al.* 2018).

Second, while there are compelling arguments that might lead policymakers to prefer carbon pricing schemes which strategically target a small number of industries or sectors with significant intersectoral carbon linkages (King *et al.* 2019), policymakers opting for such an approach should recognize that the discrepancy between current coverage levels and those that are likely needed to comply with 1.5-2°C climate targets remains stark. Thus, additional regulations that implicitly price CO_2 emissions or public green investments that reduce the costs of alternatives will be needed to incentivize decarbonization wherever an explicit and sufficiently high CO_2 price is

---

[36] For example, countries such as Denmark and Germany underwent multi-decade processes of energy system transformation in response to the oil price shocks of the 1970s, as discussed in Grubb *et al.* (2017) and elsewhere.

absent. Under a targeted carbon pricing scheme, exemptions for emissions-intensive industries should still be eliminated to the greatest extent possible, including in the implicit form of unpriced carbon embodied in internationally traded goods (Moran *et al.* 2018); nor should greater reliance on non-pricing climate measures distract policymakers from the need to eliminate fossil fuel subsidies that function as a negative carbon price, and about three quarters of which globally are due to domestic factors which are alterable via energy pricing reforms (Coady *et al.* 2019).

Climate change mitigation policies, when strategically targeted and combined, may be highly synergistic (Farmer *et al.* 2019; Grubb 2014; Mercure *et al.* 2014). Carbon pricing still has the potential to be a powerful tool contributing to emission reductions, but it is 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 sources, 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;

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

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

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

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

$q^{CO2}_{i,t,j}$ 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. $q^{CO2}_{i,t,j}/q^{CO2}_{i,t}$. 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.

Table A.1

Data Sources for CO$_2$ Emissions by Emission Source

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

Table A.2

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

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 tons 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 ton CO_2 (constant 2015 prices)	Updated from Dolphin *et al.* (2020)
GDP: total, manufacturing, transport, and services	US dollars (millions, constant 2015 prices)	UNCTAD (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 specifications are informed by diagnostic tests for cross-section dependence, common factors, unit roots, and panel cointegration.

First, we strongly reject the null hypothesis of cross-section independence (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 of 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. Thus, all variables are integrated of order $I(1)$.

The null hypothesis that additive (time and unit) fixed effects are sufficient is strongly rejected at the 1% level using the Hausman-type test in Bai (2009). 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).[37]

To distinguish between common and idiosyncratic components of the residuals (Bai and Ng 2004, 2010), we apply the PANNICA testing procedure described in Reese and Westerlund (2016) with results presented in Table C.1. The procedure combines the strong small sample performance of the tests developed in Pesaran (2006) with the flexibility regarding orders of integration for common and idiosyncratic error components as in the tests developed in Bai and Ng (2004, 2010). The results corroborate the presence of multiple common factors. When variables are entered in levels, we fail to reject the null hypothesis 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.

---

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

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. We apply those proposed by Westerlund (2007) to the baseline specification for the model of total aggregate CO_2 emissions. Results are presented in Table C.2. 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.2

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
(0.000)	(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

Table D.2.

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

	Total		Electricity and heat		Manufacturing		Road transport		Buildings	
ΔP_t	-0.001 (0.001)	-	-0.002 (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 (0.001)	-	-6e-04 (7e-04)	-	-0.0017 (0.003)
P_{t-1}	-	0.0015 (0.001)	-	0.0019 (0.002)	-	0 (0.001)	-	7e-04 (8e-04)	-	0.002 (0.003)
$\Delta\log(\mathrm{GDP})$	-0.229 (0.98)	-0.231 (0.939)	-1.735 (1.878)	-1.737 (2.004)	0.9005 (1.981)	0.87 (2.093)	-1.642 (4.697)	-1.635 (4.634)	3.696 (34.762)	4.8632 (38.923)
$\Delta\log(\mathrm{GDP})^2$	0.0249 (0.04)	0.0252 (0.038)	0.0805 (0.078)	0.080 (0.082)	-0.012 (0.085)	-0.01 (0.0901)	0.068 (0.184)	0.068 (0.181)	-0.149 (1.391)	-0.1946 (1.554)
$\Delta\log(\mathrm{population})$	-1.834 (1.095)	-1.802 (1.215)	-3.007 (2.648)	-3.2801 (2.556)	-3.296 (1.801)	-3.289 (1.951)	-2.779 (2.77)	-2.863 (3.175)	-8.05 (5.092)	-7.263 (6.015)
$\Delta\log(\mathrm{servicesGDP})$	-	-	-	-	-	-	-	-	-4.2258 (18.459)	-5.0843 (21.174)
$\Delta\log(\mathrm{servicesGDP})^2$	-	-	-	-	-	-	-	-	0.23 (0.916)	0.272 (1.046)
$\Delta\log(\mathrm{manufacturingGDP})$	-	-	-	-	0.387 (0.558)	0.394 (0.533)	-	-	-	-
$\Delta\log(\mathrm{manufacturingGDP})^2$	-	-	-	-	-0.020 (0.027)	-0.021 (0.026)	-	-	-	-
$\Delta\log(\mathrm{transportGDP})$	-	-	-	-	-	-	2.079 (2.342)	2.082 (2.415)	-	-
$\Delta\log(\mathrm{transportGDP})^2$	-	-	-	-	-	-	-0.09 (0.117)	-0.09 (0.121)	-	-
$\Delta\log(\mathrm{heatingdegreedays})$	-	-	-	-	-	-	-	-	-	-
$\Delta\log(\mathrm{coolingdegreedays})$	-	-	-	-	-	-	-	-	-	-
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:

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

where p is the emissions-weighted carbon price, the bars indicate cross-section averages of the variables, $\omega^{CA}_{1,i,k},\dots,\omega^{CA}_{4,i,k}$ are the unknown coefficients for the cross-section averages, and the superscript *Sel* indicates that the number of lags of first-differenced variables (which may be heterogeneous of i) are selected using a general-to-specific lag truncation procedure (Campos *et al.* 2005).[38] 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 (with respect to the carbon price) is significantly different from zero, we compute the long run average coefficient as

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

---

[38] 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).

where the standard error, $\bar{T}$ statistic, and p-value are computed using the Delta method.[39] 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, $L^{CA} = T^{1/3} - 1$, 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.

---

[39] 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]
$\beta_{0,i.k}^{EC}$	-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)
log(oilprice)			-.0176
			(.0293)
Treated countries	2	5	3
Treated observations	50	119	
Total observations			129
Countries used to compute ^{CA}	39	39	39
RMSE	0.0119	0.0283	0.0121
Panel EC $\bar{T}$ test for $\log(CO2)_{i,k,t-1}$	-4.480	-7.141	-3.574
	[p≤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. $\beta_{0,i.k}^{EC}$ denotes the speed of equilibrium adjustment; the panel EC $\bar{T}$ statistic tests the significance of the cointegrating relationship; RMSE is the root mean squared error; and 'CD test' refers to the Pesaran (2015) test for weak cross-section dependence, under the null hypothesis of cross-section independence.

Supplementary Material

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

References

Abadie, A., 2019. Using synthetic controls: Feasibility, data requirements, and methodological aspects. *Journal of Economic Literature.*

Abadie, A., Athey, S., Imbens, G.W. and Wooldridge, J.M. 2020. Sampling-Based versus Design-Based Uncertainty in Regression Analysis. *Econometrica*, 88(1), 265-296.

Abadie, A., Diamond, A. and Hainmueller, J. 2010. Synthetic control methods for comparative case studies: Estimating the effect of California's tobacco control program. *Journal of the American Statistical Association*, 105(490), 493-505.

——— 2015. Comparative politics and the synthetic control method. *American Journal of Political Science*, 59(2), 495-510.

Abrell, J., Ndoye Faye, A. and Zachmann, G. 2011. Assessing the impact of the EU ETS using firm level data. Bruegel working paper No. 2011/08.

Abrell, J., Kosch, M., Rausch, S. 2020. How effective was the UK carbon tax?—A machine learning approach to policy evaluation. Working Paper 19/317bis, Center of Economic Research at ETH Zurich.

Agnolucci, P. 2010. Stochastic trends and technical change: the case of energy consumption in the British industrial and domestic sectors. *The Energy Journal*, 111-135.

Ahn, S.C. and Horenstein, A.R. 2013. Eigenvalue ratio test for the number of factors. Econometrica, 81(3), 1203-1227.

Andersen, M., Dengsoe, N., and Pedersen, A. 2000. An evaluation of the impact of green taxes in Nordic countries. Copenhagen, Denmark: The Nordic Council (TemaNord).

Andersen, M.S. 2004. Vikings and virtues: a decade of CO_2 taxation. *Climate Policy*, 4(1), 13-24.

Andersson, J.J. 2019. Carbon taxes and CO_2 Emissions: Sweden as a Case Study. *American Economic Journal: Economic Policy*, 11(4), 1-30.

Athey, S., Bayati, M., Doudchenko, N., Imbens, G. and Khosravi, K. 2018. Matrix completion methods for causal panel data models. National Bureau of Economic Research, Working Paper No. 25132.

Avner, P., Rentschler, J., and Hallegatte, S. 2014. Carbon price efficiency lock-in and path dependence in urban forms and transport infrastructure. World Bank, Policy Research Working Paper 6941, 1-26.

Bai, J. 2009. Panel data models with interactive fixed effects. *Econometrica*, 77(4), 1229-1279.

Bai, J., Kao, C. and Ng, S. 2009. Panel cointegration with global stochastic trends. *Journal of Econometrics*, 149(1), 82-99.

Bai, J. and Ng, S. 2004. A PANIC attack on unit roots and cointegration. *Econometrica*, 72(4), 1127-1177.

——— 2010. Panel unit root tests with cross-section dependence: a further investigation. *Econometric Theory*, 26(4).

Barron, A.R., Fawcett, A.A., Hafstead, M.A., McFarland, J.R. and Morris, A.C. 2018. Policy insights from the EMF 32 study on U.S. carbon tax scenarios. *Climate Change Economics*, 9(01), 1840003.

Baumol, W.J. and Oates, W.E. 1988. *The theory of environmental policy (2nd edition)*. Cambridge University Press.

Bayer, P. and Aklin, M., 2020. The European Union emissions trading system reduced CO_2 emissions despite low prices. *Proceedings of the National Academy of Sciences*, 117(16), 8804-8812.

Bel, G. and Joseph, S. 2015. Emission abatement: Untangling the impacts of the EU ETS and the economic crisis. *Energy Economics*, 49, 531-539.

Best, R., Burke, P.J. and Jotzo, F. 2020. Carbon Pricing Efficacy: Cross-Country Evidence. *Environmental and Resource Economics*, 1-26.

Billmeier, A. and Nannicini, T. 2013. Assessing economic liberalization episodes: A synthetic control approach. *Review of Economics and Statistics*, 95(3), 983-1001.

Bocklet, J., Hintermayer, M., Schmidt, L. and Wildgrube, T. 2019. The reformed EU ETS-intertemporal emission trading with restricted banking. *Energy Economics*, 84, 104486.

Carbon Dioxide Information Analysis Center (CDIAC). 2017. Fossil fuel CO_2 emissions: Global, regional, and national annual time series (1751-2014). Oak Ridge National Laboratory, U.S. Department of Energy. Oak Ridge, TN, USA.

Carbone, J.C., Rivers, N., Yamazaki, A. and Yonezawa, H. 2020. Comparing applied general equilibrium and econometric estimates of the effect of an environmental policy shock. *Journal of the Association of Environmental and Resource Economists*, 7(4), 687-719.

Chamon, M. and Kaplan, E. 2013. The iceberg theory of campaign contributions: Political threats and interest group behavior. *American Economic Journal: Economic Policy*, 1-31.

Chatterjee, S. and Huang, K.W. 2020. Unrealistic energy and materials requirement for direct air capture in deep mitigation pathways. *Nature Communications*, 11(1), 1-3.

Chudik, A. and Pesaran, M.H. 2015. Common correlated effects estimation of heterogeneous dynamic panel data models with weakly exogenous regressors. *Journal of Econometrics*, 188(2), 393-420.

Coady, D., Parry, I., Nghia-Piotr, L., Shang, B. 2019. Global fossil fuel subsidies remain large: An update based on country-level estimates. IMF Working Paper, Fiscal Affairs Department.

Copeland, B.R. and Taylor, M.S. 2003. *Trade and the environment: Theory and evidence.* Princeton, NJ: Princeton University Press.

Cropper, M.L., Morgenstern, R.D. and Rivers, N., 2018. Policy Brief—Facilitating Retrospective Analysis of Environmental Regulations. *Review of Environmental Economics and Policy*, 12(2), 359-370.

Dahl, C.A. 2012. Measuring global gasoline and diesel price and income elasticities. *Energy Policy*, 41, 2–13.

Daniel, K.D., Litterman, R.B. and Wagner, G., 2019. Declining CO_2 price paths. *Proceedings of the National Academy of Sciences*, 116(42), 20886-20891.

Dargay, J.M. and Gately, D. 2010. World oil demand's shift toward faster growing and less price-responsive products and regions. *Energy Policy*, 38(10), 6261-6277.

Dechezleprêtre, A., Nachtigall, D., Venmans, F. 2018. The joint impact of the European Union emissions trading system on carbon emissions and economic performance. OECD Economics Department Working Papers No. 1515. Paris, France.

Dolphin, G., Pollitt, M.G., and Newbery, D.G. 2020. The political economy of carbon pricing: a panel analysis. *Oxford Economic Papers*, 72(2), 472-500.

Dube, A. and Zipperer, B. 2015. Pooling multiple case studies using synthetic controls: An application to minimum wage policies. Institute for the Study of Labor (IZA) Discussion Paper No. 8944.

Economists' Statement on Carbon Dividends. 2019. *The Wall Street Journal*, January 17.

Eberhardt, M., Helmers, C. and Strauss, H. 2013. Do spillovers matter when estimating private returns to R&D? *Review of Economics and Statistics*, *95*(2), 436-448.

Eberhardt, M. and Presbitero, A.F. 2015. Public debt and growth: Heterogeneity and non-linearity. *Journal of International Economics,* 97(1), 45-58.

Edenhofer, O., Knopf, B., Barker, T., Baumstark, L., Bellevrat, E., Chateau, B., Criqui, P., Isaac, M., Kitous, A., Kypreos, S. and Leimbach, M. 2010. The economics of low stabilization: model comparison of mitigation strategies and costs. *The Energy Journal,* 11-48.

Ekins, P. and Barker, T. 2001. Carbon taxes and carbon emissions trading. *Journal of Economic Surveys, 15*(3), 325-376.

Ellerman, A. D., and Buchner, B. K. 2008. Over-allocation or abatement? A preliminary analysis of the EU ETS based on the 2005–06 emissions data. *Environmental and Resource Economics*, *41*(2), 267-287.

Erutku, C. and Hildebrand, V. 2018. Carbon tax at the pump in British Columbia and Quebec. *Canadian Public Policy, 44*(2), 126-133.

Farmer, J.D., Hepburn, C., Ives, M.C., Hale, T., Wetzer, T., Mealy, P., Rafaty, R., Srivastav, S. and Way, R., 2019. Sensitive intervention points in the post-carbon transition. *Science, 364*(6436), 132-134.

Fawcett, A.A., Clarke, L.E., Rausch, S. and Weyant, J.P. 2014. Overview of EMF 24 policy scenarios. *Energy Journal,* 35.

Ferguson, T., Jorgensen, P. and Chen, J. 2013. Party Competition and Industrial Structure in the 2012 Elections. International *Journal of Political Economy*, 42, 3-41.

Fricko, O., Havlik, P., Rogelj, J., Klimont, Z., Gusti, M., Johnson, N., Kolp, P., Strubegger, M., Valin, H., Amann, M. and Ermolieva, T., 2017. The marker quantification of the Shared Socioeconomic Pathway 2: A middle-of-the-road scenario for the 21st century. *Global Environmental Change*, 42, 251-267.

Fried, S., Novan, K., Peterman, W.B. 2020. The Macro Effects of Climate Policy Uncertainty. Working paper, ASSA Annual Meeting.

Friedrich, M., Fries, S., Pahle, M. and Edenhofer, O., 2019. Understanding the explosive trend in EU ETS prices–fundamentals or speculation? arXiv preprint arXiv:1906.10572.

Gengenbach, C., Urbain, J.P. and Westerlund, J. 2016. Error correction testing in panels with common stochastic trends. *Journal of Applied Econometrics*, *31*(6), 982-1004.

Gershkovich, E. 2019. How does a powerful Russian lobby plan to halt climate change? With coal, oil and gas. *The Moscow Times*, November 15.

Gobillon, L. and Magnac, T. 2016. Regional policy evaluation: Interactive fixed effects and synthetic controls. *Review of Economics and Statistics*, 98(3), 535-551.

Goulder, L. and Hafstead, M. 2017. *Confronting the climate challenge: A general equilibrium assessment of U.S. policy options.* Columbia University Press.

Grant, N., Hawkes, A., Napp, T. and Gambhir, A. 2020. The appropriate use of reference scenarios in mitigation analysis. *Nature Climate Change*, 1-6.

Griscom, B.W., Adams, J., Ellis, P.W., Houghton, R.A., Lomax, G., Miteva, D.A., Schlesinger, W.H., Shoch, D., Siikamäki, J.V., Smith, P. and Woodbury, P., 2017. Natural climate solutions. *Proceedings of the National Academy of Sciences*, *114*(44), 11645-11650.

Grubb, M., Bashmakov, I., Drummond, P., Myshak, A., Hughes, N., Biancardi, A., Agnolucci, P. and Lowe, R. 2017. Minus 1: Empirics, theory and implications of the 'Bashmakov-Newbery Range of Energy Expenditure'. Or: an exploration of energy cost constants, affordability limits and adjustment processes. Working paper, Institute for New Economic Thinking.

Grubb, M. 2014. *Planetary economics: Energy, climate change and the three domains of sustainable development.* Routledge.

Gugler, K., Haxhimusa, A., Liebensteiner, M. 2020. Carbon pricing and emissions: Causal effects of Britain's carbon tax. Working paper.

Hafstead, M., Metcalf, G.E. and Williams III, R.C. 2017. Adding quantity certainty to a carbon tax through a tax adjustment mechanism for policy pre-commitment. *Harvard Environmental Law Review*, 41, 41-57.

Hendry, D.F. and Doornik, J.A. 2014. *Empirical model discovery and theory evaluation: automatic selection methods in econometrics.* Cambridge, MA: MIT Press.

Hepburn, C. 2017. Climate change economics: Make carbon pricing a priority. *Nature Climate Change, 7*(6), 389-390.

Hepburn, C., Adlen, E., Beddington, J., Carter, E.A., Fuss, S., Mac Dowell, N., Minx, J.C., Smith, P. and Williams, C.K., 2019. The technological and economic prospects for CO_2 utilization and removal. *Nature, 575*(7781), 87-97.

Im, K.S., Pesaran, M.H. and Shin, Y. 2003. Testing for unit roots in heterogeneous panels. *Journal of Econometrics, 115*(1), 53-74.

Intergovernmental Panel on Climate Change (IPCC). 2006. *Guidelines for national greenhouse gas inventories.* IPCC, Geneva, Switzerland.

————— 2014. *Climate change 2014: Mitigation of climate change. Working Group III Contribution to the Fifth Assessment Report of the Intergovernmental Panel on Climate Change.* [Edenhofer, O. *et al.* (eds.)]. New York: Cambridge University Press.

International Energy Agency (IEA). 2018. *CO_2 emissions from fossil fuel combustion.* Paris, France.

Isaksen, E.T. 2020. Have international pollution protocols made a difference? *Journal of Environmental Economics and Management*, 103, 102358.

Jenkins, J.D. 2014. Political economy constraints on carbon pricing policies: What are the implications for economic efficiency, environmental efficacy, and climate policy design? *Energy Policy*, 69, 467-477.

Kapetanios, G., Pesaran, M.H. and Yamagata, T. 2011. Panels with non-stationary multifactor error structures. *Journal of Econometrics, 160*(2), 326-348.

Kim, D. and Oka, T. 2014. Divorce law reforms and divorce rates in the USA: an interactive fixed-effects approach. *Journal of Applied Econometrics, 29*(2), 231-245.

King, M., Tarbush, B. and Teytelboym, A. (2019). Targeted carbon tax reforms. *European Economic Review*, 119, 526-547.

Klenert, D., Mattauch, L., Combet, E., Edenhofer, O., Hepburn, C., Rafaty, R. and Stern, N., 2018. Making carbon pricing work for citizens. *Nature Climate Change, 8*(8), 669-677.

Kneip, A., Sickles, R.C. and Song, W. 2012. A new panel data treatment for heterogeneity in time trends. *Econometric Theory*, 590-628.

Koch, N., Fuss, S., Grosjean, G. and Edenhofer, O. 2014. Causes of the EU ETS price drop: Recession, CDM, renewable policies or a bit of everything?—New evidence. *Energy Policy*, 73, 676-685.

Koch, N., Grosjean, G., Fuss, S. and Edenhofer, O. 2016. Politics matters: Regulatory events as catalysts for price formation under cap-and-trade. *Journal of Environmental Economics and Management*, 78, 121-139.

Koller, M. and Stahel, W.A. 2011. Sharpening Wald-type inference in robust regression for small samples. *Computational Statistics & Data Analysis*, *55*(8), 2504-2515.

Lawley, C. and Thivierge, V. 2018. Refining the evidence: British Columbia's carbon tax and household gasoline consumption. *The Energy Journal*, 39(2).

Lin, B. and Li, X. 2011. The effect of carbon tax on per capita CO_2 emissions. *Energy Policy*, 39(9), 5137-5146.

Leroutier, M. (2019) Carbon Pricing and Power Sector Decarbonisation: Evidence from the UK. FAERE Working Paper 2019.12.

Madlener, R., Bernstein, R., and Ángel Alva González, M. 2011. Econometric estimation of energy demand elasticities. E.ON Energy Research Center, Aachen University, Aachen, Germany, Series 3.

Markard, J. and Rosenbloom, D. 2020. Political conflict and climate policy: the European emissions trading system as a Trojan Horse for the low-carbon transition? *Climate Policy*, 1-20.

Martin, R., De Preux, L.B. and Wagner, U.J. 2014. The impact of a carbon tax on manufacturing: Evidence from microdata. *Journal of Public Economics*, 117, 1-14.

Martin, R., Muûls, M., De Preux, L.B. and Wagner, U.J. 2014b. Industry compensation under relocation risk: A firm-level analysis of the EU emissions trading scheme. *American Economic Review*, *104*(8), 2482-2508.

Martus, E., 2019. Russian industry responses to climate change: the case of the metals and mining sector. *Climate Policy*, *19*(1), 17-29.

Meng, K.C. and Rode, A. 2019. The social cost of lobbying over climate policy. *Nature Climate Change*, 9(6): 472-476.

Mercure, J.F., Pollitt, H., Chewpreecha, U., Salas, P., Foley, A.M., Holden, P.B. and Edwards, N.R. 2014. The dynamics of technology diffusion and the impacts of climate policy instruments in the decarbonisation of the global electricity sector. *Energy Policy*, 73, 686-700.

Mercure, J.F., Pollitt, H., Bassi, A.M., Viñuales, J.E. and Edwards, N.R. 2016. Modelling complex systems of heterogeneous agents to better design sustainability transitions policy. *Global Environmental Change*, 37, 102-115.

Metcalf, G.E. and Stock, J.H. 2020. Measuring the macroeconomic impact of carbon taxes. *AEA Papers and Proceedings*, 110, 101-06.

——————— 2020b. The macroeconomic impact of Europe's carbon taxes. Working Paper 27488, National Bureau of Economic Research.

Metcalf, G.E. and Weisbach, D. 2009. The design of a carbon tax. *Harvard Environmental Law Review*, 33.

Metcalf, G.E. 2020. An emissions assurance mechanism: adding environmental certainty to a US carbon tax. *Review of Environmental Economics and Policy*, *14*(1), 114-130.

Mildenberger, M. 2020. *Carbon captured: How business and labor control climate politics.* MIT Press.

Mistry, M.N. 2019. Historical global gridded degree-days: A high-spatial resolution database of CDD and HDD. *Geoscience Data Journal*, 6(2), 214-221.

Moran, D., Hasanbeigi, A., and Springer, C. 2018. *The carbon loophole in climate policy: Quantifying the embodied carbon in traded products.* KGM & Associates Pty Ltd. and Global Efficiency Intelligence, LLC.

Nordhaus, W.D. 1992. An optimal transition path for controlling greenhouse gases. *Science,* 258(5086), 1315-1319.

——————— 1993. Optimal greenhouse-gas reductions and tax policy in the "DICE" model. *American Economic Review*, 83(2), 313-317.

Organisation for Economic Cooperation and Development (OECD). 1997. *Evaluating economic instruments for environmental policy.* Paris, France.

——————— 2018. *Effective carbon rates 2018: Pricing carbon emissions through taxes and emissions trading.* Paris, France.

Parry, I. 2020. Increasing carbon pricing in the EU: Evaluating the options. *European Economic Review*, 121, 103341.

Pesaran, M.H. 2007. A simple panel unit root test in the presence of cross-section dependence. *Journal of Applied Econometrics*, *22*(2), 265-312.

Pesaran, M.H. 2015. Testing weak cross-sectional dependence in large panels. *Econometric Reviews*, *34*(6-10), 1089-1117.

Pesaran, M.H. and Smith, R. 1995. Estimating long-run relationships from dynamic heterogeneous panels. *Journal of Econometrics*, *68*(1), 79-113.

Pretis, F. 2019. Does a carbon tax reduce CO_2 emissions? Evidence from British Columbia. SSRN Working Paper. Manuscript Version: February 5, 2019.

Reese, S. and Westerlund, J. 2016. Panicca: Panic on Cross-Section Averages. *Journal of Applied Econometrics*, 31(6), 961-981.

Riahi, K., Van Vuuren, D.P., Kriegler, E., Edmonds, J., O'Neill, B.C., Fujimori, S., Bauer, N., Calvin, K., Dellink, R., Fricko, O. and Lutz, W., 2017. The shared socioeconomic pathways and their energy, land use, and greenhouse gas emissions implications: an overview. *Global Environmental Change*, 42, pp.153-168.

Rivers, N. and Schaufele, B. 2015. Salience of carbon taxes in the gasoline market. *Journal of Environmental Economics and Management*, 74, 23-36.

Rosenbloom, D., Markard, J., Geels, F.W. and Fuenfschilling, L. 2020*a*. Opinion: Why carbon pricing is not sufficient to mitigate climate change—and how "sustainability transition policy" can help. *Proceedings of the National Academy of Sciences*, *117*(16), pp. 8664-8668.

————— 2020*b*. Reply to van den Bergh and Botzen: A clash of paradigms over the role of carbon pricing. *Proceedings of the National Academy of Sciences*, *117*(38), 23221-23222.

Sengupta, S., Williams, J., and Chandrasekhar, A. 2019. How one billionaire could keep three countries hooked on coal for decades. *The New York Times*, August 15.

Sims, C. 1982. Policy analysis with econometric models. Brookings Papers on Economic Activity, 1, 107-152.

Sterner, T. 2007. Fuel taxes: an important instrument for climate policy. *Energy Policy*, *35*(3), 3194–3202.

Stern-Stiglitz High-Level Commission on Carbon Prices. 2017. *Report of the High-level Commission on Carbon Prices*. Carbon Pricing Leadership Coalition. Washington, DC: World Bank.

Stiglitz, J.E., 2019. Addressing climate change through price and non-price interventions. *European Economic Review*, 119, 594-612.

Stock, J. H. and Watson, M.W. 1988. Testing for common trends. *Journal of the American Statistical Association*, 83, 1097-1107.

——————— 2002. Forecasting Using Principal Components from a Large Number of Predictors. *Journal of the American Statistical Association*, 97, 1167-1179.

Stokes, L.C. 2020. *Short circuiting policy: Interest groups and the battle over clean energy and climate policy in the American States*. Oxford University Press.

Tinbergen, J. 1952. On the Theory of Economic Policy. North-Holland Publishing Company, Amsterdam.

United Nations Framework Convention on Climate Change (UNFCCC). 2018. Greenhouse gas inventory data, 1990-2016.

UNEP [United Nations Environment Programme]. 2019. Emissions Gap Report 2019.

van den Bergh, J. and Botzen, W. 2020. Low-carbon transition is improbable without carbon pricing. *Proceedings of the National Academy of Sciences*, 117(38), 23219-23220.

Webster, M., Paltsev, S., Reilly, J. 2008. Autonomous efficiency improvement or income elasticity of energy demand: does it matter? *Energy Economics*, 30, 2785–2798.

Westerlund, J. and Urbain, J.P. 2015. Cross-sectional averages versus principal components. *Journal of Econometrics*, 185, 372-377.

Westerlund, J. 2007. Testing for error correction in panel data. *Oxford Bulletin of Economics and Statistics*, 69, 709–748.

World Bank and Ecofys. 2015. *State and trends of carbon pricing 2015*. Washington, DC.

World Bank, Vivid Economics, and Ecofys. 2018. *State and trends of carbon pricing 2018*. Washington, DC.

World Bank. 2020. *State and trends of carbon pricing 2020*. Washington, DC.

Xu, Y. 2017. Generalized synthetic control method: Causal inference with interactive fixed effects models. *Political Analysis, 25*(1), 57-76.