risks

MDPI

Article

Does Carbon Pricing Displace Crypto-Mining Emissions? Quantile Evidence on Carbon Leakage from EU27, Russian and Rest-of-World Power Grids

Pham Ngoc Toan [1], Le Tran Trung Hieu [1,*] and Nguyen Vu Trung Nguyen [2]

[1] School of Accounting, University of Economics Ho Chi Minh City, Ho Chi Minh City 72400, Vietnam; toanpn@ueh.edu.vn
[2] School of Finance, Banking University of Ho Chi Minh City, Ho Chi Minh City 72400, Vietnam; 030139230486@st.buh.edu.vn
* Correspondence: hieule.524202141020@st.ueh.edu.vn

Abstract

Carbon pricing is jurisdictional, while proof-of-work cryptocurrency mining is a highly mobile electricity load. We examine whether daily power-sector emissions display a cross-regional and distributional pattern consistent with short-run emissions displacement. Using daily observations covering calendar years 2019–2025 (with a boundary observation on 1 January 2026; N = 2550 after transformation and cleaning), we estimate quantile regressions for the EU27, the Russian Federation and the rest of the world using the interaction between Bitcoin returns and European carbon-allowance returns. The focal Russian lower-tail interaction is positive (q10 beta = 0.0662); OLS and dynamic specifications remain positive, and a 1000-replication pairs bootstrap gives $p = 0.0077$. The association survives a trading-day-only sample, calendar and persistence controls, and a seven-lag specification, while randomised-carbon and non-power-sector placebo outcomes are null. However, the coefficient loses conventional significance without Winsorisation, the May-2021 Chinese-ban timing prediction is not supported, and a direct EU27-minus-Russia substitution diagnostic is null. Quantile-on-quantile estimates place the largest Russian Bitcoin-return coefficients in high-carbon-price, low-emission states, but remain descriptive. Because the design does not observe mining capacity moving across jurisdictions and the available full-sample Russian emissions series is national rather than subnational, the evidence supports a leakage-consistent operational association rather than proof of physical relocation or a broad causal effect of EU carbon pricing.

Keywords: carbon leakage; carbon pricing; EU ETS; Bitcoin mining; power-sector emissions; quantile regression

Check for updates

Academic Editor: Anatoliy Swishchuk

Received: 23 July 2026
Revised: 17 August 2026
Accepted: 20 August 2026
Published: 21 August 2026

Copyright: © 2026 by the authors. Licensee MDPI, Basel, Switzerland. This article is an open access article distributed under the terms and conditions of the Creative Commons Attribution (CC BY) license.

1. Introduction

Unilateral climate policy can alter both the level and the location of emissions. Standard leakage models emphasise competitiveness, trade and investment channels: a policy that raises production costs in one jurisdiction can shift output, investment or embodied emissions toward less-regulated locations (Copeland and Taylor 2004; Fischer and Fox 2012; Branger and Quirion 2014). Yet empirical evidence from the EU Emissions Trading System (EU ETS) is mixed rather than uniformly supportive. Naegele and Zaklan (2019), Martin et al. (2014a, 2014b, 2016), Dechezleprêtre et al. (2022) and Verde (2020) find little or no robust realised leakage in important manufacturing settings, while other work documents

heterogeneous competitiveness, innovation and performance responses (Aldy and Pizer 2015; Calel and Dechezleprêtre 2016; Marin et al. 2018; Löschel et al. 2019). The appropriate starting point is therefore not that carbon pricing necessarily causes relocation, but that relocation is one mechanism whose empirical relevance depends on mobility, cost exposure and available destinations. More broadly, Bayer and Aklin (2020) estimate that the EU ETS reduced emissions despite low allowance prices.

Proof-of-work (PoW) cryptocurrency mining differs from conventional heavy industry along the dimensions most relevant to leakage: revenue is globally determined, electricity is a dominant operating input, and specialised hardware can be redeployed more quickly than a steel or cement plant (Taylor 2017). Environmental-accounting and location studies estimate Bitcoin electricity use, carbon footprint and geographic distribution and show that results depend materially on hardware efficiency, utilisation, electricity mix and mining geography (de Vries 2018; Krause and Tolaymat 2018; Stoll et al. 2019; Jiang et al. 2021; de Vries et al. 2022; Sun et al. 2022; Jones et al. 2022; Baur and Oll 2022). These studies provide the strongest direct or semi-direct evidence on mining activity and mobility, but they do not test whether a market-price interaction appears in daily regional power emissions.

A parallel crypto-energy-finance literature studies connectedness and spillovers among Bitcoin, energy commodities, utilities and carbon markets (Tan et al. 2020; Corbet et al. 2021; Di Febo et al. 2021; Sarkodie et al. 2022; Zhang et al. 2023; Sibande et al. 2023). This work makes common energy-finance shocks a serious alternative explanation for any Bitcoin-carbon-emissions interaction. The gap between direct mining-location evidence and market-spillover evidence motivates a deliberately narrower design: regional physical power-sector emissions are the outcome, but mining location itself is not observed. The study can therefore identify a leakage-consistent pattern only in a reduced-form sense, not relocation itself.

This paper asks whether daily power-sector emissions display a cross-jurisdictional pattern in which Bitcoin returns and European allowance returns interact differently across grids. Russia is analysed as a large non-EU power system with documented mining activity, while the EU27 and the rest of the world provide regional benchmarks rather than treated and control groups. The research question is therefore about whether the spatial, distributional and sectoral patterns predicted by a mobile-load mechanism appear in the data, not whether the regression can by itself identify a causal EU-to-Russia relocation event.

Our empirical strategy combines three features. First, daily Carbon Monitor power-sector CO_2 estimates align the emissions outcome with the frequency of the market variables while remaining model-derived rather than direct meter readings. Second, the coefficient of interest is the Bitcoin-return and carbon-allowance-return interaction, which describes whether the conditional Bitcoin-emissions association changes with the carbon-price state. Third, quantile regression allows this association to vary across the conditional distribution of emission growth. The design is supplemented by high-replication inference, trading-day restrictions, dynamic and calendar controls, alternative regime splits, placebo outcomes, a direct cross-region diagnostic, outlier sensitivity and quantile-on-quantile surfaces. None of these features creates an instrument or direct observation of relocation.

A daily test can speak more naturally to an operational margin than to a capital margin. The capital margin is physical relocation of mining hardware—containers shipped, sites built and grid connections secured—and unfolds over months. The operational margin is faster: operators can raise utilisation on one grid and curtail already-installed machines on another as relative operating conditions change. Daily data may capture this short-run load reallocation, but they cannot identify a particular facility or container moving from the EU to Russia. Throughout the paper, physical relocation is reserved for evidence that directly observes mining location.

The paper makes three bounded contributions. First, it bridges carbon-leakage research with crypto-mining and energy-finance research by moving the outcome from asset returns or an aggregate Bitcoin footprint to region-specific power-sector emissions. Second, it makes the evidence hierarchy explicit: direct relocation evidence is distinguished from mechanism-aligned falsification tests and from reduced-form market-emissions associations. Third, it reports both supportive and contradictory sensitivity evidence in the main text rather than leaving robustness in the do-file. A secondary Green Bond control is retained for specification continuity, but its economically small coefficients are not treated as a central green-paradox result.

The remainder proceeds as follows. Section 2 reviews the leakage, crypto-mining and energy-finance literatures and develops the theoretical mechanism and hypotheses. Section 3 describes data, estimation, the risk-management interpretation, quantile-on-quantile analysis and inference. Section 4 reports the baseline and principal robustness results. Section 5 discusses the evidence relative to competing mechanisms and prior research. Section 6 concludes with deliberately limited policy implications.

2. Literature Review and Theoretical Framework

2.1. Carbon Leakage, Mobility and Relocation Costs

A standard leakage mechanism begins with a relative cost change across jurisdictions. In trade and location models, unilateral environmental policy can alter production, investment or trade patterns when regulated firms face higher marginal costs and can substitute toward less-regulated locations (Copeland and Taylor 2004; Fischer and Fox 2012; Böhringer et al. 2012; Branger and Quirion 2014). Border adjustment and output-based allocation studies show that instrument design can materially alter leakage incentives (Monjon and Quirion 2011; Böhringer et al. 2016; Mehling et al. 2019; Mörsdorf 2022; Perdana and Vielle 2022; European Parliament and Council 2023). Empirically, EU ETS studies often find limited realised relocation, with free allocation, sunk capital, trade costs and agglomeration helping to explain why theoretical exposure does not automatically become observed leakage (Martin et al. 2014a, 2014b, 2016; Naegele and Zaklan 2019; Verde 2020; Dechezleprêtre et al. 2022). Mobility is therefore not background context but a central condition of the mechanism tested here. The institutional evolution of the EU ETS and its allocation and market-design architecture are reviewed in Ellerman et al. (2010, 2016).

For a stylised mining operator choosing location j, profit equals mining revenue minus electricity use multiplied by delivered operating cost. Mining revenue depends on the Bitcoin price and network difficulty or hashrate; delivered cost combines the local electricity price, carbon costs embedded in generation, grid fees, taxes, curtailment, climate and cooling costs, regulation and other charges. A mobile operator changes location or utilisation only when expected operating-cost savings exceed relocation, contracting and operational frictions. This distinction also separates a slow capital margin (physical redeployment of equipment) from a faster operational margin (utilisation of already-installed capacity).

Electricity prices, fuel prices, carbon costs, grid fees and regulation can all affect delivered power costs, while carbon-cost pass-through is neither automatic nor uniform (Sijm et al. 2006; Fell 2010; Keppler and Mansanet-Bataller 2010; Fabra and Reguant 2014; Hintermann 2016; Cludius et al. 2020). Carbon-market research also shows that allowance prices are connected to energy and financial markets and shaped by institutional design, expectations and policy credibility (Koch et al. 2014; Hintermann et al. 2016; Tan et al. 2020; Osorio et al. 2021; Sitarz et al. 2024). The Bitcoin $\times$ EFP coefficient is therefore interpreted as a reduced-form conditional association, not as a structural pass-through coefficient.

This distinction is essential for the leakage claims. A causal relocation statement would require evidence that mining capacity actually moved between jurisdictions and that

the movement was induced by the relevant cost wedge. The present regressions observe neither condition directly. They test whether spatial, sectoral, distributional and timing patterns predicted by a mobile-load mechanism are present or absent.

2.2. Crypto-Mining Location, Environmental Impacts and Energy-Finance

The crypto-mining environmental literature establishes both the materiality and measurement difficulty of mining electricity use. Early studies quantify substantial electricity and carbon requirements but disagree over hardware assumptions, utilisation, future growth and marginal emissions (de Vries 2018; Krause and Tolaymat 2018; Mora et al. 2018; Stoll et al. 2019; Dittmar and Praktiknjo 2019; Houy 2019; Masanet et al. 2019). More recent work explicitly models or measures mining geography and shows that location and electricity mix are central to carbon outcomes (Jiang et al. 2021; de Vries et al. 2022; Sun et al. 2022). This stream provides the strongest basis for the mobility premise because it uses mining activity, pools, hashrate geography or energy-mix information rather than inferring location from financial prices alone.

A parallel energy-finance literature focuses on interactions rather than physical relocation. Carbon allowances are connected to energy and financial markets (Tan et al. 2020); Bitcoin and energy markets exhibit interrelationships (Corbet et al. 2021); Bitcoin and carbon allowances display tail-risk spillovers (Di Febo et al. 2021); trading activity is associated with Bitcoin energy use and carbon footprint (Sarkodie et al. 2022); cryptocurrency energy use and hashrate have nonlinear climate implications (Zhang et al. 2023); and mining electricity demand can interact with fossil-fuel prices (Sibande et al. 2023). These findings make common market shocks a credible competing channel. Our contribution relative to this stream is the region-specific physical emissions outcome; our limitation relative to the location literature is that mining location itself remains unobserved.

2.3. Evidence Hierarchy and Leakage Mechanism

We organise the evidence in three levels. Level 1 is direct relocation evidence: location-specific hashrate shares, mining-pool/IP geolocation, facility-level electricity demand or other observations identifying where mining capacity operates over time. Level 2 is mechanism-aligned indirect evidence: destination-specific power-emission responses, timing around relocation-relevant policy shocks, sector-specific placebo tests and direct origin-destination substitution diagnostics. Level 3 is reduced-form association: the Bitcoin $\times$ EFP interaction in regional power-emission growth. This study has Level 3 evidence and several Level 2 falsification tests, but no Level 1 evidence. A result can therefore be described as leakage-consistent only when multiple mechanism predictions line up; it cannot be described as proof of relocation.

The mechanism generates four observable implications. First, because the effect of an incremental electricity load depends on the generating margin, any association need not be uniform across the conditional distribution. Second, a spatial-displacement story requires destination specificity rather than an equally strong global pattern. Third, a mining-load mechanism should be more visible in power emissions than in non-power sectors. Fourth, if the association is specifically tied to the large-scale reallocation episode after China's mining prohibition, it should strengthen after the May 2021 break. These implications motivate the hypotheses and define conditions under which the leakage interpretation should be weakened.

H1 (distributional prediction). *If the focal association reflects an electricity-load channel, the Bitcoin $\times$ EFP coefficient may be concentrated in particular conditional quantiles of power-emission growth rather than uniform across the distribution. The empirical analysis tests the full quantile profile and does not impose a deterministic dispatch ranking ex ante.*

H2a (spatial prediction). *A leakage-consistent pattern requires a positive and comparatively robust Russian interaction that is not mirrored by an equally robust lower-tail association in the broad ROW benchmark. The EU27 is treated as an origin-side comparison, not as a treated group from which relocation is directly observed.*

2.4. Timing, Sectoral Specificity and Common-Shock Falsification

Timing and outcome specificity provide sharper mechanism tests than a cross-region coefficient alone. The May 2021 Chinese mining prohibition is the primary candidate break because the literature documents a major subsequent change in global mining geography (de Vries et al. 2022; Sun et al. 2022). The empirical implementation uses a mid-May 2021 split consistent with the replication output. By contrast, March 2020 is reported only as an alternative regime split: it captures a large macro-financial and electricity-demand shock but is not specific to mining relocation. A February 2022 Ukraine split provides an additional geopolitical sensitivity check. Sectoral placebos are equally important because a mining-load mechanism should affect the power sector more directly than Russian residential or ground-transport emissions.

H2b (timing prediction). *If the Russian association is specifically related to the post-China relocation episode, the q10 Bitcoin × EFP coefficient should be stronger after the May 2021 break than before it. A formal coefficient-break test, not separate-sample significance alone, is required. Failure of this prediction weakens the relocation-timing interpretation.*

H3 (sectoral/common-shock prediction). *A mining-load interpretation is strengthened if the Russian power-sector association is not reproduced in Russian residential and ground-transport placebos and if the broad ROW benchmark does not show a comparable lower-tail pattern. These are falsification conditions, not causal identification tests.*

2.5. Green Finance as a Secondary Control

Green-bond markets may correlate with emissions through opposing channels, including clean-capital deployment, macro-financial conditions and infrastructure construction. Prior work reports heterogeneous relationships between green financial assets, cryptocurrency markets and sectoral emissions (Siddique et al. 2023; Pata et al. 2025). Because the Green Bond coefficients in this study are economically small and the model is not designed to identify the use of bond proceeds, the index is retained as a control and secondary descriptive result rather than a separate theoretical contribution. A broader supply-side literature also argues that anticipated climate policy can affect the timing of fossil-resource extraction (Sinn 2008); that mechanism is not identified in the present design.

No directional Green Bond hypothesis is used to support the leakage mechanism. The coefficient is reported for specification continuity and transparency only, and statistical significance is not treated as evidence for or against the paper's central displacement interpretation.

3. Data and Methodology

3.1. Data

The working dataset covers calendar years 2019–2025, with a boundary observation dated 1 January 2026 in the supplied file; after transformation and cleaning, N = 2550. We therefore describe the substantive analysis window as 2019–2025 rather than as a 2026 sample.

Dependent variables. Daily power-sector CO_2 emissions for the EU27, the Russian Federation and the rest-of-world aggregate are drawn from Carbon Monitor (Liu et al. 2020a, 2020b) and its high-frequency power-generation infrastructure (Zhu et al. 2023).

These are model-based emissions estimates rather than direct emissions-meter readings. For Russia, high-frequency total generation is combined with modelled fuel disaggregation and emission factors, so daily variation contains both observed activity information and modelling assumptions. Carbon Monitor does not report a single observed/modelled ratio for the final national daily emissions series; we therefore document the provenance and treat measurement error as a substantive limitation rather than inventing such a ratio.

Independent variables. EFP is the front-December EU allowance (EUA) futures series used in the authors' replication dataset; BTC is the Bitcoin closing price. Controls are Ethereum (ETH), WTI crude oil (WTI_Oil) and the Bloomberg MSCI Green Bond Index (Green_Bond). For reproducibility, the empirical interpretation in this paper follows the variable definitions used in the authors' supplied workbook and code.

Transformation and magnitude. Price and emissions series are converted to log first differences, x_t = ln X_t − ln X_{t − 1}, and Winsorised at the 1st and 99th percentiles in the baseline. The outlier sensitivity check reruns the focal Russian q10 model without Winsorisation. For scale, the baseline Russian q10 coefficient implies that a 10% Bitcoin increase combined with a 10% EFP increase contributes mechanically about 0.000601 log points, or approximately 0.060% of daily Russian power emissions, holding the other regressors fixed. At a sample mean near 2.05 $MtCO_2$/day, this is roughly 1.23 $ktCO_2$/day. It is an illustrative fitted association, not tonnes of identified relocation.

Calendar mismatch and controls. Carbon Monitor is daily, whereas financial markets close on weekends and holidays. The supplied construction carries the last available EFP and WTI prices forward, creating zero returns on non-trading days; the trading-day-only robustness sample removes these observations. Additional specifications include a lagged dependent variable, seven emission lags, alternative regime splits and calendar/policy controls. Temperature, local electricity prices and load, renewable availability and other local dispatch conditions are not available in the supplied replication dataset and remain explicit omitted-variable limitations rather than being replaced by undocumented proxies.

3.2. Estimator

We estimate simultaneous quantile regressions (Koenker and Bassett 1978; Koenker 2005) of the form

$$Q_\tau(\Delta e_{r,t} \mid \cdot) = \alpha_\tau + \beta_\tau \Delta btc_t + \gamma_\tau \Delta efp_t + \boldsymbol{\delta_\tau (\Delta btc_t \times \Delta efp_t)} + \theta_\tau' \mathbf{z}_t$$

for each region r ∈ {EU27, Russia, ROW} and quantiles τ ∈ {0.10, 0.25, 0.50, 0.75, 0.90}, where **z** collects ETH, WTI and Green Bond returns. The coefficient of interest is $\boldsymbol{\delta_\tau}$, which measures how the carbon price modulates the pass-through from Bitcoin returns to power-sector emissions at the τ-th conditional quantile.

Three features of this specification deserve comment. Estimating the five quantiles simultaneously rather than separately permits formal cross-quantile comparisons, so distributional heterogeneity can be tested rather than merely displayed. Quantile regression is useful for heavy-tailed daily emission and crypto data, but it does not make the regressors exogenous. In particular, an omitted macro-energy shock can affect Bitcoin returns, carbon returns and regional emissions in different ways and can therefore load onto the interaction term. The robustness design addresses persistence, timing and falsification, but it does not purge all common factors.

Interpretation of δ_τ. In a specification in first differences of logs, δ_τ is the change in the conditional Bitcoin-return sensitivity of emission growth associated with a one-unit change in the carbon-allowance return. A positive δ_τ means that the Bitcoin-emissions association is stronger on days when the allowance return is higher, holding the included

controls fixed. It does not by itself show that the carbon price caused the emissions change or that mining load moved between jurisdictions.

Choice of estimator. The design rules out several natural alternatives more than it selects quantile regression by default. A conditional-mean panel or difference-in-differences design of the kind used in the manufacturing-leakage literature (Naegele and Zaklan 2019) needs a clean treated-versus-control contrast and a relocation that unfolds over the years such a design can resolve; neither holds for a load that can change utilisation quickly and lacks a clean untreated grid. A GARCH or volatility-spillover model would characterise second moments, but the empirical question here concerns the level and location of emissions rather than their variance. Tail-connectedness and CAViaR or TVP-VAR frameworks (Siddique et al. 2023) are designed primarily for market co-movement. Quantile regression instead provides a coefficient at different points of the conditional emission distribution and is useful with heavy-tailed daily data. Its costs are equally important: extreme-tail estimates can be imprecise, low-replication bootstrap inference can be unstable, and the interaction term does not eliminate endogeneity or region-specific common shocks.

A more general reading. Estimating across quantiles places the exercise in the same frame as standard risk-management tools, where the object of interest is a tail of a distribution rather than its mean. Value-at-Risk is a quantile of the loss distribution, and Expected Shortfall, also known as Averaged Value-at-Risk, averages losses beyond a tail threshold; a quantile regression of emission growth is the conditional-distribution analogue, describing how covariates move particular points of the emission distribution rather than only its centre. The correspondence matters here because the focal Russian association is concentrated in the lower conditional tail. Quantiles are one route to that tail; expectiles are another, defined by asymmetric squared loss and sensitive to the magnitude of tail outcomes. Zaevski and Nedeltchev (2023) discuss expectile-based measures relative to Expected Shortfall in the Basel context. We report quantile estimates because the interaction coefficient has a direct reading at each quantile; no universal dispatch ordering is assumed.

3.3. Quantile-on-Quantile Regression

The interaction specification above conditions on the state of the *grid*—the quantile τ of emission growth—but treats the carbon price as a single continuous regressor. Yet the leakage hypothesis is a statement about two states simultaneously. It says that when the carbon price is *high* and the receiving grid is *slack*, mining load relocates. Testing that proposition directly requires conditioning on the state of the carbon market as well.

We therefore complement the baseline with the quantile-on-quantile regression (QQR) of Sim and Zhou (2015), which estimates a coefficient surface $\beta(\theta, \tau) = \partial\, Q_\tau(\Delta e_{r,t})/\partial\, \Delta btc_t$, conditional on Δefp_t lying at its θ-th quantile over the grid $\theta, \tau \in [0.10, 0.90]^2$. The estimator solves a locally weighted quantile problem in which each observation is weighted by a Gaussian kernel on the distance between the empirical CDF of the carbon-price series and θ: $\min \Sigma\, \rho_\tau [\Delta e_t - \beta_0(\theta,\tau) - \beta_1(\theta,\tau)(\Delta btc_t - F^{-1}(\theta))] \cdot K([F(\Delta efp_t) - \theta]/h)$ with bandwidth $h = 0.05$ and $h = 0.10$ as a sensitivity check. The QQR and interaction specifications are complementary: the interaction imposes a linear moderation term and estimates one number per emission quantile, while QQR relaxes that functional form and describes where stronger local Bitcoin-emissions slopes occur across the joint carbon-state/emission-state plane. Neither estimator by itself creates exogenous variation in the carbon price.

The leakage prediction is spatially specific in this plane. In **Russia**, $\beta(\theta, \tau)$ should be largest in the upper-θ, lower-τ corner—expensive European carbon, slack Russian grid. In

the **EU27** it should show no such concentration, and in the **rest of the world** no structure at all.

3.4. Inference

Quantile inference is reported with explicit attention to resampling instability. The original simultaneous-quantile estimates use bootstrap standard errors, but low replication counts can make borderline *p*-values unstable (Hahn 1995; Parzen et al. 1994; Feng et al. 2011). We therefore treat the 20-replication exercise in Section 4.8 as a diagnostic rather than primary evidence. For the focal Russian q10 coefficient we additionally ran a 1000-replication pairs bootstrap (beta = 0.0662, bootstrap SE = 0.0248, $p = 0.0077$; percentile 95% interval [0.0115, 0.1097]). The main robustness claims are required to survive draw-independent or higher-replication checks rather than one low-replication draw.

As complements to the bootstrap, we report OLS with heteroskedasticity-robust standard errors, dynamic quantile specifications, a trading-day-only sample, alternative regime splits, a seven-lag model, placebo outcomes, an EU27-minus-Russia diagnostic and quantile-on-quantile surfaces. These checks are reported in Section 4. Their purpose is not to manufacture causality, but to show which parts of the empirical pattern survive reasonable changes in timing, measurement and specification.

Software and reproducibility. All core estimation was carried out in Stata 18. Simultaneous quantile regressions use sqreg with bootstrap inference; conditional-mean checks use regress with vce(robust); additional quantile checks use qreg; and purpose-written Stata code implements the Sim and Zhou (2015) quantile-on-quantile surfaces. A separate 1000-replication pairs bootstrap was used for the focal q10 interaction to assess low-replication sensitivity. Stata provides a transparent and reproducible workflow, but sqreg inference can vary with bootstrap replication count, which is why the manuscript reports robustness across estimators and samples rather than relying on a single *p*-value.

4. Results

4.1. Baseline Estimates

Table 1 reports the baseline Bitcoin × carbon-price interaction and Green Bond control across quantiles and regions. Because low-replication bootstrap standard errors can be unstable, the baseline point estimates are interpreted together with the higher-replication and analytic robustness checks reported below. Figure 1 provides a region-by-quantile visual summary of the same baseline estimates.

Table 1. Simultaneous quantile regression of power-sector CO_2 emission growth.

Panel/Quantile	EU27	Russia	ROW
A. Bitcoin × EFP—q10	0.2458 (0.1672)	0.0662 (0.0191)	0.0358 (0.0298)
A. Bitcoin × EFP—q25	0.1793 (0.1379)	0.0304 * (0.0176)	0.0117 (0.0437)
A. Bitcoin × EFP—q50	0.1023 (0.0663)	0.0207 ** (0.0093)	−0.0034 (0.0240)
A. Bitcoin × EFP—q75	0.0390 (0.0922)	0.0197 (0.0163)	−0.0121 (0.0303)
A. Bitcoin × EFP—q90	0.2667 (0.3710)	−0.0196 (0.0477)	0.0590 (0.0873)
B. Green Bond—q10	0.0001 (0.0012)	0.0001 (0.0003)	−0.0002 (0.0004)
B. Green Bond—q25	−0.0004 (0.0012)	−0.0000 (0.0002)	−0.0003 (0.0003)
B. Green Bond—q50	−0.0012 ** (0.0006)	−0.0003 (0.0001)	−0.0003 (0.0003)
B. Green Bond—q75	−0.0005 (0.0011)	−0.0005 (0.0001)	−0.0010 (0.0002)
B. Green Bond—q90	−0.0031 (0.0021)	−0.0003 (0.0004)	−0.0007 * (0.0004)

Bootstrap standard errors in parentheses in the original simultaneous-quantile baseline. N = 2550 per regression. Controls include ETH, WTI oil, and the Bitcoin and EFP main effects. Pseudo-R^2 ranges 0.0006–0.0083. Because low replication counts are unstable, focal inference is cross-checked using a 1000-pairs bootstrap and alternative estimators/samples in the subsequent robustness analyses. * $p < 0.10$; ** $p < 0.05$.

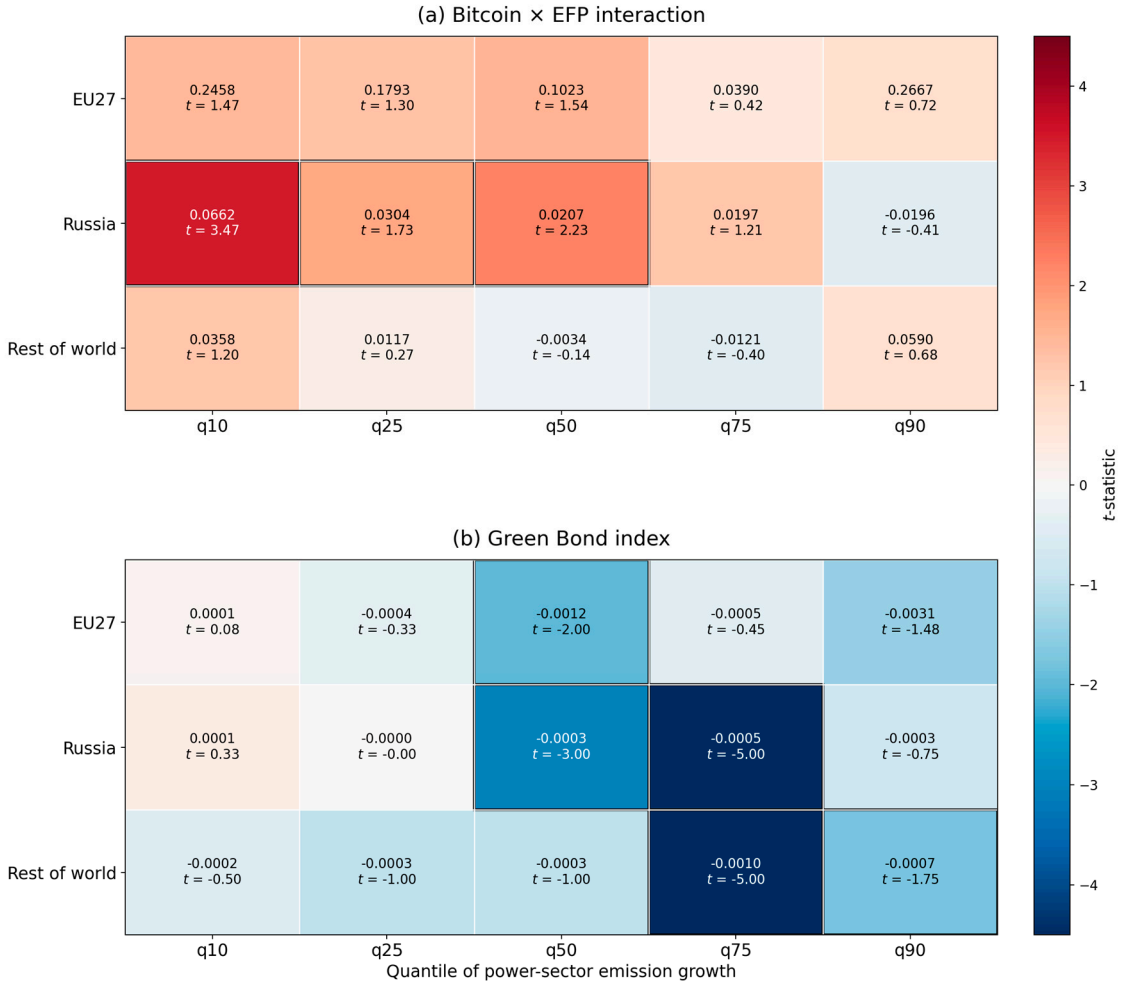


Figure 1. Baseline simultaneous-quantile estimates. (**a**) Bitcoin × EFP interaction coefficients across regions and emission quantiles; (**b**) Green Bond coefficients across the same region-by-quantile grid. Cell text reports coefficient estimates and *t*-statistics; outlined cells indicate significance at the 10% level or better. The figure is descriptive because strengthened inference is used for the focal Russian q10 result.

4.2. EU27: No Stable Interaction After Strengthened Inference

In the EU27, the interaction is positive at every reported quantile, but the evidence is not stable enough to support a strong behavioural claim (Table 1, Figure 2a). OLS gives an interaction of 0.1153 ($p = 0.081$) and the model is not jointly significant ($p = 0.254$). In the dynamic q10 specification, the EU27 interaction is also not significant. We therefore treat the EU27 estimates as a comparison series rather than evidence that the carbon price either successfully restrained or failed to restrain mining load.

Two ancillary specifications are worth reporting because they qualify rather than overturn this null. Under OLS with robust standard errors, the interaction is 0.1153 with $p = 0.081$—marginal—but the model as a whole is not jointly significant (F-test $p = 0.254$), so the coefficient cannot be read as evidence of a relationship the specification has otherwise failed to detect. In the dynamic specification, the q25 coefficient reaches significance (0.1572, $p = 0.013$) while q10, q50, q75 and q90 do not. We regard an isolated significant coefficient at a single interior quantile, in one of several specifications, as insufficient to support a claim, and we note it here rather than build on it.

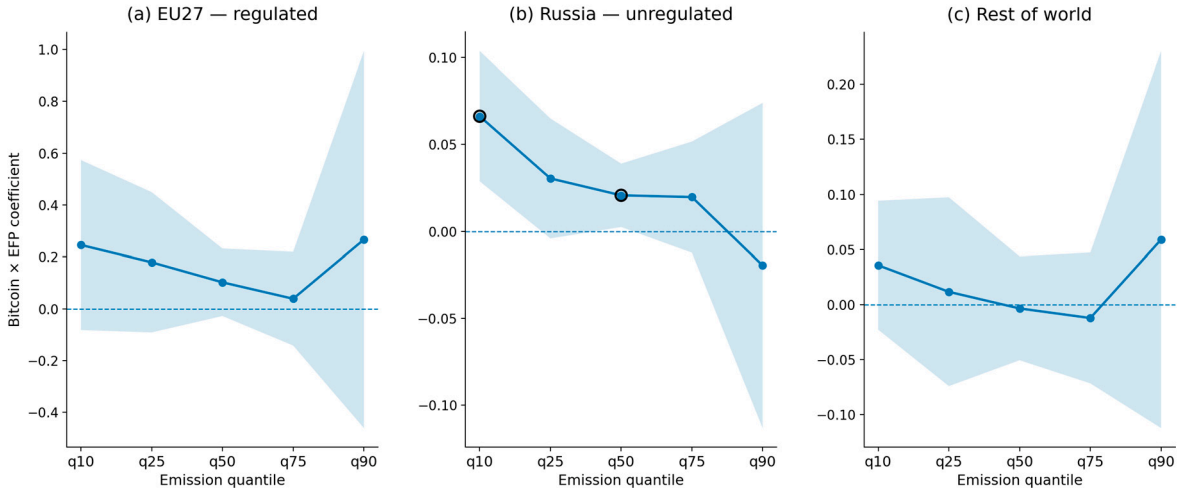


Figure 2. Quantile profiles of the Bitcoin × EFP interaction for (**a**) EU27, (**b**) Russia and (**c**) rest of world, with 95% confidence bands from the baseline simultaneous-quantile estimates. Circled markers indicate 5% significance in the low-replication baseline; strengthened inference narrows the focal result to the Russian lower tail.

We therefore interpret H1 cautiously. The EU27 profile does not provide a robust negative interaction, but absence of a stable negative coefficient is not itself proof of policy ineffectiveness. Daily emissions are noisy, the model omits several local electricity-market determinants, and the design is observational.

The economic implication is consequently narrower than in the earlier draft. The sample spans a period of large allowance-price changes, yet this specification does not recover a stable EU27 interaction. That result motivates the cross-region comparison, but it does not by itself identify the causal effect of the EU ETS on mining location or emissions.

4.3. Russia: The Focal Lower-Tail Association

The Russian estimates provide the focal result. The baseline interaction is positive at q10 (delta = 0.0662). A 1000-replication pairs bootstrap for this coefficient gives a standard error of 0.0248, $p = 0.0077$ and a percentile 95% interval of [0.0115, 0.1097]. The q50 point estimate is positive but is less stable under strengthened inference, so the interpretation centres on the lower tail rather than multiple quantiles.

Two features matter. First, the sign is positive: on days when the carbon-price return is higher, the conditional association between Bitcoin appreciation and Russian power-emission growth is stronger. Second, the result is concentrated in the lower part of the conditional emission distribution. This spatial and distributional pattern is consistent with an operational displacement mechanism, but the coefficient does not identify the origin of the load or demonstrate that mining hardware physically moved into Russia.

The focal association survives several alternative specifications. OLS with heteroskedasticity-robust standard errors gives delta = 0.0321 ($p = 0.012$). A one-lag dynamic quantile model gives delta = 0.0548 at q10 (200-replication bootstrap $p = 0.002$), while the lagged emission-growth term is 0.2803 ($p < 0.001$), confirming strong persistence. Additional trading-day-only and seven-lag checks reported later in Section 4.10 are not based on the calendar construction or a one-day persistence specification. These alternative-estimator results are summarised in Table 2, and Figure 3 visualises the resulting coefficient profile.

Table 2. Bitcoin × EFP interaction, Russian power emissions, alternative estimators.

Specification	Coefficient (SE)	*p*-Value	N
q10, focal 1000-pairs bootstrap	0.0662 (0.0248)	0.008	2550
OLS, robust SE	0.0321 (0.0128)	0.012	2550
Dynamic q10, 1 lag (200 reps)	0.0548 (0.0179)	0.002	2547
Dynamic q25, 1 lag (200 reps)	0.0313 (0.0156)	0.044	2547
Dynamic q50, 1 lag (200 reps)	0.0214 (0.0115)	0.061	2547
Seven-lag q10 (qreg)	0.0509 (0.0200)	0.011	2531

The one-lag dynamic specification includes lagged emission growth; its q10 lag coefficient is 0.2803 ($p < 0.001$). The seven-lag specification includes lags t-1 through t-7, which are jointly significant ($p < 0.001$). The 1000-pairs bootstrap was implemented separately from Stata's sqreg and is reported for the focal q10 coefficient as a high-replication sensitivity check.

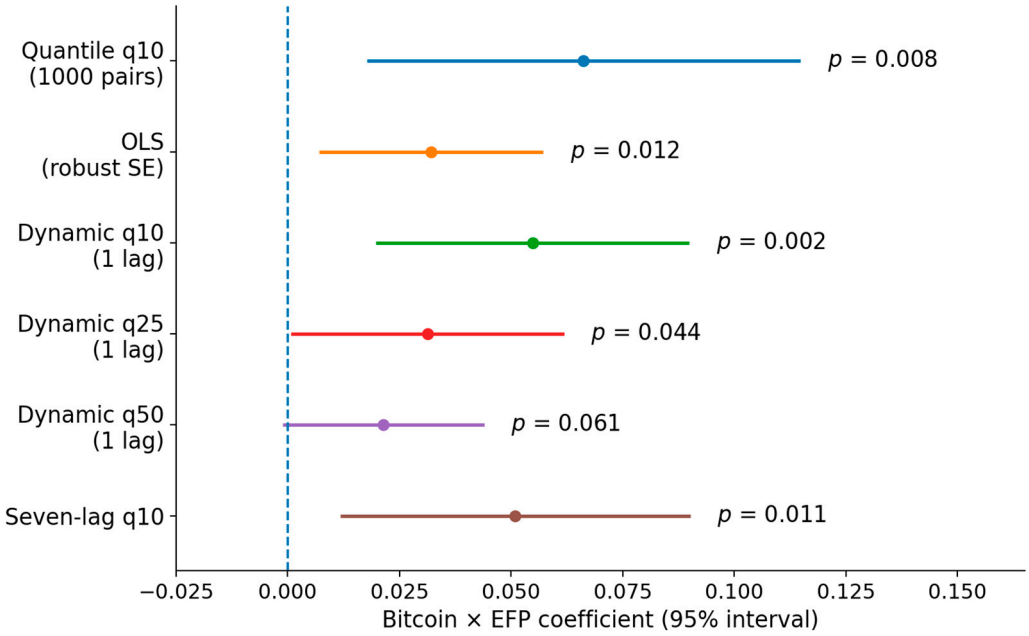


Figure 3. Russian focal Bitcoin × EFP interaction across alternative estimators. Points show coefficient estimates and horizontal bars show 95% intervals; *p*-values are reported for each specification.

That the association appears under OLS and dynamic specifications is useful evidence that it is not created solely by one quantile estimator. At the same time, robustness across estimators does not resolve omitted common shocks or location: the result remains an observational association that is sharper in the lower tail.

4.4. The Cross-Region Comparison Under a Common Estimator

Estimating the same OLS specification for all three regions provides a useful common-estimator comparison because it removes the choice of quantile and bootstrap draw. It remains an observational benchmark rather than a treated-versus-control design. Table 3 reports the common-estimator comparison, and Figure 4 visualises the corresponding coefficients and 95% confidence intervals.

Table 3. Bitcoin × EFP interaction under OLS with robust standard errors, all regions.

Region	Coefficient (SE)	*p*-Value	Model F-Test *p*
EU27	0.1153 (0.0660)	0.081	0.254
Russia	0.0321 (0.0128)	0.012	0.031
Rest of world	0.0176 (0.0207)	0.395	0.366

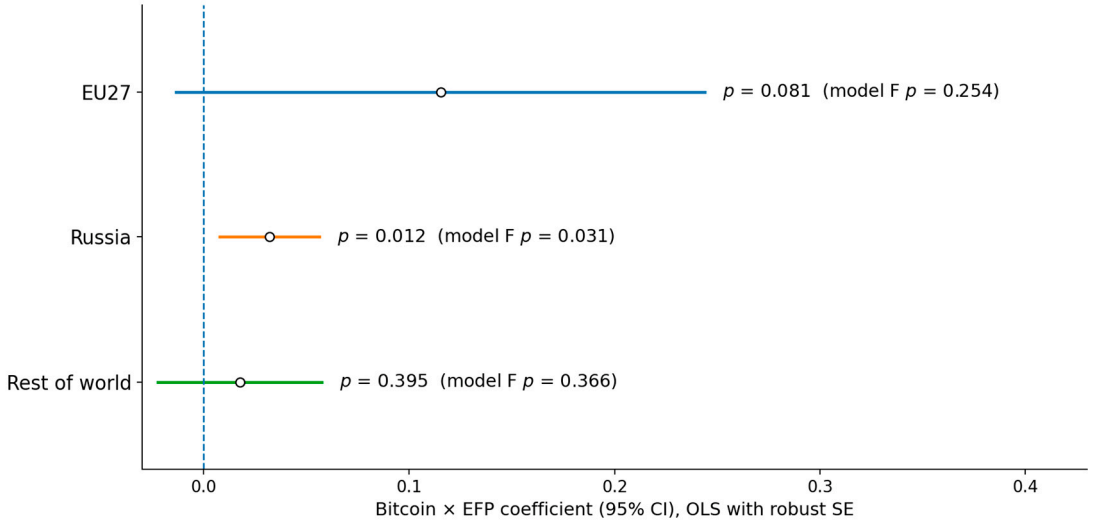


Figure 4. Bitcoin × EFP interaction under OLS with heteroskedasticity-robust standard errors across the EU27, Russia and rest of world. Points show coefficients and bars show 95% confidence intervals; model F-test *p*-values are shown alongside coefficient *p*-values.

Russia is the only region in which the OLS interaction is significant at conventional levels (beta = 0.0321, $p = 0.012$), and its model is jointly significant. EU27 is marginal at the coefficient level but not jointly significant; ROW is insignificant. The comparison is consistent with jurisdictional concentration, but heterogeneity across grids means it is not a causal counterfactual.

The same broad ordering appears in the dynamic q10 specification: the Russian interaction is positive and significant, while the corresponding EU27 and ROW q10 interactions are not. Persistence is strong in daily emissions across regions. This supports the need for dynamic controls, but it does not establish that the Russian coefficient represents displaced EU mining load.

We note, and do not overclaim, that these are contemporaneous daily associations. They establish a distributional and spatial signature consistent with leakage; they do not by themselves establish the physical relocation of specific mining capacity. Section 5.1 discusses what would be required to close that gap.

4.5. Timing Tests: The Association Predates the May 2021 Chinese Ban

The May 2021 Chinese mining prohibition is the primary mechanism-aligned timing test. Using 15 May 2021 as the split date, the Russian q10 interaction is 0.1824 before the ban ($p = 0.020$; N = 859) and 0.0582 after it ($p = 0.026$; N = 1691). The q50 interaction similarly falls from 0.0660 ($p = 0.001$) to 0.0060 ($p = 0.636$). Thus, the Russian association is already present before the Chinese exit and becomes smaller, not larger, afterwards. Table 4 reports the regime estimates, and Figure 5 visualises the same pre/post comparison.

Table 4. Russian Bitcoin × EFP interaction before and after the May 2021 Chinese mining prohibition.

Regime	q10 Interaction	q50 Interaction	q90 Interaction
Pre-15 May 2021 (N = 859)	0.1824 ($p = 0.020$)	0.0660 ($p = 0.001$)	0.0581 ($p = 0.472$)
Post-15 May 2021 (N = 1691)	0.0582 ($p = 0.026$)	0.0060 ($p = 0.636$)	−0.0342 ($p = 0.575$)
Formal q10 change	−0.0765 (robust OLS)	$p = 0.0053$	Post coefficient smaller

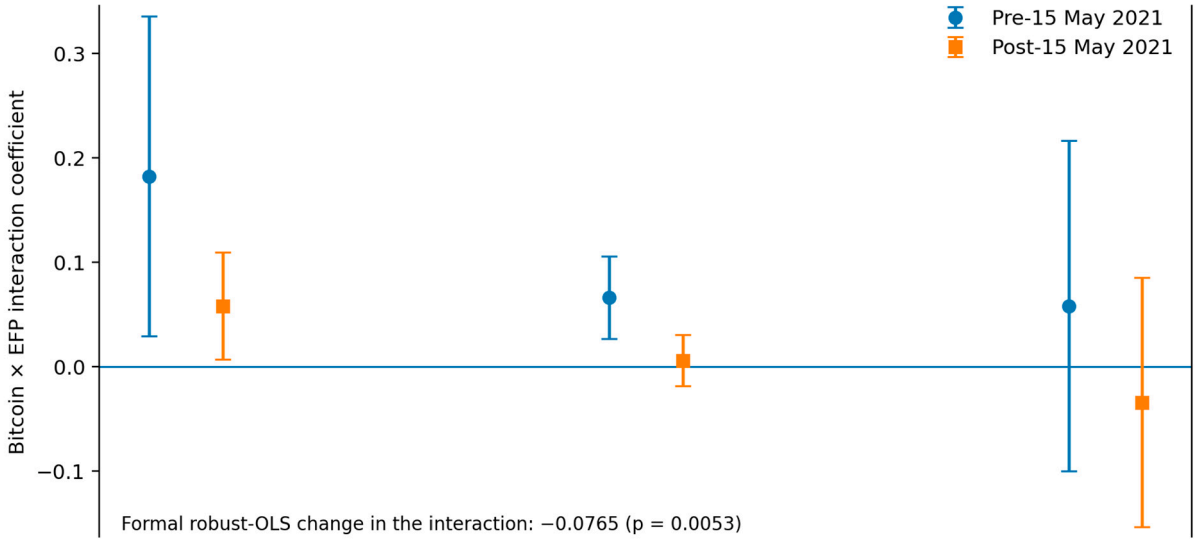


Figure 5. Russian Bitcoin × EFP interaction before and after the 15 May 2021 Chinese mining prohibition. Points show q10, q50 and q90 estimates with uncertainty intervals; the formal robust-OLS q10 interaction change is −0.0765 (p = 0.0053).

The formal fully interacted robust-OLS test estimates the post-ban change in the Bitcoin × EFP interaction at −0.0765 (p = 0.0053). This result rejects H2b as stated: the coefficient does not strengthen after the May 2021 relocation shock. The ban therefore does not explain the onset of the Russian association, but the timing evidence also does not support a simple narrative in which the post-China relocation episode amplified the estimated effect.

The earlier 11 March 2020 split is retained only as an auxiliary sensitivity check. With 200 bootstrap replications the Russian q10 interaction is −0.0016 before that date (p = 0.989; N = 431) and 0.0591 afterwards (p = 0.008; N = 2119). Because March 2020 is a pandemic break rather than a mining-policy event and the pre-period is short, we do not use it as causal evidence.

A further split on 24 February 2022, the start of Russia's invasion of Ukraine, is mixed rather than decisive: the Russian q10 interaction is 0.0840 before the break (p = 0.148) and 0.0651 afterwards (p = 0.046), while the median coefficient is significant before the break but not after it. This regime sensitivity reinforces the need to moderate causal attribution to the EU carbon-price channel.

4.6. Rest of the World: A Broad Falsification Benchmark

In the ROW specification, the interaction is insignificant at every quantile, with point estimates that change sign across the distribution (from +0.0358 at q10 to −0.0034 at q50 and +0.0590 at q90)—the profile of noise rather than of an economic relationship (Figure 2c). The null holds under every alternative estimator: OLS (p = 0.395, with the model not jointly significant at p = 0.366), the dynamic specification at all five quantiles (smallest p = 0.307), and both regime subsamples.

The ROW null is informative but not decisive. A broad global factor need not load identically on every grid, and aggregation can average away offsetting country responses. We therefore use ROW as a weak falsification benchmark rather than a causal control group. The null is compatible with jurisdictional concentration, but direct identification would require location-specific mining activity and more granular destination emissions.

4.7. Green Bond Control: Secondary and Economically Small

The Green Bond control is negative at several quantiles in the baseline results, including the median in the EU27 and Russia and q75 in Russia and ROW. Some of these coefficients also remain negative in OLS and dynamic specifications. However, their magnitudes are economically small, the pattern is secondary to the paper's leakage question, and the model does not identify how green-bond proceeds affect generation or investment. We therefore report these coefficients for specification continuity and transparency rather than as a separate test of a broad green-paradox mechanism. Figure 6 summarises the Green Bond coefficient profiles across regions and quantiles.

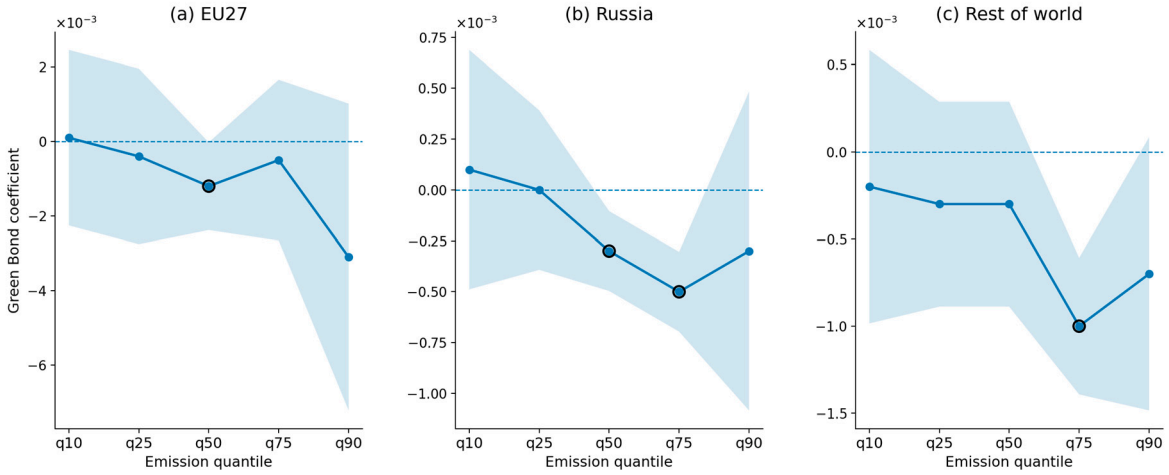


Figure 6. Green Bond coefficients across emission quantiles for (**a**) EU27, (**b**) Russia and (**c**) rest of world. Coefficients are small in magnitude and are treated as a secondary descriptive control rather than evidence of a separate green-paradox mechanism.

The negative Green Bond coefficients do not support the positive green-paradox interpretation proposed in the earlier framing, but they should not be over-read. The model does not identify the use of bond proceeds or the timing of clean-energy investment, and the coefficients are economically small. We therefore retain the variable mainly as a control and secondary descriptive result.

The magnitudes are two to three orders smaller than the focal interaction coefficients. Statistical significance in selected quantiles is therefore not treated as evidence of a broad green-finance mechanism.

4.8. Sensitivity of Inference to Bootstrap Replications

The low-replication diagnostic confirms why 20 bootstrap draws should not carry the main inference. Across three independent seeds, the Russian q10 coefficient remains significant, but the Russian median coefficient crosses conventional significance thresholds, and the EU27 q10 coefficient remains unstable. The focal q10 result is therefore also evaluated with 1000 pairs-bootstrap replications, while OLS, dynamic and trading-day checks provide inference that does not depend on one low-replication draw. Table 5 reports the three low-replication draws.

Table 5. Low-replication bootstrap sensitivity diagnostic (20 replications, three independent draws).

Coefficient (Point Estimate)	*p*, Draw 1	*p*, Draw 2	*p*, Draw 3
EU27 q10, BTC × EFP (0.2458)	0.310	0.110	0.192
Russia q10, BTC × EFP (0.0662)	0.003	0.006	0.002
Russia q50, BTC × EFP (0.0207)	0.028	0.140	0.114

The diagnostic separates a stable focal coefficient from borderline estimates. We do not use the Russian median or marginal EU27 results as central evidence. The principal claim is limited to the recurring Russian lower-tail association and is evaluated against the contradictory timing and substitution evidence reported below.

This is substantive rather than cosmetic: in a high-frequency quantile application, low-replication resampling can make individual *p*-values look more decisive than they are. Reporting the instability disciplines the number of claims made from the baseline table.

4.9. Quantile-on-Quantile Surfaces

Figure 7 plots the quantile-on-quantile coefficient β(θ, τ) for each region on the trading-day sample. The Russian surface is most positive in the high-carbon-price, low-emission corner: beta is approximately 0.00297 at (theta = 0.90, tau = 0.10) and 0.00229 at (theta = 0.75, tau = 0.10). Values are much smaller at low carbon-price quantiles. The EU27 surface changes sign across the plane, and the ROW surface is comparatively diffuse. The QQR exercise is descriptive and uses local weighting, so we treat the concentration in the Russian lower-left emission tail as corroborative pattern evidence, not as a separate causal identification strategy.

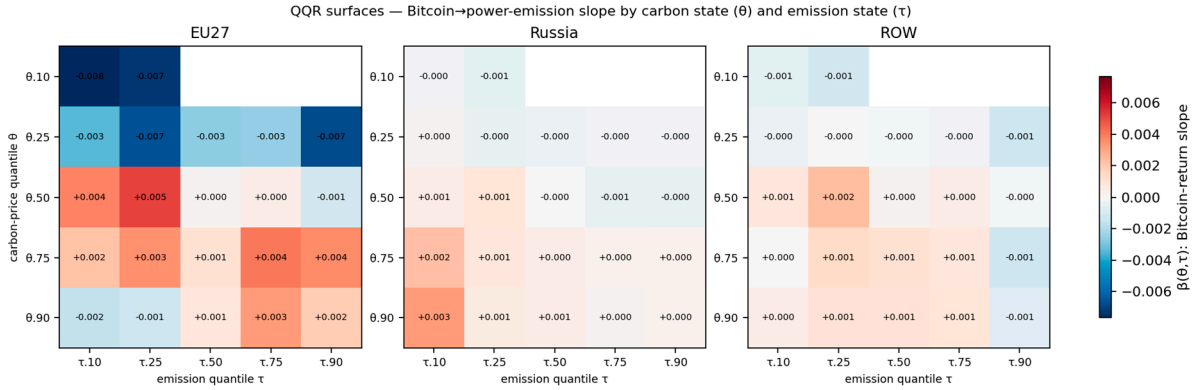


Figure 7. Quantile-on-quantile surfaces β(θ, τ) of the Bitcoin-return effect on power-sector emission growth, by carbon-price quantile θ and emission quantile τ, estimated on the trading-day sample. The Russian surface is most positive at high carbon-price and low emission quantiles; the surfaces are descriptive and do not identify relocation.

4.10. Additional Robustness, Outlier Sensitivity and Falsification Checks

Table 6 brings the completed sensitivity and falsification checks into the main paper. On the trading-day-only sample, which removes forward-filled EFP and WTI days, analytical qreg estimates with the full control set remain positive at q10 (0.0649, $p = 0.016$) and q50 (0.0233, $p = 0.040$). A seven-lag q10 specification also remains positive (0.0509, $p = 0.011$), with the emission lags jointly significant ($p < 0.001$). These results reduce concerns about calendar construction and one-day persistence. The outlier check is more cautionary: without 1st/99th percentile Winsorisation, the Russian q10 interaction remains positive (0.0364) but loses conventional significance ($p = 0.113$). We therefore describe the focal result as robust to several specification changes but sensitive to outlier treatment.

The falsification evidence is also mixed in the useful sense of constraining interpretation. Randomly permuting the carbon-price return produces no significant Russian interaction at q10 (0.0373, $p = 0.164$), q50 ($p = 0.780$) or q90 ($p = 0.733$). Russian residential and ground-transport placebo interactions are insignificant at q10 ($p = 0.786$ and $p = 0.373$), and the EU27-minus-Russia emission-growth diagnostic is null at the median (0.0332, $p = 0.581$). These nulls are compatible with a power-sector-specific association, but neither the placebos nor the direct substitution diagnostic establish displaced EU mining load.

Table 6. Additional robustness and falsification results reported in the main text.

Check	Interaction Estimate	*p*-Value	Reading
Trading-day only, Russia q10	0.0649	0.016	Survives calendar restriction
Trading-day only, Russia q50	0.0233	0.040	Positive at median
Seven-lag dynamic, Russia q10	0.0509	0.011	Survives t-1 to t-7 controls
Randomised carbon-return placebo, Russia q10	0.0373	0.164	Placebo null
Russia residential placebo, q10	0.0799	0.786	Non-power outcome null
Russia ground-transport placebo, q10	−0.0911	0.373	Non-power outcome null
EU27-minus-Russia differential, q50	0.0332	0.581	Direct substitution diagnostic null
No Winsorisation, Russia q10	0.0364	0.113	Positive sign; inference weakens

Trading-day estimates are analytical quantile regressions with the full control set on the usable trading-day sample after transformation. The randomised-carbon placebo uses the Stata 200-replication sqreg output. Residential and ground-transport placebos use the corresponding Carbon Monitor series in the authors' workbook. The seven-lag model controls for emission persistence through t-7. The no-Winsorisation row is reported because Reviewer 3 specifically requested outlier sensitivity; its loss of conventional significance is treated as a limitation rather than suppressed.

5. Discussion

5.1. Direct Evidence, Inference and the Leakage Mechanism

The central empirical fact is a recurring Russian lower-tail association, not an identified relocation event. The q10 Bitcoin × EFP interaction survives higher-replication inference, OLS, dynamic specifications, a trading-day restriction and several placebo checks. At the same time, it is already present before the May 2021 break and becomes smaller afterwards; the Ukraine split is mixed, the EU27-minus-Russia differential is null, and the coefficient loses conventional significance without Winsorisation. Supportive and contradictory results therefore have to be read together. They support further investigation of a leakage-consistent operational pattern, but not a claim that the regression has observed EU mining capacity moving into Russia.

This interpretation is consistent with the broader carbon-leakage literature, which distinguishes theoretical exposure from realised relocation. EU ETS studies frequently find smaller realised leakage than simple cost-differential models imply because fixed capital, free allocation, agglomeration, trade costs and other frictions matter (Martin et al. 2014a, 2014b, 2016; Naegele and Zaklan 2019; Verde 2020; Dechezleprêtre et al. 2022). Crypto-mining changes the mobility assumption but not the identification requirement: more footloose hardware makes relocation plausible, not automatically observed.

Operational versus capital margins. Daily coefficients are best interpreted as an operational-margin object: they can capture changes in utilisation of already-installed hashing capacity across grids as market conditions move. They are not a measurement of containers crossing a border, customs clearance, new facilities or new grid connections. The capital margin is slower and requires direct location evidence. Operational reallocation can still change where electricity is generated and where emissions occur, but calling the daily coefficient proof of physical relocation would exceed the evidence.

The results also need to be read against the crypto-mining location literature. Jiang et al. (2021), de Vries et al. (2022) and Sun et al. (2022) use mining-location, hashrate-geography or electricity-mix information to document geographic reallocation and environmental consequences. Those studies lie closer to Level 1 evidence than the present regressions. The failed May-2021 timing prediction is therefore substantive: if the Russian coefficient were a clean statistical trace of the documented post-China relocation episode, a stronger post-ban coefficient would have been expected. Its absence weakens that specific narrative even though the Russian q10 association remains statistically reproducible.

Common energy and macro-financial shocks provide a credible alternative channel. The 2022 energy crisis, the war in Ukraine, fuel-price movements and broader financial risk

can affect allowance prices, crypto prices and power-system emissions simultaneously. The energy-finance literature likewise documents nonlinear and time-varying connectedness among these markets (Tan et al. 2020; Corbet et al. 2021; Di Febo et al. 2021; Sarkodie et al. 2022; Zhang et al. 2023; Sibande et al. 2023). WTI, calendar controls, regime splits and lags reduce but do not eliminate this concern, especially because temperature, local electricity prices/load and renewable availability are absent from the supplied data.

Hashrate and direct mining activity. Bitcoin returns are an imperfect proxy for mining profitability and can also capture general risk appetite. A global daily network-hashrate series can test whether the result is tied to aggregate mining intensity, but global hashrate does not reveal whether activity is in the EU, Russia or another jurisdiction and therefore cannot identify an EU-to-Russia transfer. The strongest extension is location-specific hashrate, mining-pool/IP geolocation or facility-level electricity use matched to regional emissions. Until that information is available, the paper does not treat a global hashrate substitution as closure of the identification gap.

5.2. Robustness and Alternative Explanations

The robustness programme is most informative when supportive and contradictory tests are considered together. Supportive evidence includes the 1000-replication q10 bootstrap, OLS, the lagged-dependent-variable model, the trading-day-only sample, the seven-lag model and null randomised-carbon and non-power-sector placebos. These results make it less likely that the focal coefficient is solely a low-replication inference artefact, a forward-fill artefact, a one-day persistence artefact or a relationship that appears indiscriminately across Russian emission sectors.

The cautionary evidence is equally important. The coefficient loses conventional significance without Winsorisation; the formal May-2021 break moves opposite to H2b; the March-2020 split is not mechanism-specific and has a short pre-period; the February-2022 split is mixed; distributed timing does not trace a clean relocation response; and the EU27-minus-Russia substitution diagnostic is null. These findings rule out a stronger claim that the data reveal a clean origin-destination transfer or a uniquely dated relocation episode. Robustness strengthens confidence in the existence of the association while weakening confidence in any single causal explanation.

Quantile-on-quantile estimation adds a complementary description rather than new identification. The Russian surface is most positive at high carbon-price and low emission quantiles, whereas EU27 and ROW lack the same concentration. This is mechanism-aligned pattern evidence, but it is locally weighted and observational. Agreement between QQR and the interaction model is useful for functional-form robustness; it does not transform market comovement into exogenous variation.

5.3. Limitations and Scope Conditions

Measurement and explanatory power impose important limits. Baseline pseudo-R2 values are approximately 0.0006–0.0083, so the included financial variables explain only a small share of daily conditional emission variation. Carbon Monitor emissions are model-derived rather than metered; for Russia, high-frequency total generation is combined with modelled fuel disaggregation and emission factors. The dataset also lacks temperature, local electricity prices/load, renewable availability and other dispatch controls. These features are consistent with a small marginal-load signal but also create substantial scope for measurement error and omitted-variable bias.

Spatial aggregation within Russia is a second limit. The Russian power system contains regions with very different generation mixes, including hydro-rich Siberian systems and more fossil-reliant areas. Available public datasets do not provide one harmonised

daily Siberia-versus-European-Russia power-emissions series spanning the full 2019–2025 analysis window: Carbon Monitor Cities covers a shorter city-level period (Huo et al. 2022), GRACED provides gridded releases for limited years (Dou et al. 2023), and the constituent-entity inventory is annual and ends earlier (Xiao et al. 2021). Constructing a full matched regional series would therefore be a new data project rather than a mechanical rerun. We do not fabricate the missing decomposition.

The evidence boundary is consequently clear. ROW is a heterogeneous benchmark rather than a causal counterfactual; Bitcoin price is not location-specific mining load; the interaction does not establish the direction of relocation; and the focal Russian q10 inference is sensitive to outlier treatment. The paper's contribution is a reproducible lower-tail association, a transparent evidence hierarchy, and a set of falsification results that show both what survives and what fails. Causal displaced emissions remain a question for a dataset combining validated market exposure, local electricity conditions and direct mining-location measures.

6. Conclusions and Policy Implications

This paper studies whether daily power-sector emissions exhibit a distributional and spatial pattern consistent with, but not sufficient to establish, emissions displacement from a mobile electricity load. The strongest repeated result is a positive Bitcoin $\times$ EFP interaction in the lower tail of Russian power-emission growth. The association survives higher-replication q10 inference, OLS, dynamic specifications, a trading-day-only sample and persistence controls. At the same time, it is sensitive to outlier treatment, the May-2021 timing prediction fails, the Ukraine split is mixed, and the direct EU27-minus-Russia substitution diagnostic is null. The empirical contribution is therefore a bounded reduced-form finding rather than proof that mining capacity moved to Russia.

The policy discussion is intentionally modest because the design does not identify a broad policy effect. The estimates should not be read as tonnes of carbon leakage caused by the allowance price, as a causal estimate of EU ETS effectiveness, or as evidence that CBAM itself caused or failed to prevent crypto-mining relocation.

First, the study highlights a measurement problem for highly mobile electricity loads. Cross-market price-emissions interactions can indicate where an association warrants scrutiny, but policy attribution requires validated exposure measures and direct information on the location and utilisation of the load. Location-specific hashrate, facility-level electricity demand, grid-connection information, local electricity prices, weather and marginal-emissions data are the highest-value additions for future work.

Second, the broader carbon-leakage literature remains conceptually relevant to digital activities whose physical electricity demand can move even when their output is not a traded physical good. However, border measures, electricity-market rules, disclosure requirements and grid-connection policies operate through different channels and require separate causal evaluation. The present regressions do not identify which instrument would be effective.

Third, the robustness results show why policy claims should be proportional to the evidence. High-replication inference, trading-day checks and non-power placebos support the existence of a recurring Russian lower-tail association, whereas the failed timing prediction, outlier sensitivity and null direct-substitution diagnostic limit the relocation interpretation. The appropriate conclusion is therefore that the pattern merits further investigation with direct mining-location and subnational power-system data; stronger causal and policy claims should await that evidence.

Author Contributions: Conceptualization, P.N.T. and L.T.T.H.; methodology, P.N.T. and L.T.T.H.; software, formal analysis and visualization, L.T.T.H.; data curation, L.T.T.H. and N.V.T.N.; writing—

original draft, L.T.T.H.; writing—review and editing, P.N.T. and N.V.T.N.; supervision, P.N.T. All authors have read and agreed to the published version of the manuscript.

Funding: This research received no external funding.

Data Availability Statement: The emissions data analysed in this study are publicly available from Carbon Monitor. The market and financial series are contained in the authors' research workbook and replication files. The Stata 18 code that reproduces the reported analyses is available from the corresponding author upon reasonable request. Location-specific hashrate and a harmonised full-sample subnational Russian power-emissions series remain unavailable and are identified as future data extensions.

Conflicts of Interest: The authors declare no conflicts of interest.

References

Aldy, Joseph E., and William A. Pizer. 2015. The Competitiveness Impacts of Climate Change Mitigation Policies. *Journal of the Association of Environmental and Resource Economists* 2: 565–95. [CrossRef]

Baur, Dirk G., and Josua Oll. 2022. Bitcoin Investments and Climate Change: A Financial and Carbon Intensity Perspective. *Finance Research Letters* 47: 102575. [CrossRef]

Bayer, Patrick, and Michaël Aklin. 2020. The European Union Emissions Trading System Reduced CO_2 Emissions Despite Low Prices. *Proceedings of the National Academy of Sciences of the United States of America* 117: 8804–12. [CrossRef] [PubMed]

Böhringer, Christoph, Edward J. Balistreri, and Thomas F. Rutherford. 2012. The Role of Border Carbon Adjustment in Unilateral Climate Policy: Overview of an Energy Modeling Forum study (EMF 29). *Energy Economics* 34: S97–S110. [CrossRef]

Böhringer, Christoph, Jared C. Carbone, and Thomas F. Rutherford. 2016. The Strategic Value of Carbon Tariffs. *American Economic Journal: Economic Policy* 8: 28–51. [CrossRef]

Branger, Frédéric, and Philippe Quirion. 2014. Would Border Carbon Adjustments Prevent Carbon Leakage and Heavy Industry Competitiveness Losses? Insights from a Meta-Analysis of Recent Economic Studies. *Ecological Economics* 99: 29–39. [CrossRef]

Calel, Raphael, and Antoine Dechezleprêtre. 2016. Environmental Policy and Directed Technological Change: Evidence from the European Carbon Market. *Review of Economics and Statistics* 98: 173–91. [CrossRef]

Cludius, Johanna, Sander de Bruyn, Katja Schumacher, and Robert Vergeer. 2020. Ex-Post Investigation of Cost Pass-Through in the EU ETS: An Analysis for Six Industry Sectors. *Energy Economics* 91: 104883. [CrossRef]

Copeland, Brian R., and M. Scott Taylor. 2004. Trade, Growth, and the Environment. *Journal of Economic Literature* 42: 7–71. [CrossRef]

Corbet, Shaen, Brian Lucey, and Larisa Yarovaya. 2021. Bitcoin-Energy Markets Interrelationships—New Evidence. *Resources Policy* 70: 101916. [CrossRef]

Dechezleprêtre, Antoine, Caterina Gennaioli, Ralf Martin, Mirabelle Muûls, and Thomas Stoerk. 2022. Searching for Carbon Leaks in Multinational Companies. *Journal of Environmental Economics and Management* 112: 102601. [CrossRef]

de Vries, Alex. 2018. Bitcoin's Growing Energy Problem. *Joule* 2: 801–5. [CrossRef]

de Vries, Alex, Ulrich Gallersdörfer, Lena Klaaßen, and Christian Stoll. 2022. Revisiting Bitcoin's Carbon Footprint. *Joule* 6: 498–502. [CrossRef]

Di Febo, Elisa, Alessandra Ortolano, Matteo Foglia, Maria Leone, and Eliana Angelini. 2021. From Bitcoin to Carbon Allowances: An Asymmetric Extreme Risk Spillover. *Journal of Environmental Management* 298: 113384. [CrossRef] [PubMed]

Dittmar, Lars, and Aaron Praktiknjo. 2019. Could Bitcoin Emissions Push Global Warming Above 2 °C? *Nature Climate Change* 9: 656–57. [CrossRef]

Dou, Xinyu, Jinpyo Hong, Philippe Ciais, Frédéric Chevallier, Feifan Yan, Ying Yu, Yifan Hu, Da Huo, Yun Sun, Yilong Wang, and et al. 2023. Near-Real-Time Global Gridded Daily CO_2 Emissions 2021. *Scientific Data* 10: 69. [CrossRef] [PubMed]

Ellerman, A. Denny, Claudio Marcantonini, and Aleksandar Zaklan. 2016. The European Union Emissions Trading System: Ten Years and Counting. *Review of Environmental Economics and Policy* 10: 89–107. [CrossRef]

Ellerman, A. Denny, Frank J. Convery, and Christian de Perthuis. 2010. *Pricing Carbon: The European Union Emissions Trading Scheme*. Cambridge: Cambridge University Press.

European Parliament and Council. 2023. Regulation (EU) 2023/956 of 10 May 2023 Establishing a Carbon Border Adjustment Mechanism. *Official Journal of the European Union* L 130: 52–104.

Fabra, Natalia, and Mar Reguant. 2014. Pass-Through of Emissions Costs in Electricity Markets. *American Economic Review* 104: 2872–99. [CrossRef]

Fell, Harrison. 2010. EU-ETS and Nordic Electricity: A CVAR Analysis. *The Energy Journal* 31: 1–26. [CrossRef]

Feng, Xingdong, Xuming He, and Jianhua Hu. 2011. Wild Bootstrap for Quantile Regression. *Biometrika* 98: 995–99. [CrossRef] [PubMed]

Fischer, Carolyn, and Alan K. Fox. 2012. Comparing Policies to Combat Emissions Leakage: Border Carbon Adjustments Versus Rebates. *Journal of Environmental Economics and Management* 64: 199–216. [CrossRef]

Hahn, Jinyong. 1995. Bootstrapping Quantile Regression Estimators. *Econometric Theory* 11: 105–21. [CrossRef]

Hintermann, Beat. 2016. Pass-Through of CO_2 Emission Costs to Hourly Electricity Prices in Germany. *Journal of the Association of Environmental and Resource Economists* 3: 857–91. [CrossRef] [PubMed]

Hintermann, Beat, Sonja Peterson, and Wilfried Rickels. 2016. Price and Market Behavior in Phase II of the EU ETS: A Review of the Literature. *Review of Environmental Economics and Policy* 10: 108–28. [CrossRef]

Houy, Nicolas. 2019. Rational Mining Limits Bitcoin Emissions. *Nature Climate Change* 9: 655. [CrossRef]

Huo, Da, Xiaoting Huang, Xinyu Dou, Philippe Ciais, Yun Li, Zhu Deng, Yilong Wang, Duo Cui, Fouzi Benkhelifa, Taochun Sun, and et al. 2022. Carbon Monitor Cities Near-Real-Time Daily Estimates of CO_2 Emissions from 1500 Cities Worldwide. *Scientific Data* 9: 533. [CrossRef] [PubMed]

Jiang, Shangrong, Yuze Li, Quanying Lu, Yongmiao Hong, Dabo Guan, Yu Xiong, and Shouyang Wang. 2021. Policy Assessments for the Carbon Emission Flows and Sustainability of Bitcoin Blockchain Operation in China. *Nature Communications* 12: 1938. [CrossRef] [PubMed]

Jones, Benjamin A., Andrew L. Goodkind, and Robert P. Berrens. 2022. Economic Estimation of Bitcoin Mining's Climate Damages Demonstrates Closer Resemblance to Digital Crude than Digital Gold. *Scientific Reports* 12: 14512. [CrossRef] [PubMed]

Keppler, Jan Horst, and Maria Mansanet-Bataller. 2010. Causalities Between CO_2, Electricity, and Other Energy Variables During Phase I and Phase II of the EU ETS. *Energy Policy* 38: 3329–41. [CrossRef]

Koch, Nicolas, Sabine Fuss, Godefroy Grosjean, and Ottmar Edenhofer. 2014. Causes of the EU ETS Price Drop: Recession, CDM, Renewable Policies or a Bit of Everything? *Energy Policy* 73: 676–85. [CrossRef]

Koenker, Roger. 2005. *Quantile Regression*. Cambridge: Cambridge University Press.

Koenker, Roger, and Gilbert Bassett, Jr. 1978. Regression Quantiles. *Econometrica* 46: 33–50. [CrossRef]

Krause, Max J., and Thabet Tolaymat. 2018. Quantification of Energy and Carbon Costs for Mining Cryptocurrencies. *Nature Sustainability* 1: 711–18. [CrossRef]

Liu, Zhu, Philippe Ciais, Zhu Deng, Ruixue Lei, Steven J. Davis, Sha Feng, Bo Zheng, Duo Cui, Xinyu Dou, Biqing Zhu, and et al. 2020a. Near-Real-Time Monitoring of Global CO_2 Emissions Reveals the Effects of the COVID-19 Pandemic. *Nature Communications* 11: 5172. [CrossRef] [PubMed]

Liu, Zhu, Philippe Ciais, Zhu Deng, Steven J. Davis, Bo Zheng, Yilong Wang, Duo Cui, Biqing Zhu, Xinyu Dou, Piyu Ke, and et al. 2020b. Carbon Monitor, a Near-Real-Time Daily Dataset of Global CO_2 Emission from Fossil Fuel and Cement Production. *Scientific Data* 7: 392. [CrossRef] [PubMed]

Löschel, Andreas, Benjamin Johannes Lutz, and Shunsuke Managi. 2019. The Impacts of the EU ETS on Efficiency and Economic Performance: An Empirical Analysis for German Manufacturing Firms. *Resource and Energy Economics* 56: 71–95. [CrossRef]

Marin, Giovanni, Marianna Marino, and Claudia Pellegrin. 2018. The Impact of the European Emission Trading Scheme on Multiple Measures of Economic Performance. *Environmental and Resource Economics* 71: 551–82. [CrossRef]

Martin, Ralf, Mirabelle Muûls, and Ulrich J. Wagner. 2016. The Impact of the European Union Emissions Trading Scheme on Regulated Firms: What is the Evidence After Ten Years? *Review of Environmental Economics and Policy* 10: 129–48. [CrossRef]

Martin, Ralf, Mirabelle Muûls, Laure B. de Preux, and Ulrich J. Wagner. 2014a. Industry Compensation Under Relocation Risk: A Firm-Level Analysis of the EU Emissions Trading Scheme. *American Economic Review* 104: 2482–508. [CrossRef]

Martin, Ralf, Mirabelle Muûls, Laure B. de Preux, and Ulrich J. Wagner. 2014b. On the Empirical Content of Carbon Leakage Criteria in the EU Emissions Trading Scheme. *Ecological Economics* 105: 78–88. [CrossRef]

Masanet, Eric, Arman Shehabi, Nuoa Lei, Harald Vranken, Jonathan Koomey, and Jens Malmodin. 2019. Implausible Projections Overestimate Near-Term Bitcoin CO_2 Emissions. *Nature Climate Change* 9: 653–54. [CrossRef]

Mehling, Michael A., Harro van Asselt, Kasturi Das, Susanne Droege, and Cleo Verkuijl. 2019. Designing Border Carbon Adjustments for Enhanced Climate Action. *American Journal of International Law* 113: 433–81. [CrossRef]

Monjon, Stéphanie, and Philippe Quirion. 2011. Addressing Leakage in the EU ETS: Border Adjustment or Output-Based Allocation? *Ecological Economics* 70: 1957–71. [CrossRef]

Mora, Camilo, Randi L. Rollins, Katie Taladay, Michael B. Kantar, Mason K. Chock, Mio Shimada, and Erik C. Franklin. 2018. Bitcoin Emissions Alone Could Push Global Warming Above 2 °C. *Nature Climate Change* 8: 931–33. [CrossRef]

Mörsdorf, George. 2022. A simple fix for carbon leakage? Assessing the environmental effectiveness of the EU carbon border adjustment. *Energy Policy* 161: 112596. [CrossRef]

Naegele, Helene, and Aleksandar Zaklan. 2019. Does the EU ETS Cause Carbon Leakage in European Manufacturing? *Journal of Environmental Economics and Management* 93: 125–47. [CrossRef]

Osorio, Sebastian, Oliver Tietjen, Michael Pahle, Robert C. Pietzcker, and Ottmar Edenhofer. 2021. Reviewing the Market Stability Reserve in Light of More Ambitious EU ETS Emission Targets. *Energy Policy* 158: 112530. [CrossRef]

Parzen, Michael I., Lee-Jen Wei, and Zhiliang Ying. 1994. A Resampling Method Based on Pivotal Estimating Functions. *Biometrika* 81: 341–50. [CrossRef]

Pata, Ugur Korkut, Mustafa Tevfik Kartal, Zahoor Ahmed, and Avik Sinha. 2025. Relationship Between Green Bonds and Carbon Neutrality: Evidence from Top Five Emitting Countries' Sectoral CO_2 Emissions. *Financial Innovation* 11: 10. [CrossRef]

Perdana, Sigit, and Marc Vielle. 2022. Making the EU Carbon Border Adjustment Mechanism Work for Climate. *Energy Policy* 170: 113172. [CrossRef]

Sarkodie, Samuel Asumadu, Maruf Yakubu Ahmed, and Thomas Leirvik. 2022. Trade Volume Affects Bitcoin Energy Consumption and Carbon Footprint. *Finance Research Letters* 48: 102977. [CrossRef]

Sibande, Xolani, Riza Demirer, Mehmet Balcilar, and Rangan Gupta. 2023. On the Pricing Effects of Bitcoin Mining in the Fossil Fuel Market: The Case of Coal. *Resources Policy* 85: 103539. [CrossRef]

Siddique, Abubakar, Haitham Nobanee, Sitara Karim, and Farah Naz. 2023. Do Green Financial Markets Offset the Risk of Cryptocurrencies and Carbon Markets? *International Review of Economics & Finance* 86: 822–33. [CrossRef]

Sijm, Jos, Karsten Neuhoff, and Yihsu Chen. 2006. CO_2 Cost Pass-Through and Windfall Profits in the Power Sector. *Climate Policy* 6: 49–72. [CrossRef]

Sim, Nicholas, and Hongtao Zhou. 2015. Oil Prices, US Stock Return, and the Dependence Between Their Quantiles. *Journal of Banking & Finance* 55: 1–8. [CrossRef]

Sinn, Hans-Werner. 2008. Public Policies Against Global Warming: A Supply Side Approach. *International Tax and Public Finance* 15: 360–94. [CrossRef]

Sitarz, Joanna, Michael Pahle, Sebastian Osorio, Gunnar Luderer, and Robert Pietzcker. 2024. EU Carbon Prices Signal High Policy Credibility and Farsighted Actors. *Nature Energy* 9: 691–702. [CrossRef]

Stoll, Christian, Lena Klaaßen, and Ulrich Gallersdörfer. 2019. The Carbon Footprint of Bitcoin. *Joule* 3: 1647–61. [CrossRef]

Sun, Wei, Haitao Jin, Fengjun Jin, Lingming Kong, Yihao Peng, and Zhengjun Dai. 2022. Spatial Analysis of Global Bitcoin Mining. *Scientific Reports* 12: 10694. [CrossRef] [PubMed]

Tan, Xueping, Kavita Sirichand, Andrew Vivian, and Xinyu Wang. 2020. How Connected is the Carbon Market to Energy and Financial Markets? A Systematic Analysis of Spillovers and Dynamics. *Energy Economics* 90: 104870. [CrossRef]

Taylor, Michael Bedford. 2017. The Evolution of Bitcoin Hardware. *Computer* 50: 58–66. [CrossRef]

Verde, Stefano F. 2020. The impact of the EU Emissions Trading System on Competitiveness and Carbon Leakage: The Econometric Evidence. *Journal of Economic Surveys* 34: 320–43. [CrossRef]

Xiao, Huijuan, Weichen Zhao, Yuli Shan, and Dabo Guan. 2021. CO_2 Emission Accounts of Russia's Constituent Entities 2005–2019. *Scientific Data* 8: 172. [CrossRef] [PubMed]

Zaevski, Tsvetelin S., and Dragomir C. Nedeltchev. 2023. From BASEL III to BASEL IV and Beyond: Expected Shortfall and Expectile Risk Measures. *International Review of Financial Analysis* 87: 102645. [CrossRef]

Zhang, Dongna, Xihui Haviour Chen, Chi Keung Marco Lau, and Bing Xu. 2023. Implications of Cryptocurrency Energy Usage on Climate Change. *Technological Forecasting and Social Change* 187: 122219. [CrossRef]

Zhu, Biqing, Zhu Deng, Xuanren Song, Wenli Zhao, Da Huo, Taochun Sun, Piyu Ke, Duo Cui, Chenxi Lu, Haiwang Zhong, and et al. 2023. CarbonMonitor-Power Near-Real-Time Monitoring of Global Power Generation on Hourly to Daily Scales. *Scientific Data* 10: 217. [CrossRef] [PubMed]

Disclaimer/Publisher's Note: The statements, opinions and data contained in all publications are solely those of the individual author(s) and contributor(s) and not of MDPI and/or the editor(s). MDPI and/or the editor(s) disclaim responsibility for any injury to people or property resulting from any ideas, methods, instructions or products referred to in the content.