

Education Spending and the Distributional Effects of Carbon Pricing

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October 2026

Abstract

Carbon pricing is the most widely adopted instrument for climate mitigation, yet its distributional consequences remain a major political obstacle to broader adoption. We examine how carbon pricing affects inequality and whether fiscal policy can mitigate its distributional costs. Using a staggered difference-in-differences design on a panel of 123 countries between 1996 and 2022, of which 40 adopt carbon pricing, we estimate its effects on inequality. We find that the adoption of carbon pricing increases the pre-redistribution (market) Gini coefficient by 2.5 points. Fiscal redistribution absorbs part of this effect, yet inequality still rises, with the post-redistribution (disposable) Gini increasing by 1.9 points. However, the average effect conceals large cross-country heterogeneity driven by fiscal priorities. Countries that allocate a higher share of public spending to education experience increases in market income inequality that are roughly 55% smaller, and disposable income increases roughly 47% smaller, than in countries that prioritise education less. These results point to education spending and carbon pricing as policy complements.

JEL codes: D31, H23, Q54, Q58, I28, C23.

Keywords: carbon pricing, income inequality, education expenditure, climate policy

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1 Introduction

Carbon pricing, implemented through emissions trading systems (ETS) or explicit carbon taxes, has become the policy instrument of choice for climate mitigation. By 2023, more than 70 jurisdictions had adopted some form of carbon pricing, covering roughly 23% of global emissions (World Bank, 2023a). Scientific and policy consensus strongly supports carbon pricing (High-Level Commission on Carbon Prices, 2017; IPCC, 2022; Eskander and Fankhauser, 2020). It aligns private incentives with social costs, generates revenues that can be recycled, and achieves emissions reductions at a lower cost than many regulatory approaches.

Despite this consensus, carbon pricing continues to face strong political resistance. In France, the *gilets jaunes* protests of 2018 and 2019 forced the suspension of a planned fuel tax increase. Australia repealed its carbon price in 2014 after only two years. British Columbia’s carbon tax has faced repeated threats of rollback. In each of these cases, the central political concern was not environmental effectiveness but distributional fairness. Policymakers and citizens feared that pricing carbon would disproportionately burden lower-income households and increase inequality, and building public support for carbon pricing is widely seen as contingent on addressing this concern (Carattini et al., 2019). Whether this concern is empirically justified, and whether policy design can mitigate these effects, is an important question at the intersection of climate policy and political economy.

These concerns have a strong theoretical foundation. Carbon-intensive goods such as energy, transport, and food account for a larger share of consumption among lower-income households (Flues and Thomas, 2015; Sterner, 2012). As a result, carbon pricing can be regressive through the expenditure channel in the absence of compensation. Carbon pricing also changes production structures by shifting labour demand away from energy-intensive sectors toward services and clean energy industries. These adjustments affect wage dispersion through the labour market channel, particularly when the workforce differs in skill composition. Finally, the ultimate distributional outcome depends on the fiscal channel, namely how governments recycle carbon revenues through taxes and transfers (Metcalf, 2019; Klenert et al., 2018).

Most evidence on the distributional effects of carbon pricing comes from ex ante simulation. A meta-analysis of 53 simulation studies finds that carbon pricing tends to be regressive in developed economies when redistribution is absent, and that revenue recycling can reverse this result: lump-sum dividends are progressive, whereas reductions in income taxes often favour higher-income households (Ohlendorf et al., 2021; Goulder et al., 2019; Metcalf, 2019). In lower-income economies, wealthier households often consume more energy-intensive goods, so carbon pricing can be progressive (Dorband et al., 2019). These models depend on structural assumptions and cannot be validated against observed policy

variation, and their focus on revenue recycling leaves open whether fiscal choices made before adoption shape the outcome. Ex post evidence is scarce. Känzig (2023) provides credible causal evidence for the European Union, showing that households at the lower end of the income distribution bear a disproportionate share of the burden of carbon price shocks, but within a single institutional setting. Sen et al. (2026) use staggered adoption across countries to show that carbon pricing reduces emissions, yet comparable cross-country evidence on distribution is lacking. The present study extends the ex post evidence to a global panel and asks whether government fiscal choices influence the size of the distributional cost.

The hypothesis examined here is that the priority governments assign to education spending moderates the inequality effects of carbon pricing, through a human capital buffer. Carbon pricing shifts labour demand away from carbon-intensive industry toward services, advanced technology and renewable energy, sectors that tend to demand relatively skilled labour. Technological change has long been shown to shift labour demand toward higher-skilled workers and to raise wage dispersion (Autor et al., 2003; Acemoglu and Restrepo, 2018), and the transition to cleaner production can itself become skill-biased (Acemoglu et al., 2012). When governments devote a large share of public spending to education, the workforce is better prepared to move across sectors, so workers displaced from carbon-intensive industries can transition into expanding sectors with smaller income losses. When education receives lower priority, structural adjustment may fall more heavily on less skilled workers, leading to unemployment spells or wage declines that increase inequality.

Figure 1 illustrates the empirical pattern that motivates this hypothesis. The figure plots the relationship between pre-treatment government education spending priority and the change in the market Gini around the adoption of carbon pricing. Countries that allocate a larger share of public expenditure to education tend to experience smaller increases in inequality following adoption. While this descriptive relationship does not establish causality, it suggests that fiscal priorities may play an important role in shaping the distributional consequences of climate policy.

Evaluating this hypothesis requires a research design that combines credible causal identification with wide cross-country variation in both carbon pricing adoption and fiscal priorities. The empirical analysis exploits the staggered adoption of carbon pricing across 40 countries between 1996 and 2022. The estimation relies on the interaction weighted estimator proposed by Sun and Abraham (2021), applied to a panel of 123 countries. This approach addresses the bias that arises in standard two-way fixed effects regressions when treatment effects vary across cohorts and time (Goodman-Bacon, 2021; de Chaisemartin and D’Haultfœuille, 2020). To evaluate the moderating role of education spending, two complementary strategies are employed. First, interaction specifications relate carbon pricing adoption to the pre-treatment education budget priority of each country. Second,

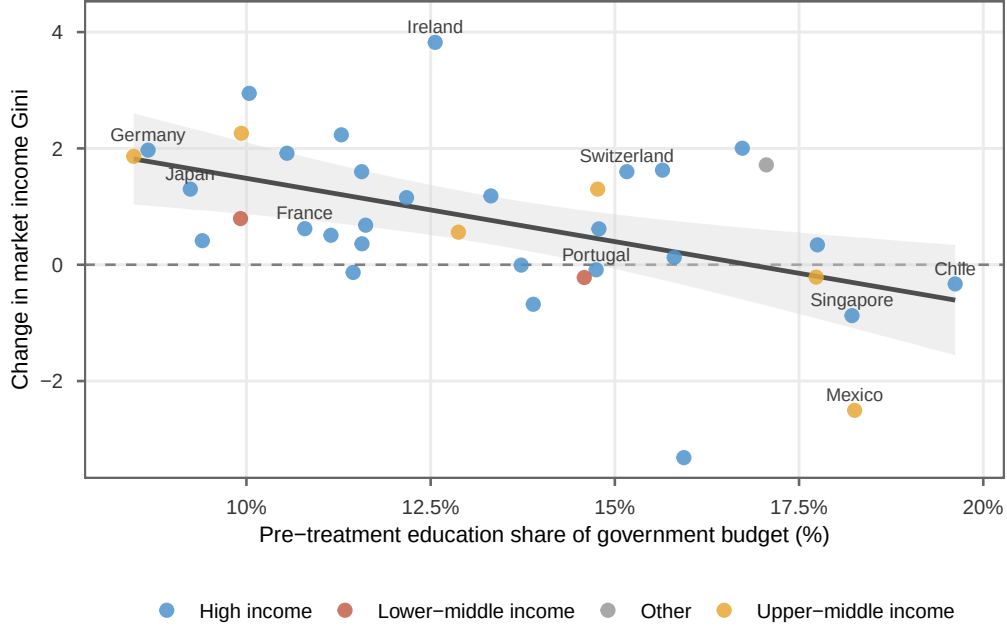


Figure 1: Education spending priority and inequality changes around carbon pricing adoption.

Note: Pre-treatment education spending (share of government expenditure, six-year average) against the change in the market Gini around adoption (five-year average after minus five-year average before), one point per treated country. The line is a linear fit with its 95% confidence band. Sources: SWIID 9.8; UNESCO; author's carbon pricing adoption compilation.

a nonparametric comparison of treatment effects is conducted between countries with higher and lower education spending priority.

The results show that carbon pricing increases the pre-redistribution market Gini coefficient by about 2.5 Gini points on average. Fiscal systems offset part of this effect, yet inequality still rises. The post-redistribution disposable Gini increases by approximately 1.9 points. The main policy result concerns the moderating role of education spending. Countries with higher pre-treatment education priority experience a substantially smaller increase in inequality. In these countries, the market Gini effect is about 1.28 points, whereas countries with lower education priority experience an increase of roughly 2.87 points. The difference corresponds to a reduction of more than one-half relative to the effect in low-priority countries. This pattern holds for both market and disposable income inequality and across the interaction and split-sample specifications; it is not detected for wealth outcomes.

This study contributes to the literature along three dimensions. It provides cross-country evidence on the realised distributional consequences of carbon pricing, covering both income and wealth inequality, and complementing recent global evidence on its environmental effectiveness and regional evidence on its distributional effects. To our knowledge, few studies have examined the effects of carbon pricing on wealth inequality using an ex post cross-country design, and this is among the first. It also quantifies the

role of existing fiscal redistribution, showing that tax and transfer systems absorb only part of the market income shock associated with carbon pricing. Most importantly, it shows that education budget priority is a policy-relevant factor that shapes the inequality costs of climate policy, suggesting that investment in human capital can ease labour market adjustment and reduce distributional tensions. This moderation is specific to income inequality: education priority does not moderate the effect of carbon pricing on wealth concentration, which points to a different channel.

The remainder of the paper proceeds as follows. Section 2 describes the data and outlines the empirical strategy. Section 3 presents the results: the baseline effect of carbon pricing on inequality, fiscal redistribution and wealth outcomes, and the moderating role of education spending. Section 4 investigates the mechanisms. Section 5 discusses the limitations and policy implications and concludes.

2 Empirical Analysis

2.1 Data and Measurement

The main inequality measures come from the Standardized World Income Inequality Database (SWIID v9.8, Solt, 2020), which harmonises information from the Luxembourg Income Study, Eurostat and national statistical agencies to produce comparable measures across countries and years (Atkinson and Brandolini, 2001). Two indicators are used. The market Gini measures inequality in income before taxes and transfers and reflects outcomes generated by labour and capital markets. The disposable Gini measures inequality after taxes and transfers, so the difference between the two captures the redistributive role of national fiscal systems.

Wealth outcomes are used as supplementary evidence in Section 3 and the Appendix. Mean net wealth per adult comes from the Credit Suisse Global Wealth Databook (Credit Suisse Research Institute, 2021), and the shares of wealth held by the top 1 percent, top 10 percent and bottom 50 percent come from the World Inequality Database (World Inequality Lab, 2023). Sample sizes are reported in Table 1.

Information on carbon pricing comes from the International Carbon Action Partnership Emissions Trading Worldwide database and the World Bank Carbon Pricing Dashboard (World Bank, 2023a; ICAP, 2023). Adoption is the first year in which an emissions trading system or an explicit carbon tax, at the national or subnational level, placed a meaningful price on carbon emissions. Appendix Figure A1 shows the cumulative number of adopting countries, with jumps after the introduction of the European Union Emissions Trading System in 2005 and after the Paris Agreement in 2016.

Countries that introduced carbon pricing before 1996 (Norway, Poland and Sweden) are excluded because they are treated throughout the estimation window. Countries

adopting after 2022 are classified as not yet treated, and countries that never adopt serve as the control group. The sample includes 42 treated countries with adoption years from 1999 to 2022, of which 40 enter the regressions (China and Uruguay lack control variables).

The moderating variable is government education priority, defined as education expenditure as a share of total government expenditure and obtained from the World Bank World Development Indicators. It measures how the budget is allocated, not the level of education spending or the education of the population; measures of spending as a share of gross domestic product, spending per student, enrolment and attainment do not moderate the effect in the same way (Appendix Table A6).

The controls are the logarithms of gross domestic product per capita, trade openness (exports plus imports relative to gross domestic product) and government consumption (capped at sixty percent of gross domestic product), and inflation winsorised at plus or minus fifty percentage points. For wealth outcomes, gross fixed capital formation replaces government consumption to capture investment dynamics. Unless otherwise noted, all variables come from the World Bank World Development Indicators (World Bank, 2023b).

The full dataset covers 234 countries between 1990 and 2022, and the estimation window runs from 1996 to 2022. Countries with missing observations for the outcome or any control are excluded from the relevant specification, so sample sizes differ across outcomes. For the market Gini, the estimation sample has 2,690 country-year observations for 123 countries, of which 40 adopt carbon pricing and 83 remain untreated (Table 1). The control group spans a wide range of income levels, although high-income economies are somewhat less represented among untreated countries; Section 3 and Appendix A3 examine this by restricting the control group to comparable income levels. Summary statistics are in Table A2.

2.2 Empirical Strategy

The starting point is the standard two-way fixed effects (TWFE) regression,

$$Y_{it} = \alpha_i + \lambda_t + \beta CP_{it} + X'_{it}\gamma + \varepsilon_{it}, \quad (1)$$

where Y_{it} is the inequality outcome for country i in year t , α_i and λ_t are country and year fixed effects, CP_{it} indicates that country i has adopted carbon pricing by year t , and X_{it} is a vector of time-varying controls. Standard errors are clustered by country. Under staggered adoption, however, TWFE implicitly compares some already-treated countries against each other and can assign negative weight to particular cohorts, which can bias the coefficient even when the true effect is positive everywhere (Goodman-Bacon, 2021). TWFE is therefore reported only as a descriptive benchmark, not as the main estimate.

The preferred estimates instead come from the staggered difference-in-differences estimator of Sun and Abraham (2021), implemented in R (R Core Team, 2024) via the

`sunab()` function in `fixest` (Bergé, 2021). Intuitively, this estimator computes a separate treatment effect for each adoption cohort using only not-yet-treated and never-treated countries as the comparison group, and then averages these cohort-specific effects into a single average treatment effect on the treated (ATT). Because it never compares one already-treated cohort to another, it avoids the bias that can affect TWFE under staggered adoption (see Roth et al., 2023 and Borusyak et al., 2024 for recent reviews of this fast-moving methodological literature). The same procedure also produces an event-study version of the estimate (the treatment effect by year relative to adoption), which we use to check for pre-trends: if carbon pricing is unrelated to prior inequality dynamics, the pre-adoption coefficients should be close to zero.

Identification rests on parallel trends: absent carbon pricing, inequality in treated and control countries would have evolved similarly, conditional on the fixed effects and controls. We assess this with the event-study pre-trends described above and with a timing-placebo test that assigns artificial adoption dates five years before the true ones. A further set of robustness checks (restricting the control group, excluding the 2005 EU ETS cohort, propensity-score matching, and an outcome placebo) is reported in Section 3 and detailed in the Appendix.

3 Results

3.1 Baseline effect on income inequality

Table 1 reports the baseline estimates. The two-way fixed-effects specification shows that the adoption of carbon pricing is associated with a statistically significant increase in both market and disposable income inequality. These estimates indicate that inequality rises following adoption, although the magnitude of the effect may be biased when treatment effects differ across adoption cohorts.

The Sun and Abraham estimates in Panel B are the preferred ones because they account for heterogeneous treatment timing. The results confirm that the introduction of carbon pricing increases inequality at the country level. The market Gini rises by roughly two and a half Gini points on average following adoption. Disposable income inequality also increases, although the magnitude is smaller, indicating that fiscal redistribution absorbs part of the market income shock.

Figure 2 presents the event study estimates for the market Gini (left panel) and the disposable Gini (right panel). In the market Gini, the pre-adoption coefficients are not flat: they are negative, at about one Gini point between ten and five years before adoption, and rise towards zero in the years just before adoption. Such patterns may reflect anticipation effects, where economic actors adjust behaviour before the policy takes effect, or selection in the timing of policy adoption. The quantitative importance of these

Table 1: Carbon Pricing Adoption and Inequality: Main Effects

	Income inequality (Gini points)		Wealth outcomes			
	(1) Market Gini	(2) Disposable Gini	(3) log(Mean wealth)	(4) Top 1% share	(5) Top 10% share	(6) Bottom 50% share
<i>Panel A: two-way fixed effects</i>						
Carbon pricing adoption	2.28*** (0.42)	1.53*** (0.40)	−0.093** (0.044)	0.016*** (0.005)	0.014*** (0.003)	−0.003*** (0.001)
<i>Panel B: Sun–Abraham ATT</i>						
Carbon pricing adoption	2.45*** (0.34)	1.90*** (0.38)	−0.159*** (0.046)	0.020*** (0.005)	0.013*** (0.005)	−0.004 (0.002)
Observations	2,690	2,690	2,200	3,166	3,166	3,166
Countries	123	123	111	129	129	129
Treated countries	40	40	44	44	44	44

Note: Panel A: two-way fixed effects (TWFE); Panel B: Sun–Abraham (2021) average treatment effect on the treated (ATT). Both with country and year fixed effects and standard errors clustered by country (Driscoll–Kraay for TWFE on wealth shares). Wealth shares are on a 0–1 scale. Income Gini: SWIID v9.8. Mean wealth: Credit Suisse Global Wealth Databook 2021. Wealth shares: World Inequality Database. Controls: log GDP per capita, log trade openness, winsorised inflation, log government consumption. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

dynamics is examined through the placebo tests reported below.

After adoption, inequality increases gradually and remains elevated relative to countries that do not adopt carbon pricing. The effect persists for several years following introduction, consistent with the idea that carbon pricing induces a gradual restructuring of production and labour demand rather than an immediate one-time change.

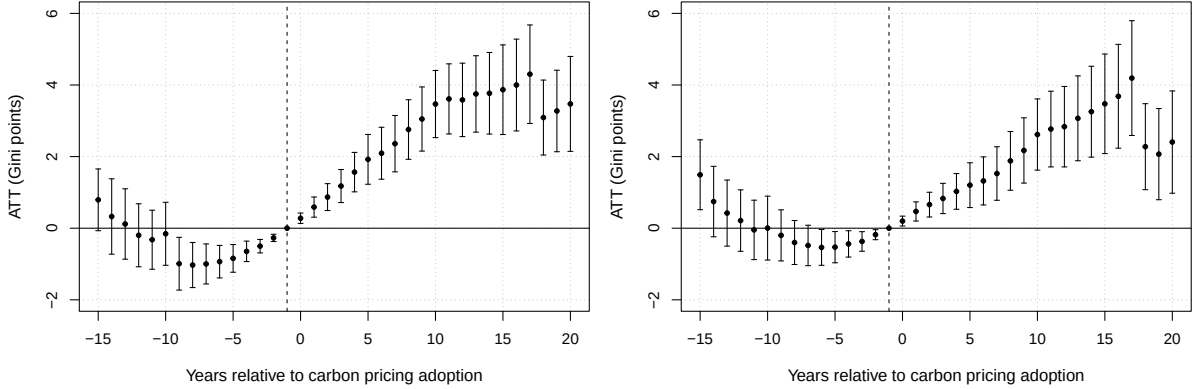


Figure 2: Carbon pricing adoption and income inequality: event study estimates.

Note: Sun and Abraham event studies for the market Gini (left) and the disposable Gini (right), with the specification of Table 1. Coefficients are relative to the year before adoption; bars are 95% confidence intervals clustered by country. Window: 15 years before to 20 years after adoption.

The baseline estimate is robust to a series of checks, detailed in Appendix A3 and summarised in Table A5. Restricting the never-treated group to income-comparable countries, excluding the 2005 EU ETS cohort (European Parliament and Council, 2003), and matching treated and control countries on pre-treatment observables via propensity

scores (Rosenbaum and Rubin, 1983) all leave the market Gini ATT statistically significant and of similar or larger magnitude than the baseline, indicating that the result is not an artefact of a particular control group or of the EU’s simultaneous adoption wave. An outcome placebo that substitutes female labour-force participation for the market Gini yields a null effect, consistent with the main result reflecting a real inequality response rather than a contemporaneous shock correlated with adoption timing. A timing placebo that shifts adoption dates five years earlier suggests that a modest share of the baseline ATT (roughly one-fifth) may be attributable to pre-existing trends, implying a causal lower bound still well above one Gini point.

3.2 Fiscal redistribution

An important question is whether fiscal redistribution offsets the market income shock associated with carbon pricing. The right panel of Figure 2 shows the same event study for the disposable Gini, and Table A4 in the Appendix reports the implied absorption rate. The overall pattern is similar across the two measures, with inequality rising following policy adoption. However, the increase in disposable income inequality is consistently smaller: taxes and transfers absorb roughly one-fifth of the market income effect but do not fully offset it. Fiscal systems therefore reduce the inequality consequences of carbon pricing, yet a large part of the increase in inequality remains visible in disposable incomes. This absorption is itself uneven: Table A4 shows that countries with higher pre-treatment education priority absorb more of the market-income shock through their fiscal systems (roughly 35%) than countries with lower priority (roughly 16%; this split uses a ten-year pre-adoption average and 20 and 18 treated countries, so it differs from the split in Table 2), so the education channel documented in Section 3.4 operates alongside, not instead of, a fiscal channel.

3.3 Wealth outcomes

Columns (3) to (6) of Table 1 report results for wealth outcomes. Carbon pricing is associated with a decline in average wealth levels and with a significant increase in wealth concentration at the top of the distribution: both the top 1 percent and top 10 percent wealth shares rise under TWFE and under the preferred Sun–Abraham specification. The bottom 50 percent wealth share falls under TWFE but the Sun–Abraham estimate for this outcome is not statistically significant. The patterns are broadly consistent with the income inequality results and suggest that the distributional consequences of carbon pricing extend beyond annual income flows.

Figure 3 shows the corresponding event studies for the top 1 percent and bottom 50 percent wealth shares. The top 1 percent share rises gradually after adoption, by about 3.8 percentage points fifteen years out and 4.3 after seventeen years, while the bottom 50

percent share falls by about 0.7 to 0.9 percentage points over the same horizon, although its confidence intervals include zero. The leads are not flat: the top 1 percent share is about 1.5 percentage points below its reference-year level ten years before adoption and converges to zero by five years before, so a rising trend in wealth concentration partly precedes adoption, as for the income Gini.

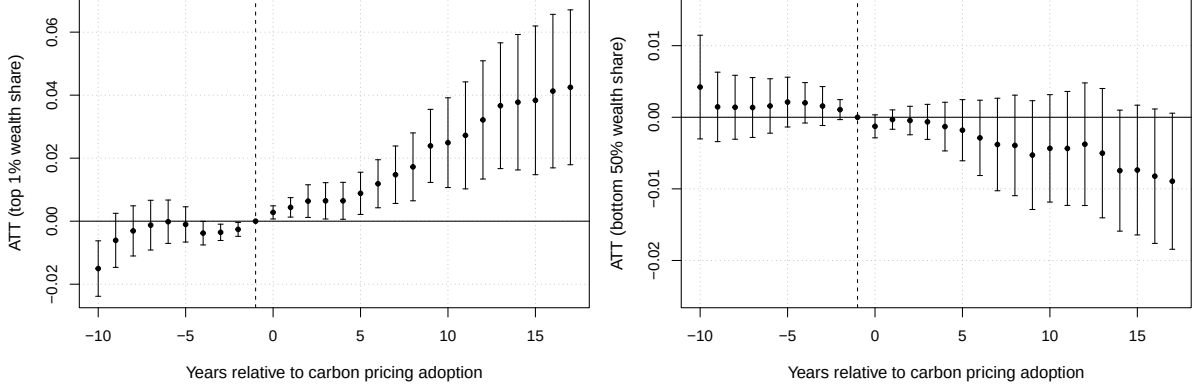


Figure 3: Carbon pricing adoption and wealth concentration: event study estimates.

Note: Sun and Abraham event studies for the top 1 percent (left) and bottom 50 percent (right) wealth shares, with the specification of columns (4) and (6) of Table 1; shares are on a 0 to 1 scale. Coefficients are relative to the year before adoption; bars are 95% confidence intervals clustered by country. Window: 10 years before to 17 years after adoption. Source: World Inequality Database.

3.4 Education priority and the inequality effect

This subsection presents the main result of the paper. The baseline effect documented above is an average across countries that differ in how well their workers can adjust to structural change. Public investment in education may reduce the inequality effect of carbon pricing by facilitating labour market adjustment: if carbon pricing shifts labour demand across sectors, the ability of workers to move between them may shape the size of the resulting rise in inequality. The analysis tests whether the priority governments assign to education spending moderates the inequality effects of carbon pricing.

To evaluate this hypothesis, the analysis uses two complementary empirical approaches. The first approach augments the baseline two-way fixed effects specification with an interaction between carbon pricing and a pre-treatment measure of government education budget priority:

$$Y_{it} = \alpha_i + \lambda_t + \beta_1 CP_{it} + \beta_2 (CP_{it} \times \overline{M}_i) + X'_{it}\gamma + \varepsilon_{it}, \quad (2)$$

where $\overline{M}_i$ denotes the country's pre-treatment average of education spending as a share of total government expenditure, standardised to mean zero and unit standard deviation. The coefficient β_2 captures how the inequality effect of carbon pricing varies with the education priority assigned by governments before policy adoption.

The second approach implements a non-parametric comparison by splitting treated countries at the pre-treatment median of education spending priority. Separate Sun-Abraham staggered difference-in-differences estimations are then conducted for each group. This split-sample strategy avoids functional form assumptions embedded in interaction models and allows the analysis to compare the average treatment effect across countries with relatively higher and lower education priorities.

Table 2 reports the moderation results. Panel A presents the interaction estimates from the two-way fixed effects specification. The interaction between carbon pricing and education spending priority is negative and statistically significant for both measures of income inequality. This pattern indicates that countries that devote a larger share of their public budget to education experience smaller increases in income inequality following the introduction of carbon pricing.

Table 2: Education Spending and the Inequality Effects of Carbon Pricing

	Income inequality		Wealth outcomes		
	(1) Market Gini	(2) Disposable Gini	(3) log(Mean wealth)	(4) Top 1% share	(5) Top 10% share
<i>Panel A: two-way fixed effects with education interaction</i>					
CP	1.77*** (0.44)	1.31*** (0.39)	−0.089* (0.047)	0.016*** (0.006)	0.013*** (0.004)
CP × Education priority	−1.92*** (0.53)	−1.35*** (0.50)	0.003 (0.047)	0.003 (0.003)	0.003 (0.004)
Observations	1,972	1,972	1,949	2,247	2,247
Countries	110	110	98	115	115
Treated countries	32	32	34	34	34
<i>Panel B: Sun-Abraham ATT by education priority (16 high, 16 low treated countries)</i>					
<i>Full post-adoption period</i>					
High education priority	1.28*** (0.33)	1.12*** (0.35)	n.a.	n.a.	n.a.
Low education priority	2.87*** (0.38)	2.11*** (0.42)	n.a.	n.a.	n.a.
Difference (High − Low)	−1.59*** (0.50)	−0.99* (0.55)	n.a.	n.a.	n.a.
<i>First five years after adoption (event time 0 to 4)</i>					
High education priority	0.43*** (0.11)	0.35*** (0.10)	n.a.	n.a.	n.a.
Low education priority	1.14*** (0.21)	0.76*** (0.19)	n.a.	n.a.	n.a.
Difference (High − Low)	−0.71*** (0.24)	−0.41* (0.22)	n.a.	n.a.	n.a.
Observations (high / low)	1,636 / 1,625	1,636 / 1,625	n.a.	n.a.	n.a.
Treated countries (high / low)	16 / 16	16 / 16	n.a.	n.a.	n.a.

Note: Panel A: two-way fixed effects (country and year). *CP* is the carbon pricing coefficient and *CP* × *Education priority* the interaction with pre-treatment education spending (share of government expenditure, standardised). Panel B: Sun-Abraham estimates for samples split at the median education priority among treated countries; the first-five-years block restricts every treated country to event times 0 to 4; Observations, countries and treated countries are those used by each Panel A regression after dropping observations with missing controls and singleton country-years. Panel B counts refer to the full-period split samples (observations and treated countries in the high and low groups). Panel B is estimated for the two Gini outcomes only (n.a. = not estimated). Controls as in Table 1. Standard errors clustered by country (Driscoll-Kraay for wealth shares). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The wealth outcomes behave differently. The interaction is close to zero for the top 1 and top 10 percent wealth shares, and the 95 percent confidence intervals exclude an attenuation larger than about a quarter of the baseline effect, whereas for mean wealth the interval is too wide to be informative. This is consistent with the human capital mechanism: education buffers labour market earnings, while wealth concentration at the top depends on capital ownership, which education spending does not directly affect. The moderating role of education is specific to income inequality.

Panel B reports the split-sample Sun–Abraham estimates, which constitute the preferred moderation test. Countries with higher education budget priority experience a much smaller increase in market income inequality following carbon pricing adoption than countries with lower education priority. The difference between the two groups corresponds to a reduction of more than half relative to the effect in low-priority countries. A similar pattern appears for disposable income inequality, although the magnitude is smaller. The difference is not an artefact of high-education countries having adopted later and therefore having been exposed for less time: over the first five years after adoption, the market Gini effect is 0.43 in high-education and 1.14 in low-education countries. The consistency between the market and disposable Gini results suggests that the moderating effect operates primarily through labour market outcomes rather than through higher fiscal redistribution in high-education countries.

The estimates in this subsection use different samples from the baseline of Section 3.1, which is the Sun–Abraham estimate for 1996 to 2022 and its 40 treated countries (2.45 Gini points). The interaction estimates in Panel A are two-way fixed effects for 2000 to 2020 and the treated countries with education data (32 to 34 depending on the outcome), so their baseline is lower (1.77 Gini points). The split-sample estimates in Panel B use the Sun–Abraham estimator on the same window, with 16 treated countries in each group.

These results show that education budget priority attenuates the inequality effect of carbon pricing. The moderating effect appears for both market and disposable income inequality, which indicates that it operates primarily through labour markets rather than through fiscal redistribution. Measures of the level of education spending (as a share of gross domestic product or per student) and of the stock of education (enrolment and attainment) do not moderate the effect, and the budget share keeps its coefficient when they are added (Appendix Table A6); upper-secondary attainment, if anything, amplifies it. The relevant factor is not the level of education spending or the education of the population, but the priority governments assign to education within their fiscal budgets. The moderation is also unchanged when controlling for institutional quality and does not differ between countries with high and low institutional quality (Appendix Table A8). Appendix A4 collects the checks on whether the moderation reflects other country traits, timing, pre-trends or weak carbon pricing. The next section examines the channels through which carbon pricing raises inequality and why education may matter for them.

4 Mechanisms

The previous section shows that education budget priority attenuates the inequality effect of carbon pricing. This section examines the channels through which carbon pricing raises market income inequality, and why education may matter. The analysis investigates whether carbon pricing operates through labour market restructuring. If carbon pricing induces sectoral reallocation across industries with different wage structures, the ability of workers to adjust to these changes may influence the magnitude of the resulting inequality effects. The mediation tests below identify the channels of the carbon pricing effect, and a further test then asks whether education priority changes how those channels operate.

To evaluate these channels, the analysis applies a mediation framework based on the approach proposed by Baron and Kenny (1986). For each potential mediator, three steps are estimated. First, the relationship between carbon pricing and the mediator is examined. Second, the association between the mediator and inequality is estimated while controlling for the baseline specification. Third, the direct effect of carbon pricing on inequality is re-estimated after including the mediator. The reduction in the carbon pricing coefficient provides an indication of the share of the overall effect that operates through the candidate channel.

Table 3 reports the results for market income inequality. Two mechanisms receive consistent empirical support at both stages of the mediation test. Carbon pricing is associated with a decline in the share of employment in industrial sectors, and lower industrial employment is in turn associated with higher income inequality, consistent with part of the effect reflecting a shift away from manufacturing and other carbon-intensive activities. Carbon pricing is also associated with an increase in the renewable energy share of the energy mix, and a higher renewable energy share is positively associated with inequality, consistent with a skill-biased expansion of the renewable energy sector. Services employment rises following adoption as well, and a higher services employment share is positively associated with inequality, though the first-stage link between carbon pricing and services employment is less precisely estimated.

These results point to a structural-transformation explanation: carbon pricing reduces the relative importance of carbon-intensive industrial activities while expanding the renewable energy sector, and the expansion of renewable energy is itself associated with higher inequality. This is consistent with a skill-biased pattern of green growth, in which the sectors that expand under carbon pricing reward higher-skilled workers more than the industrial sectors they displace (Autor et al., 2003; Acemoglu and Restrepo, 2018).

Additional mechanism tests examine wealth outcomes. Evidence reported in Appendix Table A3 suggests that part of the decline in average wealth associated with carbon pricing is linked to lower investment in physical capital. This is consistent with the broader structural adjustments observed in the labour market analysis.

Table 3: Mechanism Tests for Market Income Gini: Baron–Kenny Three-Step

Mediator	Step 1	Step 2
Industrial employment (%)	−1.66*** (0.40)	−0.135*** (0.048)
Renewable energy share (%)	4.98*** (0.83)	0.064** (0.031)
Services employment (%)	0.54 (0.57)	0.073* (0.041)
Labour income share (%)	−1.41* (0.76)	0.004 (0.028)
Union density (%)	0.42 (0.87)	−0.024 (0.037)

Note: Three-step Baron–Kenny mediation with country and year fixed effects. Step 1: mediator on carbon pricing (CP). Step 2: Gini on the mediator, without carbon pricing. Controls: log GDP per capita, log trade openness, winsorised inflation, log government consumption. Sample (country-years, countries, treated countries): industrial employment 2,171, 119, 42; renewable energy share 2,213, 122, 42; services employment 2,171, 119, 42; labour income share 1,792, 119, 42; union density 1,059, 88, 41. Standard errors clustered by country. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

To link the channels to the education result, Appendix Table A14 interacts education priority with each channel at both stages of the mediation. Education priority does not significantly change how much carbon pricing moves industrial or services employment, although the point estimate for industrial employment points to a smaller decline in high-education countries, and renewable energy expands somewhat less where education priority is high. The clearer difference lies in the second stage: education priority weakens the pass-through from each of the three sectoral channels to inequality. The association between industrial employment and the market Gini is -0.132 at average education priority and about $+0.07$ one standard deviation above it. The association with the renewable share falls from $+0.073$ to about zero, and the association with the services share turns from $+0.081$ to -0.03 . The labour income share shows no such pattern. Controlling for a channel and its interaction with education priority reduces the carbon pricing $\times$ education coefficient on the market Gini by 24 percent for industrial employment, 19 percent for services and 13 percent for renewables, so the sectoral channels account for a sizeable part of the education effect but not for most of it.

Read together with the moderation results, these channels are consistent with the interpretation that education investment acts as a human capital buffer when carbon pricing alters the structure of production. As carbon pricing raises the cost of energy-intensive production, labour demand shifts away from heavy industry toward services and cleaner sectors. In economies with higher levels of human capital, workers displaced from carbon-intensive industries may possess skills that facilitate re-employment in expanding sectors, limiting increases in wage dispersion. In economies where human capital investment has been lower, the adjustment process may involve larger wage losses or transitions into lower-paid employment, increasing inequality. The channels identified above, declining

industrial employment and expanding renewable and services activity, correspond directly to the types of sectoral reallocation that human capital investment can help workers manage, and the tests in Appendix Table A14 show that these channels transmit less inequality where education priority is high.

5 Discussion and Conclusion

This paper examines the distributional consequences of carbon pricing and the fiscal conditions that shape their magnitude. Using a staggered difference-in-differences design with the Sun and Abraham estimator, applied to a panel of 123 countries between 1996 and 2022, it shows that carbon pricing raises the market Gini by about 2.5 points on average (about 2.0 points after accounting for pre-treatment dynamics), a large change relative to the interquartile range of about eight Gini points across countries in the sample. Taxes and transfers offset roughly one-fifth of the increase, leaving about 1.9 points in the disposable Gini. Education spending priority attenuates the increase by roughly 55 percent for the market Gini and 47 percent for the disposable Gini, but not for wealth outcomes (Section 3.4).

The analysis has three main limitations. First, the timing of adoption may reflect unobserved political or economic factors that also influence inequality trends. Placebo tests indicate that pre-treatment dynamics account for a modest share of the estimated effect, but without a credible instrument for adoption timing the estimates rely on conditional parallel trends. The education moderation is more exposed to this concern: treated countries with low education priority show an upward pre-adoption trend in the Gini relative to never-treated countries, and the interaction weakens and loses significance when treated-specific or country-specific trends are added (Appendix Table A10). Second, carbon pricing instruments differ widely in sectoral coverage, price level and enforcement, so the estimated effect is an average across heterogeneous designs. Adopting countries also tend to be wealthier and to have stronger institutions than those that never adopt, which limits external validity for lower-income economies. Third, wealth shares from the World Inequality Database are partly modelled for many countries, and the event studies for top wealth shares show a rising trend before adoption, so the wealth results should be read as evidence of association rather than as precisely identified effects.

Two policy implications follow. First, the data are consistent with the concern that carbon pricing raises inequality, and the increase does not appear limited to a specific institutional context, so ignoring it risks the political resistance and policy reversals seen in episodes such as the *gilets jaunes* protests in France. Second, the size of the distributional cost is not fixed: it depends in part on fiscal choices that governments make before or alongside adoption. Countries that give education higher priority appear better able to absorb the associated labour market adjustments, so investment in education can work as

a complement to carbon pricing, helping to limit the rise in inequality while emissions fall.

The complementarity is specific to income. Education priority does not moderate the effect on wealth concentration, which is consistent with a labour-market mechanism, so limiting the rise in top wealth shares would call for other instruments, such as capital taxation or broader asset ownership. Governments that pair carbon pricing with sustained investment in education and workforce development may therefore face a smaller equity trade-off. Extending the analysis to sub-national policy variation and to alternative measures of human capital investment is left for future work.

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A1 Treatment Cohorts

Table A1: Carbon-Pricing Adoption Cohorts (1996–2022)

Year	Countries	Instrument type
1999	Finland, Slovenia	ETS and tax
2000	Estonia	ETS and tax
2004	Latvia	ETS and tax
2005	Austria, Belgium, Bulgaria, Croatia, Cyprus, Denmark, France, Germany, Greece, Hungary, Ireland, Italy, Lithuania, Luxembourg, Malta, Netherlands, Portugal, Romania, Spain, United Kingdom	ETS; ETS and tax
2007	Canada	ETS and tax
2008	New Zealand, Switzerland	ETS; ETS and tax
2009	United States	ETS
2010	Iceland, Japan	Carbon tax; ETS and tax
2012	Australia	ETS
2013	China	ETS
2015	Mexico, South Korea	ETS; ETS and tax
2016	Kazakhstan, Ukraine	Carbon tax; ETS
2017	Chile, Colombia	Carbon tax
2018	Argentina	Carbon tax
2019	Singapore	Carbon tax
2020	South Africa	Carbon tax
2022	Uruguay	Carbon tax

Note: Cohorts used in the Sun–Abraham estimation. Countries adopting before 1996 (Norway, Poland, Sweden) are excluded; countries first adopting after 2022 are coded as not-yet-treated. China (2013) and Uruguay (2022) are in the sample but drop out of the regressions because their control variables are missing. Instrument type as recorded in the adoption file. Sources: ICAP (2023); World Bank Carbon Pricing Dashboard (2023).

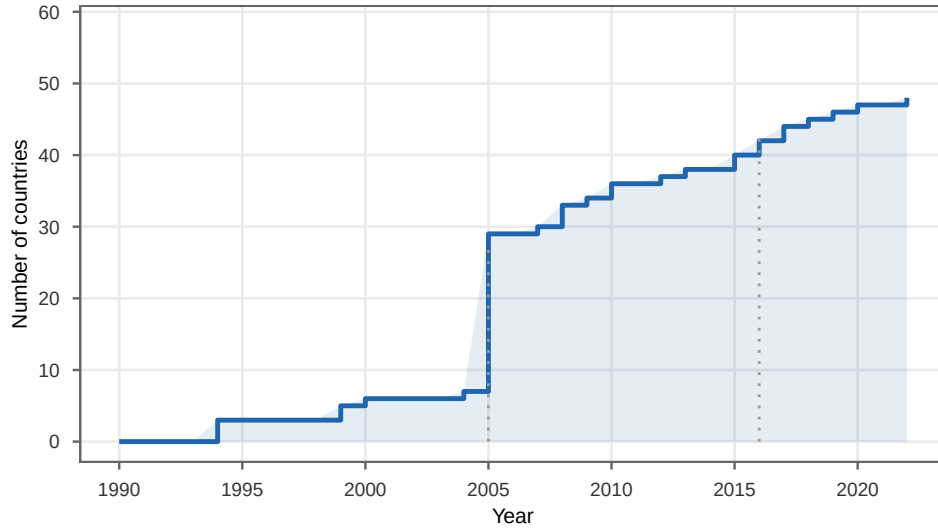


Figure A1: Cumulative number of countries with carbon pricing.

Note: Cumulative number of countries with a carbon tax or emissions trading system in force. Dotted lines mark 2005 (EU ETS launch) and 2016 (Paris Agreement). Source: author's carbon pricing adoption compilation.

A2 Additional Tables

Table A2: Descriptive Statistics

	Full sample				
	<i>N</i>	Mean	SD	Min	Max
Market income Gini	3,650	45.78	6.21	30.30	72.50
Disposable income Gini	3,650	38.59	8.19	22.10	65.20
Education spending (% gov. budget)	3,198	14.48	4.88	2.20	44.80
GDP per capita (thousand USD)	5,194	15.47	22.27	0.22	213.94
Trade openness (% GDP)	4,526	90.07	57.60	2.70	863.20
Inflation rate (%)	5,203	9.58	81.39	−31.57	4800.53
Government consumption (% GDP)	3,761	17.23	8.04	1.86	60.00
Renewable energy share (% total)	5,155	28.91	29.13	0.00	98.30
Carbon pricing active (0/1)	6,886	0.11	0.31	0.00	1.00

Note: Country-year observations, 1996–2022. The inflation rate is shown before winsorising; the regressions winsorise it at ± 50 percentage points.

Table A3: Mechanism Tests for Wealth Outcomes: Baron–Kenny Three-Step

Mediator	Step 1	Step 2
<i>Panel A: Outcome = $\log(\text{mean net wealth/adult})$</i>		
Industrial employment (%)	−2.94*** (0.56)	0.015** (0.006)
Renewable energy share (%)	5.81*** (0.82)	−0.010*** (0.003)
$\log(\text{Gross Fixed Capital Formation})$	−0.25*** (0.05)	0.212*** (0.067)
Services employment (%)	1.49** (0.63)	−0.003 (0.006)
<i>Panel B: Outcome = Top 1% wealth share</i>		
Industrial employment (%)	−2.81*** (0.72)	−0.0017*** (0.0002)
Renewable energy share (%)	6.01*** (1.37)	0.0006*** (0.0001)
$\log(\text{Gross Fixed Capital Formation})$	−0.26*** (0.06)	−0.0212*** (0.0019)
Services employment (%)	1.36*** (0.44)	0.0001 (0.0003)

Note: Three-step Baron–Kenny mediation with country and year fixed effects. Panel A: Credit Suisse mean wealth. Panel B: World Inequality Database top 1% share. Controls: log GDP per capita and log trade openness (inflation and government consumption are not included). Sample (country-years, countries, treated countries). Panel A: industrial employment 2,542, 126, 45; renewable energy share 2,558, 127, 45; log gross fixed capital formation 2,202, 111, 44; services employment 2,542, 126, 45. Panel B: industrial employment 2,779, 138, 45; renewable energy share 2,882, 145, 45; log gross fixed capital formation 2,470, 125, 44; services employment 2,779, 138, 45. Standard errors clustered by country (Panel A) and Driscoll–Kraay with lag 3 (Panel B). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A4: Fiscal Redistribution and Carbon Pricing: Market versus Disposable Gini

	Market Gini	Disposable Gini	Absorbed	% absorbed	<i>N</i>
Full sample	2.45*** (0.34)	1.90*** (0.38)	0.55 (0.51)	22.4%	2,690
High education priority	1.69*** (0.38)	1.10*** (0.36)	0.60 (0.53)	35.2%	2,124
Low education priority	3.02*** (0.41)	2.53*** (0.46)	0.48 (0.62)	16.1%	2,091
Country FE			Yes		
Year FE			Yes		
Controls			Yes		

Note: Sun–Abraham ATTs. Market Gini is pre-redistribution and disposable Gini post-tax and transfer. *Absorbed* = market ATT – disposable ATT (Gini points offset by the fiscal system); % *absorbed* = absorbed / market ATT $\times$ 100. Education priority is split at the median of the ten-year pre-adoption average education share (12.86%), which gives 20 high- and 18 low-priority treated countries in these regressions. *N* is the number of observations each model uses. Table 2, Panel B, instead splits on the 2000–2020 average and has 16 treated countries in each group, so the two splits differ in definition and sample. Standard errors clustered by country. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A3 Robustness Checks

Table A5: Robustness of the Baseline Effect: Sun–Abraham ATT

	(1) Baseline	(2) Restricted controls	(3) Non-EU cohorts	(4) Matched controls	(5) Placebo outcome	(6) Placebo dates
Outcome	Market Gini	Market Gini	Market Gini	Market Gini	Female LFPR	Market Gini
Adoption dates	True dates	True dates	True dates	True dates	True dates	5 years earlier
Carbon pricing adoption	2.45*** (0.34)	2.76*** (0.53)	1.74*** (0.32)	2.88*** (0.64)	0.74 (1.09)	0.49*** (0.14)
Observations	2,690	1,694	2,160	1,533	2,396	2,091
Treated countries	40	40	20	39	41	36
Control countries	83	33	83	22	103	83
Country FE				Yes		
Year FE				Yes		
Controls				Yes		

Note: Sun–Abraham (2021) ATT with country and year fixed effects and the controls of Table 1; standard errors clustered by country. (2) Never-treated countries restricted to high- and upper-middle-income economies, all treated countries retained. (3) The 2005 EU ETS cohort (20 countries) dropped; the remaining treated countries are non-EU adopters spanning 1999–2022. (4) Never-treated countries matched 1:3 on a propensity score (logit on pre-treatment Gini, log GDP, log trade; 1996–2004). (5) True adoption dates with the female labour-force participation rate (ILO modelled) as outcome. (6) Adoption dates shifted back five years; treated-country data restricted to years before actual adoption. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

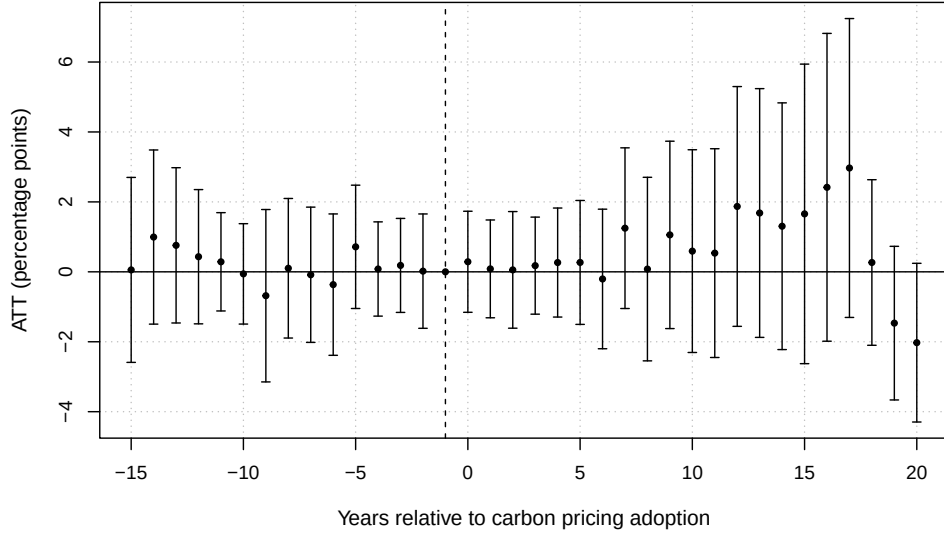


Figure A2: Outcome placebo: female labour-force participation rate.

Note: Sun and Abraham event study with female labour-force participation (ILO modelled, percentage points) as the outcome and the true adoption dates. Coefficients are relative to the year before adoption; bars are 95% confidence intervals clustered by country. Window: 15 years before to 20 years after adoption. Overall ATT = +0.74 (SE = 1.09).

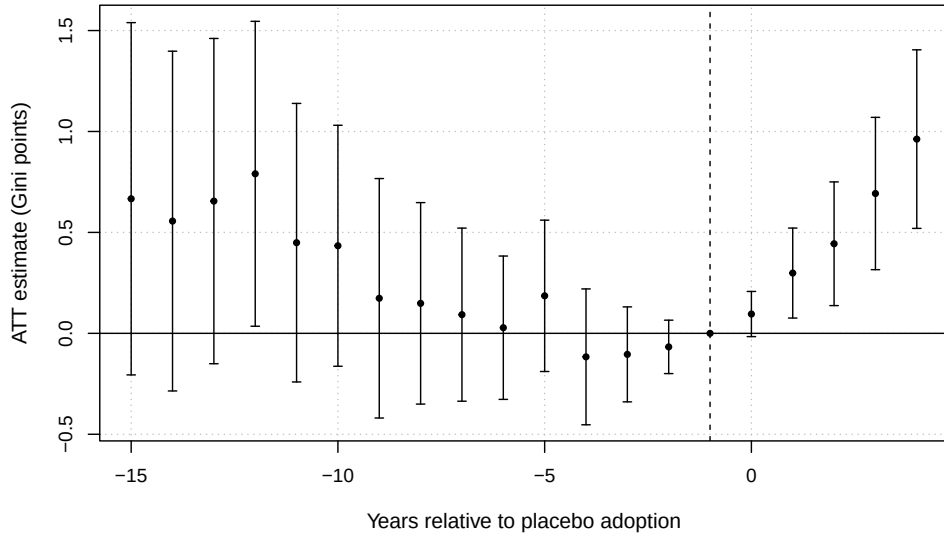


Figure A3: Timing placebo: treatment date shifted back five years.

Note: Sun and Abraham event study for the market Gini with adoption dates shifted back five years, using only years before the true adoption date. The horizontal axis is relative to the placebo date, and coefficients are relative to the year before it; bars are 95% confidence intervals clustered by country. Window: 15 years before onward. Overall ATT = +0.495 (SE = 0.141).

A4 Is the Moderation Really About Education?

A4.1 Is it education measured badly?

Table A6: Alternative Education Measures as Moderators (Market Gini)

Alternative measure Z	(1) Alone	(2) Together with budget share		Countries (treated)
	CP $\times$ Z	CP $\times$ Z	CP $\times$ Budget share	
Education % GDP (self-computed)	−1.95* (1.16)	−0.66 (1.38)	−1.69*** (0.54)	110 (32)
Education spending, % GDP	−0.65 (0.92)	−0.05 (0.70)	−1.91*** (0.46)	110 (32)
Spending per student (log)	0.36 (0.74)	0.20 (0.59)	−1.94*** (0.54)	95 (32)
Secondary enrolment	−0.12 (0.56)	−0.20 (0.40)	−1.93*** (0.53)	108 (32)
Tertiary enrolment	−0.14 (0.48)	0.05 (0.32)	−1.92*** (0.52)	106 (32)
Attainment, upper secondary	1.04* (0.56)	1.06** (0.42)	−1.93*** (0.47)	109 (32)
Attainment, post-secondary	0.25 (0.45)	0.76** (0.35)	−2.11*** (0.50)	100 (31)

Note: Each row re-estimates the interaction of Table 2 with an alternative pre-treatment education measure Z (standardised), first alone (column 1) and then together with the budget share (columns 2 and 3). Education % GDP (self-computed) is the budget share times government consumption as a share of GDP. Attainment is the share of adults with at least upper-secondary or post-secondary education. Country and year fixed effects, controls as in Table 2, standard errors clustered by country. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A4.2 Is it a proxy for other country traits?

Table A7: Is the Moderation a Proxy for Other Country Traits?

Rival added	Market Gini			Disposable Gini			N
	Edu alone	Edu with rival	Rival	Edu alone	Edu with rival	Rival	
Income level	−1.92*** (0.53)	−1.89*** (0.54)	0.55 (0.51)	−1.35*** (0.50)	−1.35*** (0.51)	0.04 (0.45)	1,972
State size	−1.92*** (0.53)	−2.07*** (0.71)	−0.50 (0.94)	−1.35*** (0.50)	−1.32* (0.67)	0.12 (0.89)	1,972
Carbon intensity	−1.92*** (0.53)	−1.91*** (0.54)	−0.45** (0.19)	−1.35*** (0.50)	−1.35*** (0.51)	−0.08 (0.19)	1,972
Income, state size, carbon intensity	−1.92*** (0.53)	−2.09*** (0.75)	—	−1.35*** (0.50)	−1.33* (0.68)	—	1,972
Active labour market programmes	−1.88*** (0.63)	−1.64*** (0.55)	0.44 (0.26)	−1.34** (0.57)	−0.95* (0.47)	0.70*** (0.24)	587
Institutional quality (composite)	−1.92*** (0.53)	−1.87*** (0.54)	0.53 (0.41)	−1.35*** (0.50)	−1.32*** (0.50)	0.31 (0.37)	1,972
All six governance indicators	−1.92*** (0.53)	−2.58*** (0.64)	—	−1.35*** (0.50)	−1.79** (0.69)	—	1,972
Social protection coverage	−1.92*** (0.53)	−1.39*** (0.50)	1.41*** (0.51)	−1.35*** (0.50)	−0.92* (0.47)	1.16** (0.53)	1,972
Military budget share	−1.57*** (0.47)	−1.68*** (0.52)	0.37 (0.50)	−1.08** (0.49)	−1.12* (0.60)	0.13 (0.49)	1,804
Health budget share	−1.92*** (0.53)	−1.94*** (0.56)	−0.21 (0.58)	−1.35*** (0.50)	−1.41*** (0.53)	−0.59 (0.44)	1,972
Other budget share	−1.57*** (0.47)	−1.80*** (0.67)	−0.29 (0.48)	−1.08** (0.49)	−0.95 (0.77)	0.16 (0.44)	1,804
Fossil-fuel subsidies	−1.92*** (0.53)	−1.86*** (0.55)	−0.63*** (0.23)	−1.35*** (0.50)	−1.33** (0.52)	−0.18 (0.19)	1,944
Four energy-structure variables	−1.93*** (0.55)	−1.32** (0.56)	—	−1.38*** (0.52)	−1.21** (0.52)	—	1,632
Budget execution	−1.80*** (0.48)	−1.82*** (0.37)	−0.83*** (0.24)	−1.12* (0.57)	−1.13** (0.47)	−0.97*** (0.22)	1,836
Effective carbon rate	−1.92*** (0.53)	−1.76*** (0.65)	0.31 (0.44)	−1.35*** (0.50)	−1.17* (0.59)	0.36 (0.35)	1,972

Note: Each row re-estimates the education interaction of Table 2 (Panel A) after adding the carbon pricing interaction with one rival country trait, standardised across countries; “alone” is the education interaction on the same sample without the rival. Two-way fixed effects (country and year), 2000–2020, controls as in Table 2, standard errors clustered by country. Institutions, government-budget shares and energy structure are averages over the pre-adoption years; budget execution is the 2004–2020 average; social protection (2000 onward), fossil-fuel subsidies (2015 onward) and effective carbon rates (2018–2023) are averages over the years available, so they describe current levels, not the situation at adoption. Columns: coefficient on CP × Education without the rival (Edu alone), with the rival (Edu with rival), and on CP × rival (Rival). The samples differ across rows with data availability (*N* is for the market Gini). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A8: Institutional Quality and the Education Moderation

	Market Gini	Disposable Gini
<i>Panel A: CP × Education with institutions added</i>		
Baseline	−1.92*** (0.53)	−1.35*** (0.50)
Adding CP × institutional composite	−1.87*** (0.54)	−1.32*** (0.50)
CP × institutional composite	0.53 (0.41)	0.31 (0.37)
Adding CP × government effectiveness	−1.89*** (0.53)	−1.34*** (0.49)
CP × government effectiveness	0.44 (0.33)	0.25 (0.30)
Adding all six governance dimensions	−2.58*** (0.64)	−1.79** (0.69)
<i>Panel B: Does the moderation differ by institutional quality?</i>		
CP × Education, low-institution countries	−1.86*** (0.62)	−1.11 (0.82)
CP × Education, high-institution countries	−1.96** (0.81)	−1.55** (0.61)
Difference (three-way interaction)	−0.09 (1.01)	−0.44 (1.00)
Three-way interaction, continuous composite	0.19 (0.60)	0.19 (0.57)
<i>Panel C: Education net of institutional quality</i>		
CP × Education net of institutions	−1.65*** (0.50)	−1.17** (0.47)
Sun–Abraham ATT, high (net-of-institutions split)	1.28 (0.33)	1.12 (0.35)
Sun–Abraham ATT, low (net-of-institutions split)	2.87 (0.38)	2.11 (0.42)
Difference (high – low)	−1.59*** (0.50)	−0.99* (0.55)
Observations	1,972	1,972
Countries	110	110
Treated countries	32	32

Note: Institutional quality is the pre-adoption average (1996 onward; full window for never-treated countries) of the six World Bank Worldwide Governance Indicators (Kaufmann et al., 2011), standardised; the composite is their mean. Panels A and B: two-way fixed effects (country and year), 2000–2020, controls as in Table 2; the CP × Education coefficient (per standard deviation of the education budget share) is reported, and high and low institutions are split at the median among treated countries. Panel C: education net of institutions is the residual of the education budget share on the composite (institutions explain 1.3 percent of its variation; correlation with the raw share 0.99), and the split is the Sun–Abraham median split of Table 2, Panel B. Standard errors clustered by country in parentheses. Observations, countries and treated countries refer to the baseline interaction sample of Panel A, which is also the sample of the two-way rows in Panels B and C. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A4.3 Is it an artefact of timing, the sample or pre-trends?

Table A9: Is the Moderation an Artefact of Timing or of the Sample? Education Gap by Subsample

	Market Gini		Disposable Gini	
	(1) Group 1	(2) Group 2	(3) Group 1	(4) Group 2
All treated countries, full period (Table 2, Panel B)	−1.59*** (0.50)		−0.99* (0.55)	
All treated countries, first five years after adoption	−0.71*** (0.24)		−0.41* (0.22)	
Adoption timing (early late)	−1.25** (0.61)	−1.18*** (0.29)	−0.51 (0.67)	−0.39 (0.29)
treated countries (high/low education)	[6/15]	[10/1]	[6/15]	[10/1]
Fossil-fuel dependence (low high)	−1.45** (0.57)	−1.79*** (0.57)	−0.92* (0.50)	−1.01 (0.71)
treated countries (high/low education)	[8/8]	[8/8]	[8/8]	[8/8]
Carbon intensity of GDP (low high)	−2.35*** (0.54)	−0.82 (0.58)	−1.70*** (0.57)	−0.42 (0.65)
treated countries (high/low education)	[6/11]	[10/5]	[6/11]	[10/5]
2005 EU ETS cohort (other EU cohort)	−0.99*** (0.29)	−1.25* (0.68)	−0.04 (0.29)	−0.60 (0.73)
treated countries (high/low education)	[11/1]	[5/15]	[11/1]	[5/15]
Instrument (ETS only carbon tax)	−0.38 (0.51)	−2.62*** (0.50)	−0.13 (0.53)	−1.46** (0.63)
treated countries (high/low education)	[6/9]	[10/7]	[6/9]	[10/7]
OECD membership (non-OECD OECD)	−1.28*** (0.46)	−1.60*** (0.55)	−1.08 (0.75)	−0.89 (0.57)
treated countries (high/low education)	[6/3]	[10/13]	[6/3]	[10/13]
Initial inequality (low high market Gini)	−2.35*** (0.42)	−1.03 (0.63)	−1.80*** (0.47)	−0.25 (0.64)
treated countries (high/low education)	[7/8]	[8/8]	[7/8]	[8/8]
Renewables base (low high)	−0.65 (0.55)	−2.53*** (0.55)	−0.34 (0.72)	−1.77*** (0.56)
treated countries (high/low education)	[7/9]	[9/7]	[7/9]	[9/7]
Income level (low high)	−1.47* (0.77)	−1.58*** (0.50)	−0.65 (0.74)	−1.25** (0.62)
treated countries (high/low education)	[9/5]	[7/11]	[9/5]	[7/11]
Social protection coverage (low high)	−2.25*** (0.50)	−1.36** (0.60)	−1.81*** (0.56)	−0.56 (0.66)
treated countries (high/low education)	[8/6]	[8/10]	[8/6]	[8/10]
Effective carbon rate (low high)	−0.99 (0.75)	−1.97*** (0.60)	0.58 (0.43)	−1.73*** (0.64)
treated countries (high/low education)	[10/4]	[6/12]	[10/4]	[6/12]

Note: Each entry is the education gap, the Sun–Abraham ATT of high-education-priority treated countries minus that of low-priority ones (same median split as Table 2, Panel B), within the subsample named in the row label (Group 1 and Group 2 are the two groups named there, in that order), against the full never-treated control pool; standard errors from the two independent estimates in parentheses. Counts of treated countries (high/low education) are in square brackets; cells with few countries are imprecise. The first-five-years row keeps every treated country only for event times 0 to 4. Income level, social protection and the effective carbon rate are current country levels; splits are at the median among treated countries. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A10: Is the Moderation an Artefact of Pre-Trends?

	Market Gini	Disposable Gini
<i>Panel A: CP × Education interaction under trend controls</i>		
Baseline (Table 2, Panel A)	−1.92*** (0.53)	−1.35*** (0.50)
Adding an education-specific linear trend (all countries)	−1.44** (0.56)	−1.02* (0.55)
Adding treated-specific trends (interacted with education)	−0.54 (0.46)	−0.34 (0.42)
Adding country-specific linear trends	−0.22 (0.31)	−0.10 (0.30)
<i>Panel B: pre-adoption linear trend of treated countries relative to never-treated (Gini points per year)</i>		
High education priority	−0.036 (0.078)	−0.057 (0.090)
Low education priority	0.281*** (0.061)	0.149*** (0.051)
Difference (low − high)	0.317*** (0.090)	0.206** (0.088)
Observations	1,972	1,972
Countries	110	110
Treated countries	32	32

Note: Panel A adds trend controls to the two-way fixed effects interaction model of Table 2 (2000–2020, same controls, standard errors clustered by country); the education coefficient is per standard deviation. Trend controls that are estimated over the whole window also absorb part of a gradually growing post-adoption effect, so the last two rows are demanding: the main carbon pricing coefficient falls as well. Panel B uses only observations before adoption (and all never-treated observations) and regresses the outcome on separate linear time trends for high- and low-priority treated countries, with country and year fixed effects; standard errors are clustered by country and stars use the normal approximation. Observations, countries and treated countries refer to the baseline sample of Panel A; Panel B uses pre-adoption observations only. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A4.4 Is it just weak carbon pricing?

Table A11: Is the Moderation Just Weak Carbon Pricing?

<i>Panel A: effective carbon rate of the treated countries (euros per tonne of CO₂e; mean, median, number)</i>		
High education priority		44.9 (24.8) [17]
Low education priority		67.2 (69.3) [18]
Correlation with the education budget share		−0.45
<i>Panel B: carbon pricing effect (Sun–Abraham ATT) by stringency, split at the median effective carbon rate</i>		
	Market Gini	Disposable Gini
Low-stringency treated countries	1.82*** (0.39)	1.10*** (0.33)
number of treated countries	[16]	[16]
High-stringency treated countries	2.56*** (0.39)	2.12*** (0.42)
number of treated countries	[18]	[18]
<i>Panel C: CP × Education with the effective carbon rate</i>		
	Market Gini	Disposable Gini
CP × Education, baseline	−1.92*** (0.53)	−1.35*** (0.50)
CP × Education, adding CP × effective carbon rate	−1.76*** (0.65)	−1.17* (0.59)
CP × effective carbon rate, alone	0.79** (0.37)	0.67** (0.31)
education gap within low / high stringency		see Table A9
Observations	1,972	1,972
Countries	110	110
Treated countries	32	32

Note: Effective carbon rates are the OECD Net Effective Carbon Rates (carbon taxes, emissions trading permit prices and fuel excise duties, averaged over all domestic emissions, current prices), averaged over 2018, 2021 and 2023, so they describe current regimes and cover 79 countries, including all 35 treated countries here. Panel C is the two-way fixed effects interaction model of Table 2 with the effective carbon rate standardised across treated countries; never-treated countries enter with a zero interaction. Standard errors in parentheses. Observations, countries and treated countries refer to the sample of Panel C. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A4.5 Does it depend on the type of instrument (carbon tax or ETS)?

Table A12: Carbon Tax Versus Emissions Trading System: Countries and Effects

<i>Panel A: treated countries by instrument</i>				
	Countries	High education	Mean adoption year	Mean effective carbon rate
ETS only	17	6	2007	55
Carbon tax only	6	6	2017	25
ETS and carbon tax	12	5	2006	74

<i>Panel B: carbon pricing effect (Sun–Abraham ATT), full post-adoption period and first five years</i>				
	Market Gini		Disposable Gini	
	(1) Full period	(2) First five years	(3) Full period	(4) First five years
ETS only [15]	2.47*** (0.36)	1.06*** (0.20)	2.08*** (0.38)	0.68*** (0.17)
Carbon tax only [5]	−0.52*** (0.15)	−0.16 (0.12)	−0.08 (0.15)	0.15 (0.12)
ETS and carbon tax [12]	2.46*** (0.46)	0.82*** (0.21)	1.57*** (0.47)	0.57*** (0.18)
Any carbon tax (alone or with ETS) [17]	2.04*** (0.44)	0.56*** (0.21)	1.33*** (0.43)	0.45*** (0.16)

Note: Instrument type from the adoption file (carbon tax and emissions trading system, or both). Effective carbon rate is the OECD measure of Table A11, averaged over 2018–2023. Panel B: Sun–Abraham estimates against the full never-treated control pool, as in Table 2 (Panel B); “first five years” keeps each treated country only for event times 0 to 4. Number of treated countries in square brackets. $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.

Table A13: Carbon Tax Versus Emissions Trading System: Education Moderation

	(1) Market Gini	(2) Disposable Gini
<i>Panel A: education gap (high – low priority) by instrument [treated high/low education]</i>		
ETS only [6/9]	–0.38 (0.51)	–0.13 (0.53)
Carbon tax only [5/0]	n.a.	n.a.
ETS and carbon tax [5/7]	–2.17*** (0.53)	–1.19* (0.65)
Any carbon tax [10/7]	–2.62*** (0.50)	–1.46** (0.63)
Excluding the carbon-tax-only countries [11/16]	–1.26** (0.52)	–0.75 (0.57)
<i>Panel B: two-way fixed effects interaction models</i>		
CP × Education, baseline	–1.92*** (0.53)	–1.35*** (0.50)
CP × Education, excluding the carbon-tax-only countries	–1.43** (0.59)	–0.92 (0.61)
CP × Education in countries with no carbon tax	–0.65 (0.48)	–0.18 (0.45)
CP × Education in countries with a carbon tax	–2.88*** (0.67)	–2.20*** (0.60)
Difference (three-way interaction)	–2.23*** (0.83)	–2.02*** (0.75)
Observations	1,972	1,972
Countries	110	110
Treated countries	32	32

Note: Panel A: Sun–Abraham estimates against the full never-treated control pool; the education gap is the ATT of high-priority treated countries minus that of low-priority ones, with standard errors from the two independent estimates. The six carbon-tax-only countries are all in the high-education group, so no gap can be computed for them. Panel B: interaction model of Table 2 (Panel A), 2000–2020, education per standard deviation, standard errors clustered by country, never-treated countries kept. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A4.6 How does it work?

Table A14: Education Priority and the Mediation Channels (Market Gini)

Mediator	Stage 1: outcome = mediator		Stage 2: outcome = Gini		Attenuation: outcome = Gini		
	CP	CP $\times$ Edu	Mediator	Mediator $\times$ Edu	CP $\times$ Edu	CP $\times$ Edu, with mediator	Shrinkage
Industrial employment (%)	-1.73*** (0.48)	0.82 (0.50)	-0.132** (0.052)	0.203*** (0.060)	-1.92*** (0.53)	-1.45*** (0.53)	24%
Renewable energy share (%)	5.55*** (0.98)	-1.93* (1.04)	0.073** (0.032)	-0.066* (0.036)	-1.92*** (0.53)	-1.68*** (0.53)	13%
Services employment (%)	0.66 (0.75)	-0.35 (0.76)	0.081 (0.056)	-0.113*** (0.040)	-1.92*** (0.53)	-1.56*** (0.55)	19%
Labour income share (%)	-1.74* (0.90)	-0.47 (0.70)	0.001 (0.032)	0.024 (0.032)	-1.64*** (0.44)	-1.62*** (0.43)	1%
Union density (%)	-0.42 (0.98)	0.77 (1.07)	-0.056 (0.042)	0.113** (0.044)	-2.01*** (0.65)	-1.75*** (0.59)	13%

Note: Two-way fixed effects, 2000–2020, with the controls of Table 3. Edu is the pre-treatment education share of government expenditure, standardised. Stage 1: mediator on carbon pricing (CP) and CP $\times$ Edu. Stage 2: market Gini on the mediator and mediator $\times$ Edu. Last three columns: market Gini on CP and CP $\times$ Edu without and with the mediator terms; shrinkage is the percentage reduction in the CP $\times$ Edu coefficient. Each row uses one sample (78 to 110 countries, 31 to 32 treated). Standard errors clustered by country. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.