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Does International Climate Finance Produce Green Innovation? Estimation Weights and a 540-Specification Curve for 118 Developing Economies

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Abstract

International climate finance is widely presumed to accelerate green technological innovation in recipient economies, and cross-country panel studies report positive effects. This paper shows that such estimates are governed by how nonlinear fixed-effects estimators weight a highly concentrated panel. We separate each country's influence into a *likelihood weight*, which governs estimation of the year effects and control slopes, and an *identifying weight*, which governs the coefficient of interest, and show that the two diverge when the outcome is concentrated. Using OECD ENV-TECH patent data and OECD DAC Rio Marker mitigation commitments for 118 developing economies over 2004–2022, we estimate 540 specifications spanning two estimators, three treatment scalings, five denominator floors, two trend structures, three time-control schemes, and three samples. China holds 80.1% of the sample's green patent families and 80.1% of the count estimator's likelihood weight, but only 3.7% of its identifying weight; the count and share estimators identify the coefficient from almost disjoint sets of economies. The baseline estimate of 0.113 ($p = 0.011$) falls to 0.039 when China is merely allowed its own control slopes, and to 0.020 when it is excluded. Across the curve, 96.9% of estimates are positive and 44.4% significant, but significance requires retaining a dominant inventor, admitting denominators below five inventions, or omitting year effects. Among the 48 specifications free of all three, none is significant. Claims that bilateral climate finance delivers the technological upgrading envisaged under Sustainable Development Goal 8 remain unproven.

Keywords Green finance · Climate finance · Green innovation · Estimation weights · Specification-curve analysis · Developing economies

JEL Classification O31 · O33 · Q54 · Q55 · F35 · C23

1 Introduction

Mobilising financial flows toward low-carbon technology is a central pillar of the international climate regime. Developed economies committed at Copenhagen and Cancún to jointly mobilise USD 100 billion per year for climate action in developing countries, and Article 2(1)(c) of the Paris Agreement commits all parties to "making finance flows consistent with a pathway towards low greenhouse gas emissions" [1]. Underlying these commitments is a theory of change in which external green finance does more than fund the deployment of existing equipment: it is expected to build domestic technological capabilities, redirect inventive effort toward environment-related technologies, and thereby generate the self-sustaining innovation dynamics emphasised by the directed-technical-change literature [2, 3]. Whether international climate finance actually produces green *innovation*, as distinct from green *investment*, is therefore a first-order empirical question for sustainable finance. It bears directly on Sustainable Development Goal 8, whose targets call for higher productivity through "diversification, technological upgrading and innovation" (target 8.2) and for efforts to "decouple economic growth from environmental degradation" (target 8.4) [4]. If external climate finance builds domestic green inventive capacity, it contributes to the innovation-led, environmentally sustainable growth path that SDG 8 envisages; if it does not, that path must be financed through other channels.

A growing empirical literature answers the question in the affirmative. Studies of domestic green credit and green bond programmes report improvements in environmental performance and green innovation [5, 6], and recent cross-country work extends the claim to international flows. Bakry et al. [7] find that international green finance raises green innovation across 76 developing economies once flows exceed a threshold, with green innovation in turn reducing CO_2 emissions. Comparable panel studies typically employ 30–80 countries, patent-count outcomes, and report positive and statistically significant coefficients.

This paper argues that these estimates are shaped less by the finance–innovation relationship than by an econometric property of the panels in which it is estimated, and it develops the theory needed to see why. Among the developing economies eligible for bilateral climate finance, green patenting is extraordinarily concentrated: in the sample assembled here *China alone accounts for 80.1% of environment-related patent families and 78.7% of all families, and China and India together for 89.1%*, even though China holds only 10.7% of the world total. This concentration is invariant to the denominator restrictions imposed below and has risen over the sample period, from 57.4% in 2004–2009 to 87.5% in 2016–2022. It is a

structural feature of the recipient set, not a measurement artefact: the class of economies eligible for development finance excludes every advanced patenting nation and leaves one large inventor inside it.

Section 2.5 develops the consequence formally. In any weighted nonlinear fixed-effects estimator, a country influences the treatment coefficient through two distinct channels that the literature has not distinguished. The first is its *identifying weight*: the share of the weighted within-country variance of the residualised treatment that it supplies, which determines its contribution to the variance-weighted average of unit-specific slopes that the pooled coefficient represents [8, 9]. The second is its *likelihood weight*: the share of the total estimation weight it supplies, which determines its contribution to the nuisance parameters, the year effects and control slopes that define the residualised treatment for *every other* country. Proposition 1 establishes that these two weights coincide only when the weighted within-variance of the treatment residual is proportional to the estimation weight across units, a condition that fails systematically in a concentrated panel. A unit with negligible identifying weight can therefore determine the pooled estimate through the nuisance channel alone.

The data bear this out exactly. Under the count estimator China holds 80.1% of the likelihood weight but 3.7% of the identifying weight; the largest identifying weight belongs to Türkiye (21.6%). Under the share estimator the likelihood weight is nearly uniform, yet the identifying weight concentrates in a different set of economies entirely: Mauritius (13.6%), Azerbaijan, Bosnia and Herzegovina, Armenia. Of the ten economies carrying the most identifying weight under each estimator, only one, Morocco, appears in both lists. The two estimators that the literature treats as alternative implementations of the same question in fact identify the coefficient from almost disjoint sets of countries, and neither set is the one the pooled estimate appears to describe. The corresponding predictions are confirmed directly: allowing China its own control slopes, so that it no longer determines the nuisance parameters while remaining in the sample, cuts the baseline estimate from 0.113 to 0.039; allowing it its own *treatment* slope, so that it retains the nuisance channel, leaves the pooled coefficient at 0.109.

Rather than selecting one specification and defending it, the study then subjects the hypothesis to a specification-curve analysis [10, 11] over 540 models spanning two estimators, three treatment scalings, five denominator floors, two trend structures, three schemes for common time variation, and three samples. The method operationalises a concern as old as Leamer [12]: that reported estimates reflect specification searching as much as data, and that credibility requires displaying fragility rather than concealing it [13]. Including the

time-control dimension proves essential. Of the 240 significant estimates, 99 come from specifications with a common linear trend and 78 from specifications with no common time control, against 63 from specifications with year fixed effects; excluding China, the count model is significant in 42 of 60 specifications without year effects and in 1 of 30 with them. Studies that do not absorb common time shocks recover the coincident global rise in both green patenting and climate finance.

The findings follow. Of 540 estimates, 96.9% are positive and none is significantly negative, a pattern consistent with a genuinely non-negative association. But the 240 significant estimates concentrate in exactly the three regions the theory predicts: samples retaining a dominant inventor, denominator floors below five inventions, and time specifications that leave common trends unabsorbed. Among the 48 specifications that exclude both dominant inventors, require at least five total inventions, and scale the treatment by country size, not one is significant at the 5% level; the median standardised effect is 0.044 with a median standard error of 0.037, implying a minimum detectable effect of 0.104.

The paper makes three contributions. First, it provides a theoretical account, formalised in Proposition 1 and its corollary and tested directly, of how concentration in the outcome distribution determines what a nonlinear fixed-effects estimator identifies, and it supplies a weight decomposition that any applied study can compute. Second, it applies that account to the international green finance–green innovation relationship and shows that the published consensus is reproduced by, and only by, the weighting channels the theory isolates. Third, it demonstrates the value of specification-curve analysis for sustainable-finance research, a field in which measurement choices are numerous, consequential, and rarely examined jointly.

Two clarifications delimit the claim. The analysis concerns bilateral, mitigation-marked official flows and patented invention; it does not speak to the effects of climate finance on technology *deployment*, adaptation outcomes, or emissions, nor to multilateral flows through the Green Climate Fund and development banks, which the Rio Marker dataset does not cover. And a null result is not a demonstration that no effect exists: the power calculations reported below show the design can detect only comparatively large effects. The conclusion is that the *evidence base* for the innovation channel is far weaker than the literature suggests, not that the channel is proven absent.

The remainder of the paper proceeds as follows. Section 2 reviews the literature, develops the identification theory, and states the hypotheses. Section 3 describes the data. Section 4 sets

out the estimators, the weight decomposition, and the specification-curve design. Section 5 presents the results. Section 6 discusses implications and limitations, and Section 7 concludes.

2 Theory and hypotheses

2.1 Green finance and the direction of technical change

The theoretical case for expecting finance to shape the direction of innovation is well established. In the directed-technical-change framework of Acemoglu et al. [2], the allocation of research effort between clean and dirty technologies responds to relative profitability, which policy and finance can tilt; Popp [3] provides the canonical evidence that energy prices induce energy-efficient patenting. Green finance instruments such as concessional climate finance, green credit, and green bonds are commonly framed as mechanisms that lower the cost of capital for clean activities and thereby redirect inventive effort [14]. At the firm level, Flammer [5] shows that corporate green bond issuance is followed by improvements in environmental performance, and a large literature, reviewed bibliometrically by Zhang et al. [6], documents associations between green financial development and green patenting, predominantly in Chinese data. Evidence from emerging economies points in the same direction but also to marked heterogeneity: green innovation is an important lever for emissions reduction in the emerging seven (E7) economies [15], while the effects of green finance on energy efficiency across developing countries are concentrated in the upper quantiles of the outcome distribution rather than spread evenly across it [16].

For developing economies specifically, the presumed channel runs through international flows. Domestic financial systems in low- and middle-income countries are often too shallow to fund risky, long-horizon green R&D, so concessional external finance is expected to relax the binding constraint [14]. Bakry et al. [7] formalise and test this proposition for 76 developing countries over 2000–2019, reporting a nonlinear long-run relationship in which international green finance raises green innovation once flows pass a threshold, together with an inverted-U pattern and a subsequent emissions-reducing effect of green patents. The present study is a direct examination of the robustness of this class of finding, using an overlapping country set, a closely related treatment measure, and a deliberately exhaustive specification space.

2.2 Absorptive capacity and the scale of the domestic innovation system

Directed technical change explains why finance might redirect inventive effort, but not whether a given economy possesses inventive effort to redirect. The absorptive-capacity literature supplies that condition. Cohen and Levinthal [17] argue that an organisation's ability to recognise, assimilate, and commercially apply external knowledge is itself a function of prior related investment, so that the return to an external knowledge or finance inflow is increasing in the recipient's existing stock of research capability. Transposed to the country level, the implication is a complementarity rather than a substitution: external climate finance raises patentable invention where a domestic research base, an industrial sector able to absorb the technology, and an intellectual-property infrastructure already exist, and funds deployment of imported equipment where they do not.

The innovation-geography evidence is consistent with this reading. Dechezleprêtre et al. [18] document that climate-mitigation inventions originate overwhelmingly in a small number of countries, with most developing economies participating in technology *transfer* rather than invention. Among the economies eligible for bilateral climate finance, the implication is sharp: the marginal recipient is not a scaled-down inventor but a non-inventor, for which the theoretically predicted innovation response is close to zero regardless of the flow received. The same fact that makes this prediction plausible, extreme concentration of inventive activity, is what creates the econometric hazard analysed in Section 2.5.

2.3 Measuring climate finance: the Rio Marker system

The standard source for cross-country climate finance data is the OECD Development Assistance Committee (DAC) Creditor Reporting System, in which donors tag each aid activity with Rio Markers indicating whether climate change mitigation (or adaptation) is a "principal" objective, a "significant" objective, or not targeted. The system's limitations are well documented. Michaelowa and Michaelowa [19] show that marker coding responds to donor political incentives, generating systematic over-reporting; Weikmans and Roberts [20] describe the resulting accounting as a "muddle" in which whole-project values are attributed to climate even when only components are climate-relevant; and Toetzke et al. [21], using a machine-learning classifier over 2.7 million project descriptions, conclude that actual bilateral climate finance is likely far below Rio Marker totals. These critiques motivate two design choices: the baseline treatment uses only *principal*-marked commitments, the conservative measure recommended by this literature, and the fraction of a recipient's aid never screened against the marker is tracked and used as a robustness restriction.

2.4 Measuring green innovation: environment-related patents

Green innovation is proxied, as is standard, by patents in environment-related technologies (ENV-TECH), identified by the OECD from the Cooperative Patent Classification following Haščič and Migotto [22]. Patent indicators have attractive properties (they are output-oriented, commensurable across countries, and finely disaggregable) but also known weaknesses: propensity to patent varies across countries and sectors, many inventions are never patented, and simple counts conflate quality tiers [22]. The OECD indicators mitigate the quality problem by counting patent *families* filed in a minimum number of jurisdictions; filing abroad is costly and signals expected commercial value. The analysis below uses the two-or-more-offices family definition, which reproduces the OECD's published green-share indicator exactly.

2.5 What a nonlinear fixed-effects estimator identifies in a concentrated panel

Consider a generalised linear model with a canonical link, estimated with country and year fixed effects:

$$\mathbb{E}[y_{it} \mid \cdot] = h(\eta_{it}), \qquad \eta_{it} = \beta x_{it} + \boldsymbol{\gamma}'\mathbf{z}_{it} + \alpha_i + \tau_t + o_{it}, \tag{1}$$

where x is the treatment, $\mathbf{z}$ the controls, and o a known offset. The quasi-likelihood is maximised by iteratively reweighted least squares: at convergence the parameter vector solves a weighted least-squares problem in the working response ζ with weights

$$w_{it} = \frac{\partial \mu_{it}}{\partial \eta_{it}} = \begin{cases} \mu_{it} & \text{(log link, Poisson variance)} \\ \mu_{it}(1-\mu_{it}) & \text{(logit link, binomial variance).} \end{cases} \tag{2}$$

Applying the Frisch–Waugh–Lovell theorem in the w-metric, let $\tilde{x}_{it}$ be the residual from the w-weighted projection of x on the controls and the two sets of fixed effects, and define country i's own weighted slope and the weighted within-country variance of its residualised treatment as

$$\hat{\beta}_i = \frac{\sum_t w_{it}\,\tilde{x}_{it}\tilde{\zeta}_{it}}{\sum_t w_{it}\,\tilde{x}_{it}^2}, \qquad s_i^2 = \frac{\sum_t w_{it}\,\tilde{x}_{it}^2}{\sum_t w_{it}}. \tag{3}$$

Two distinct shares then govern country i's influence:

$$\omega_i^{\mathrm{ID}} = \frac{\sum_t w_{it}\,\tilde{x}_{it}^2}{\sum_j \sum_s w_{js}\,\tilde{x}_{js}^2}, \qquad \omega_i^{\mathrm{LK}} = \frac{\sum_t w_{it}}{\sum_j \sum_s w_{js}}. \tag{4}$$

We call ω_i^{ID} the *identifying weight* and ω_i^{LK} the *likelihood weight*. The first governs the treatment coefficient, since $\hat{\beta} = \Sigma_i \omega_i^{\mathrm{ID}} \hat{\beta}_i$: the pooled estimate is a variance-weighted average of

country-specific slopes, the nonlinear analogue of the weighting results of Solon et al. [9] and Gibbons et al. [8]. The second governs the *nuisance* parameters: the year effects τ_t and the control slopes $\boldsymbol{\gamma}$ solve weighted normal equations in which country i enters in proportion to ω_i^{LK}. Because those nuisance parameters define $\tilde{x}_{it}$ for every country, the likelihood weight is a second, indirect channel of influence on $\hat{\beta}$ that operates even for a country with negligible identifying weight.

Proposition 1 (weight divergence). *With s_i^2 as defined in Eq. (3),*

$$\omega_i^{\mathrm{ID}} = \frac{\omega_i^{\mathrm{LK}}\, s_i^2}{\sum_j \omega_j^{\mathrm{LK}}\, s_j^2}, \qquad (5)$$

so $\omega_i^{\mathrm{ID}} = \omega_i^{\mathrm{LK}}$ for all i if and only if s_i^2 is constant across countries. Under the log link $w_{it} = \mu_{it}$, so ω_i^{LK} is the country's share of predicted outcome mass; under the logit link the weight $\mu_{it}(1-\mu_{it})$ is bounded above by 0.25, so ω_i^{LK} is approximately the country's share of observations. When outcome mass is concentrated in one country whose treatment varies little relative to others, ω^{LK} approaches unity for that country while ω^{ID} remains small.

The proof is immediate from Eqs. (3) and (4). The proposition has three consequences that motivate the empirical design. First, the intuition that a dominant country "drives" a count regression is correct about the likelihood weight and wrong about the identifying weight; the two must be reported separately. Second, because the logit weight is bounded, the fractional-response estimator equalises likelihood weights but does not equalise identifying weights, which migrate instead to country-years whose share is measured with the greatest relative error, namely those with very small denominators. Neither estimator, therefore, identifies β from the middle of the recipient distribution where the policy question lives. Third, the two channels can be separated experimentally, which yields a test.

Corollary 1 (common-parameter test). *Suppose $\beta_i = \beta$ for all i. Then (a) $\hat{\beta}$ is consistent for β whatever the weights ω^{ID}; (b) the estimate obtained after deleting any single country is consistent for β; and (c) the estimate obtained after replacing the common control slopes by country-specific slopes for the dominant country is consistent for β. A pooled estimate that fails (b) exhibits slope heterogeneity concentrated in the deleted country; a pooled estimate that fails (c) is determined by that country's contribution to the nuisance parameters rather than by its own slope.*

Tests (b) and (c) are distinct, and their pattern is informative. If a dominant country influences $\hat{\beta}$ only through its own slope, freeing that slope while holding the nuisance parameters

common should remove its influence; if it influences $\hat{\beta}$ through the nuisance channel, freeing its slope will not, but freeing its control slopes will. Section 5.3 implements all three tests.

2.6 Analytical robustness and the garden of forking paths

The empirical strategy draws on the methodological literature on researcher degrees of freedom. Leamer [12] argued that regression evidence is credible only when its sensitivity to specification is displayed; Simmons et al. [13] demonstrated how undisclosed flexibility inflates false-positive rates; Steegen et al. [11] proposed multiverse analysis as a remedy; and Simonsohn et al. [10] formalised specification-curve analysis as a three-step procedure: enumerate the defensible specifications, display the estimates jointly, and conduct inference across the curve. Sustainable-finance empirics are unusually exposed to these concerns because the field's core constructs ("green finance", "green innovation") each admit multiple defensible measurements, and because published results overwhelmingly report a single preferred specification. To our knowledge, this is the first application of specification-curve analysis to the climate finance–innovation nexus, and the first to include the estimator's weighting structure among the dimensions varied.

2.7 Hypotheses

The two strands above generate four testable propositions, the first two substantive and the second two econometric.

H1. Bilateral climate-mitigation finance raises green inventive output in recipient economies, conditional on total inventive activity [2, 7].

H2. Any such effect is increasing in the recipient's absorptive capacity, so it is negligible among economies without an established inventive base [17, 18].

H3. In a panel whose outcome mass is concentrated, the likelihood and identifying weights of a nonlinear fixed-effects estimator diverge, and count- and share-based estimators identify β from substantially different sets of economies (Proposition 1).

H4. If the pooled estimate recovers a common parameter, it is invariant to deleting the dominant country and to freeing that country's control slopes; failure of the second test in particular indicates that the estimate is determined through the nuisance channel (Corollary 1).

H3 and H4 are prior to H1 in the logical order: what the data say about H1 depends on which specifications the answer to H3 and H4 leaves standing. Section 5 therefore tests H3 and H4 first.

3 Data and variables

3.1 Sample construction

The unit of observation is the recipient-country–year. The sample is the intersection of (i) economies appearing as recipients in the OECD DAC Rio Marker dataset and (ii) economies with at least one patent family recorded in the OECD ENV-TECH data in a given year. Merging the two sources and requiring the baseline covariates and two-year treatment lags yields an unbalanced panel of *118 economies over 2004–2022* in the baseline count specification (1,624 observations). The same estimation sample is used for every treatment scaling, so that differences across the specification curve reflect analytical choices rather than changes in sample composition. The sample period is bounded below by the availability of consistently coded Rio Marker data (from 2002, so that two-year lags begin in 2004) and above by the World Development Indicators renewable-energy series, which is unavailable for 2023. Appendix A reports the full attrition sequence from the raw merged panel (2,396 observations, 147 economies) to the lag-complete sample of 1,682 observations and 132 economies, together with that sample's income and regional composition: 20 low-income, 43 lower-middle-income, 55 upper-middle-income, and 14 high-income (aid-graduating) economies, with Sub-Saharan Africa (42) and Latin America and the Caribbean (31) the largest regional groups. Coverage is stable at 83–99 economies per year through 2021 but falls to 16 in 2022, the final sample year, reflecting the reporting lag in the patent data; China's last observation is 2021 (Appendix C).

3.2 Dependent variable: green innovation

Green innovation is measured from the OECD *Patents: Technology Development* dataset (OECD Environment Statistics, ENV-TECH classification), extracted via the OECD Data Explorer SDMX API [23]. Two series are used: patent families in *environment-related technologies* and patent families in *all technologies*, both counted by *inventor country*, using *fractional counts* (a family with inventors in several countries is apportioned across them) and the *two-or-more-offices* family-size definition, which restricts attention to inventions their owners considered valuable enough to protect in multiple jurisdictions [22].

Two construction details are consequential. First, the OECD suppresses zero cells rather than reporting them: a country-year with inventions but no *green* inventions is absent from the environment-related series. The dependent variable is therefore reconstructed from counts: a country-year present in the all-technologies series but absent from the environment-related

series is assigned a true zero. In the full OECD panel (212 economies, 1990–2023) this reconstruction recovers 36% of country-years as genuine zeros; in the lag-complete sample the share is 34.7% (Appendix C), concentrated among low-income countries (60% of their country-years) and essential for any analysis of where green finance fails to produce innovation, since those country-years are precisely the low-absorptive-capacity cases H2 concerns. Second, the reconstruction is validated against the OECD's own published green-share indicator (*Patents: Indicators*, "percentage of technologies"): across all overlapping country-years the two series agree to within 7×10^{-8}, confirming that the denominator and family-size conventions match the OECD's exactly.

Three outcome variables result: the green patent-family count ($GreenInv_{it}$), the total count ($TotalInv_{it}$), and the green share ($GreenShare_{it} = GreenInv_{it}/TotalInv_{it}$).

3.3 Treatment variable: international climate-mitigation finance

The treatment is bilateral official development finance marked with the Rio Marker for *climate change mitigation*, from the OECD DAC "RioMarkers" dataset [24]: commitments by all DAC donors, all sectors, constant 2024 USD. Following the measurement literature [19, 21], the baseline uses *principal-objective* commitments only; principal-plus-significant totals serve as a robustness measure. All four marker scores (principal, significant, screened-not-targeted, not screened) were downloaded, which permits two additional constructions: recipient-years receiving screened aid but no mitigation-marked aid are assigned true zeros (19.5% of recipient-years in the merged panel and 15.2% in the lag-complete sample for the principal marker), and the share of a recipient's aid never screened against the marker is computed as a data-quality indicator (median 13.2% in the estimation sample; a robustness restriction drops recipient-years at or above 30%, about one-third of the sample).

Because the finance data are in constant 2024 USD while GDP is in constant 2015 USD, the finance series is rebased to 2015 prices using the US GDP implicit price deflator (FRED series `GDPDEF`; conversion factor 1.2888) before any ratio is formed. Three scalings of the resulting flow are used throughout: *per capita* (baseline), *per cent of GDP*, and *constant USD* (log of the unscaled flow). All enter as $\log(1 + x)$, given the heavy right skew of the flows (Appendix C), and, in the baseline, at a two-year lag, reflecting research-to-filing delays in patenting.

3.4 Covariates

Baseline controls, all from the World Bank World Development Indicators [25] unless noted, are: log GDP per capita (constant 2015 USD; `NY.GDP.PCAP.KD`), FDI net inflows as a share of GDP (`BX.KLT.DINV.WD.GD.ZS`), and the renewable share of final energy consumption (`EG.FEC.RNEW.ZS`), the last capturing demand-pull for green technology. Extended specifications add domestic credit to the private sector (`FS.AST.PRVT.GD.ZS`), trade openness (exports plus imports over GDP), and gross tertiary enrolment (`SE.TER.ENRR`); all three are excluded from the baseline because of missing data (14% of country-years for private credit, 13% for trade openness, and 34% for tertiary enrolment), concentrated among the poorest countries. Institutional quality is measured by the Worldwide Governance Indicators rule-of-law estimate (`GOV_WGI_RL.EST`) [26], used in the exploratory moderation analysis of Section 5.6. Population (`SP.POP.TOTL`) enters the per-capita scaling. Table 1 summarises all variables, sources, and constructions, and Table 2 reports descriptive statistics. Pairwise correlations among the regressors are moderate: the largest are between log GDP per capita and the renewable share ($r = -0.69$) and between log GDP per capita and rule of law ($r = 0.53$), while log green finance per capita correlates only weakly with any control ($|r| \leq 0.23$).

Table 1 Variable definitions, construction, and sources.

Variable	Definition and construction	Source	Access
$GreenInv_{it}$	Environment-related patent families (ENV-TECH); inventor country, fractional counts, families filed in ≥ 2 offices; zeros reconstructed from total counts	OECD Patents: Technology Development	SDMX API / CSV
$TotalInv_{it}$	Patent families, all technologies; same conventions	OECD Patents: Technology Development	SDMX API / CSV
$GreenShare_{it}$	$GreenInv_{it}/TotalInv_{it}$; validated against OECD published indicator (max. gap 7×10^{-8})	Derived	—
GF_{it}	Bilateral commitments, Rio Marker "climate change mitigation", principal objective, all DAC donors, all sectors; constant 2024 USD rebased to 2015 USD (deflator 1.2888)	OECD DAC RioMarkers	Data Explorer
GF^{tot}_{it}	As above, principal + significant objective	OECD DAC RioMarkers	Data Explorer
$Unscreened_{it}$	Share of recipient's bilateral aid not screened against the marker	OECD DAC RioMarkers	Data Explorer
GDP per capita	Constant 2015 USD (`NY.GDP.PCAP.KD`), logged	World Bank WDI	`wbstats`
FDI	Net inflows, % of GDP (`BX.KLT.DINV.WD.GD.ZS`)	World Bank WDI	`wbstats`
Renewables	Renewable energy, % of final consumption (`EG.FEC.RNEW.ZS`)	World Bank WDI	`wbstats`
Credit; Trade; Tertiary	Private credit % GDP; (X+M)/GDP; gross tertiary enrolment (extended controls only)	World Bank WDI	`wbstats`
Rule of law	WGI governance estimate (≈ −2.5 to +2.5), `GOV_WGI_RL.EST`	Worldwide Governance Indicators	`wbstats`
Population	Total population (`SP.POP.TOTL`)	World Bank WDI	`wbstats`
US GDP deflator	`GDPDEF`, annual mean, 2015 = 1; retrieved via the FRED API	Federal Reserve Bank of St. Louis	`fredr`

Note: "SDMX API" denotes programmatic retrieval from `sdmx.oecd.org`; the FRED deflator was the only series retrieved with an API key.

Table 2 Descriptive statistics, lag-complete sample (country-years with lagged treatment and baseline covariates observed).

Variable	N	Mean	SD	Median	Min	Max
Green inventions (families)	1682	51.97	561.56	0.58	0	14,194.5
Total inventions (families)	1682	459.19	4307.01	5.50	0.06	80,997.3
Green share of inventions	1682	0.129	0.182	0.088	0	1
Green finance p.c. (2015 USD)	1682	3.24	31.24	0.14	0	1180.0
Green finance, % of GDP	1682	0.091	0.983	0.005	0	38.88
Rule of law (WGI)	1682	−0.495	0.639	−0.545	−2.467	1.512
GDP per capita (2015 USD)	1682	4526.8	4136.9	3441.6	252.6	24,605.5
FDI, % of GDP	1682	4.18	5.58	2.88	−37.17	86.99
Renewables, % of final energy	1682	36.74	29.32	30.55	0	95.3
Population (millions)	1682	58.98	193.06	13.36	0.0098	1414.2

Note: 34.7% of country-years record zero green inventions despite positive total inventions, and 15.2% record zero principal-marked climate finance despite receiving screened aid; both are reconstructed true zeros (Sect. 3.2 and Sect. 3.3). The table covers the 1,682 country-years of the lag-complete sample; the baseline count model uses 1,624 of them after countries with no green invention in any year are absorbed by the country fixed effects.

4 Empirical strategy

4.1 Baseline econometric models

Two estimators instantiate Eq. (1), corresponding to the two natural readings of "green innovation". The first models the *count* of green inventions as a rate relative to total inventive activity, via a quasi-Poisson fixed-effects regression with an exposure offset:

$$\mathbb{E}[GreenInv_{it} \mid \cdot] = \exp\left(\beta\, GF_{i,t-2} + \boldsymbol{\gamma}'\mathbf{X}_{it} + \alpha_i + \tau_t + \log TotalInv_{it}\right), \qquad (6)$$

where α_i and τ_t are country and year fixed effects, $\mathbf{X}_{it}$ collects the controls, and the offset constrains the elasticity of green inventions with respect to total inventions to unity. Quasi-Poisson rather than Poisson maximum likelihood is required because fractional inventor-country apportionment renders 58.7% of the green-invention counts (and 93.2% of total-invention counts) in the estimation sample non-integer, precluding a Poisson likelihood; the quasi-likelihood requires only correct specification of the conditional mean [27, 28]. The

offset restriction is testable: estimating log $TotalInv_{it}$ freely yields a coefficient of 1.060 (clustered SE 0.074), which does not reject unity ($p = 0.42$; Appendix B). Estimated dispersion is $\hat{\varphi} = 1.125$, close to equidispersion.

The second estimator models the *composition* of innovation directly, via a fractional-response (quasi-binomial logit) regression [29]:

$$\mathbb{E}[GreenShare_{it} \mid \cdot] = \Lambda\big(\beta\, GF_{i,t-2} + \boldsymbol{\gamma}'\mathbf{X}_{it} + \alpha_i + \tau_t\big), \qquad (7)$$

with $\Lambda(\cdot)$ the logistic function. This estimator accommodates the boundary mass at zero natively and, by Eq. (2), assigns each country-year a bounded likelihood weight.

The treatment $GF_{i,t-2}$ is log(1 + green finance per capita) at a two-year lag in the baseline. Controls are log GDP per capita, FDI, and the renewable share of final energy. Standard errors are clustered by country throughout, with the small-sample correction $G/(G-1) \times (n-1)/(n-K)$ and inference on the t distribution with $G-1$ degrees of freedom; residual within-country autocorrelation ($\hat{\rho} = 0.204$, SE 0.026) confirms clustering is required. All lags are constructed on an exact calendar-year grid, so that a lag is never formed across a gap in a country's reporting record.

4.2 Measuring who identifies the coefficient

The weights of Eq. (4) are computed directly from the converged fit. For each estimator, the IRLS weights w_{it} of Eq. (2) are taken at the final iteration; the treatment is residualised on the controls and both sets of fixed effects in the w-metric by alternating weighted projection; and the two shares are aggregated to the country level. Concentration is summarised by a Herfindahl index across countries, $H = \Sigma_i \omega_i^2$, and by the implied effective number of countries $1/H$, which is 1 if a single country holds all the weight and N if the weight is uniform. Reporting ω^{LK} and ω^{ID} alongside each other is the direct test of H3.

H4 is tested by three re-estimations of Eq. (6) implementing Corollary 1. Test (b) deletes the dominant country. Test (c) retains it but interacts every control with an indicator for it, so that the dominant country contributes to the year effects but no longer constrains the control slopes. An intermediate test frees the dominant country's *treatment* slope while holding the controls and year effects common; under the nuisance interpretation this should leave the pooled coefficient largely unchanged, whereas under the slope-heterogeneity interpretation it should not. Because the year effects are themselves a nuisance parameter estimated with the likelihood weights, the same logic implies that the treatment of common time variation is an

analytical choice of first-order importance, which motivates its inclusion as a dimension of the specification curve.

4.3 The specification curve

Six analytical dimensions are varied factorially [10]:

1. *Estimator* (2): quasi-Poisson count model, Eq. (6); fractional-response share model, Eq. (7);
2. *Treatment scaling* (3): green finance per capita; per cent of GDP; constant USD, each in $\log(1 + x)$, lagged two years;
3. *Denominator floor* (5): country-years retained if $TotalInv_{it} \geq 1$, 2, 3, 5, or 10;
4. *Country trends* (2): absent; country-specific linear trends;
5. *Common time variation* (3): year fixed effects; a single common linear trend; none;
6. *Sample* (3): full sample; excluding China; excluding China and India.

This yields $2 \times 3 \times 5 \times 2 \times 3 \times 3 = 540$ specifications. The fifth dimension is new relative to standard practice in this literature and follows directly from Section 2.5: year effects are nuisance parameters estimated with likelihood weights, so in a concentrated panel they are fitted largely to one country's experience, and whether they are included is not an innocuous detail. The subset of 180 specifications that impose year fixed effects corresponds to the design that a conventional analysis would run.

To render coefficients comparable across treatment scalings, each is standardised by the sample standard deviation of its own regressor. Joint conclusions are drawn from the sign distribution, the share of specifications significant at 5%, and, critically, from *where* in the specification space significance concentrates, displayed in the descriptor panel of the specification-curve figure. Supporting diagnostics comprise: leave-one-country-out re-estimation under both estimators; a distributed-lag profile over lags 0–4; an eight-point denominator-floor sensitivity path; a reverse-causality test regressing future green finance on current green innovation; and minimum-detectable-effect calculations.

4.4 Software and reproducibility

Data assembly was carried out in R 4.4.1 [30] using the `tidyverse` [31], with World Bank series retrieved via `wbstats` and the US GDP deflator via the FRED API; OECD patent and Rio Marker data were retrieved from the OECD Data Explorer (SDMX endpoint and filtered

CSV export). All estimation reported in this paper was carried out in Python 3.11 with NumPy [32], pandas [33], and SciPy [34], using an iteratively reweighted least-squares implementation of Eq. (1) with alternating-projection absorption of the fixed effects, the cluster-robust covariance and small-sample correction described in Section 4.1, and the same treatment of separated observations as `fixest` [35]. The implementation was validated against `fixest` 0.13.2 on every model that the earlier R pipeline had stored, reproducing the stored coefficients and standard errors to at least four decimal places, and it reproduces 174 of the 180 year-fixed-effects specifications of the curve to within 0.0021 in the coefficient and 0.0003 in the standard error; the remaining six differ because lags here are built on an exact-year grid rather than by row position. Computing the IRLS weights of Eq. (2) and the residualised treatment requires access to the internals of the fit, which is the practical reason for the own implementation. Figures were drawn with matplotlib [36]. Claude (Anthropic) was used to assist in developing the estimation code, designing the specification-curve and weight-decomposition framework, and drafting the manuscript; all code, analyses, and text were verified and edited by the authors, who take full responsibility for the content.

5 Results

5.1 The concentration of green patenting among aid recipients

Table 3 reports the distribution of inventive mass across the sample. China accounts for 78.7% of all patent families and 80.1% of green families; India adds a further 10.4% and 9.1% respectively. The third-largest contributor, Türkiye, holds 2.2%. This is not a property of the data source: in the same OECD data, China holds 10.7% of the *world* total over 2004–2022, behind the United States (22.7%) and Japan (21.9%). The concentration arises because the set of DAC-eligible recipients excludes every advanced patenting economy, leaving China as the only large-scale inventor inside it.

Two features make the concentration structural rather than incidental. It is invariant to the denominator restrictions used throughout the paper: China's share of green families in the estimation sample is 80.1% with no floor and 80.5% when country-years with fewer than ten total inventions are dropped, and the China-plus-India share moves from 89.2% to 89.6% over the same range. And it is increasing: China's share of sample green families rises from 57.4% in 2004–2009 to 69.3% in 2010–2015 and 87.5% in 2016–2022. Any cross-country study of green patenting among aid recipients inherits this structure, and inherits more of it the more recent the sample.

Table 3 Concentration of patent families among sample recipients, 2004–2022.

Rank	Economy	Families	% of total	% of green	Cumul. %
1	China	607,849	78.70	80.11	78.70
2	India	80,603	10.44	9.06	89.14
3	Türkiye	17,273	2.24	1.59	91.37
4	Brazil	13,602	1.76	1.94	93.13
5	Malaysia	7,675	0.99	0.75	94.13
6	South Africa	7,425	0.96	1.00	95.09
7	Mexico	6,078	0.79	0.85	95.88
8	Ukraine	3,925	0.51	0.56	96.38

Note: patent families filed in ≥ 2 offices, inventor country, fractional counts. For context, China holds 10.7% of the corresponding world total (USA 22.7%, Japan 21.9%, Germany 9.3%, Korea 9.2%).

5.2 Likelihood and identifying weights (H3)

Table 4 and Fig. 1 report the decomposition of Eq. (4) for both estimators, and they confirm H3 in a stronger form than anticipated. Under the count model, China holds 80.1% of the likelihood weight, almost exactly its share of green patent families, as Proposition 1 implies for a log link. Its share of the *identifying* weight is 3.7%. The largest identifying weight belongs to Türkiye (21.6%), followed by Brazil, India (7.3%), Thailand, Morocco, and Chile. The two distributions are not variations on a theme: the Herfindahl index of the likelihood weights is 0.6509, an effective number of countries of 1.54, against 0.0763 and 13.11 for the identifying weights. One country supplies half the likelihood weight; six are required to supply half the identifying weight.

The share model equalises the likelihood weights as Eq. (2) implies, with a Herfindahl index of 0.0145 across 95 countries, an effective number of 69.1. But equal likelihood weights do not deliver equal identifying weights. The identifying weight concentrates instead in Mauritius (13.6%, against a likelihood weight of 2.1%), Azerbaijan, Bosnia and Herzegovina, Armenia, and Tunisia, giving a Herfindahl index of 0.0432 and an effective number of 23.1 countries. These are economies whose green share is computed from a handful of patent families and therefore moves a great deal from year to year, which is precisely what Eq. (4) rewards.

The two estimators consequently identify the same parameter from different populations. Of the ten economies carrying the most identifying weight under each estimator, exactly one, Morocco, appears in both lists; the count model's identifying weight sits with the mid-sized industrialising recipients, the share model's with small economies at the edge of

measurability. Neither list is the set of countries a reader of a pooled coefficient would have in mind, and neither contains China, whose influence therefore has to run through the other channel of Proposition 1.

Table 4 Likelihood and identifying weights by country under each estimator.

Count model (quasi-Poisson)			Share model (fractional response)		
Economy	**Lklhd. %**	**Ident. %**	**Economy**	**Lklhd. %**	**Ident. %**
China	80.11	3.74	Mauritius	2.08	13.64
India	9.06	7.29	Azerbaijan	2.22	6.47
Brazil	1.94	9.04	Bosnia & Herz.	1.89	5.46
Türkiye	1.59	21.56	Armenia	1.62	5.06
South Africa	1.00	3.73	Tunisia	1.65	4.03
Mexico	0.85	3.36	Dominican Rep.	1.68	3.91
Malaysia	0.75	0.93	Namibia	0.97	3.74
Ukraine	0.56	1.98	Morocco	2.16	3.63
Herfindahl index	0.6509	0.0763	Herfindahl index	0.0145	0.0432
Effective no. of countries	1.54	13.11	Effective no. of countries	69.1	23.1

Note: weights defined in Eq. (4), computed at the converged fit of the baseline specification. The count model is ordered by likelihood weight and covers 118 countries; the share model is ordered by identifying weight, uses a denominator floor of two, and covers 95 countries. The Herfindahl index is the sum of squared country shares; its reciprocal is the effective number of countries.

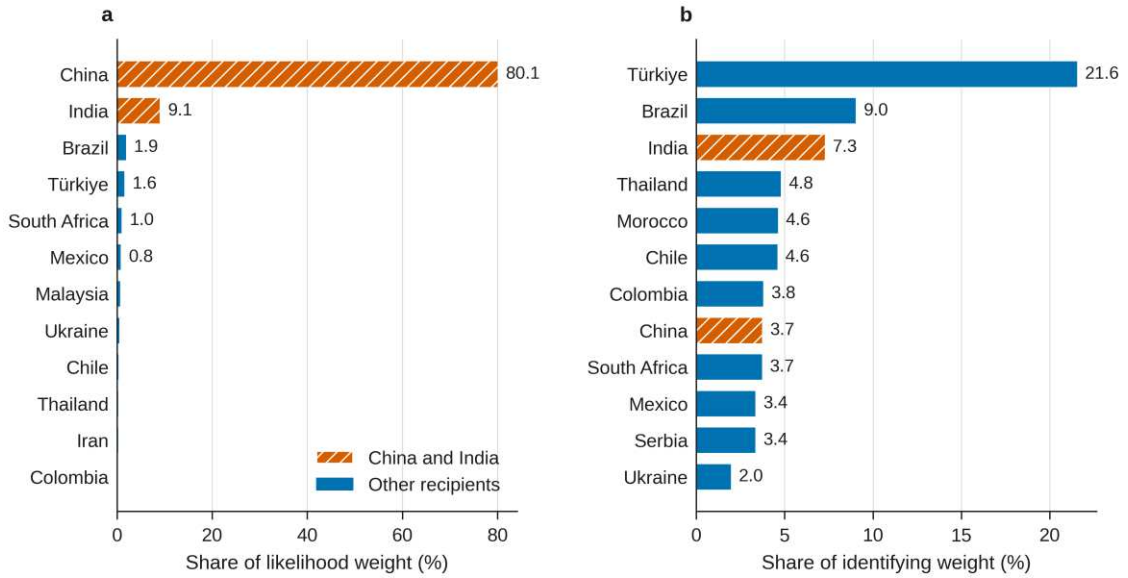


Fig. 1 Where the count estimator's weight lies: **a** likelihood weight, **b** identifying weight, top twelve countries in each. China and India are shown as hatched bars. The likelihood weight governs the year effects and control slopes; the identifying weight governs the treatment coefficient. China holds 80.1% of the first and 3.7% of the second

5.3 The baseline estimate and the channel of China's influence (H4)

Table 5 reports the estimates a conventional analysis would present, together with the three tests of Corollary 1. Column (1) is the baseline count model: the coefficient on lagged green finance per capita is 0.113 (SE 0.044, $p = 0.011$), and it is essentially unchanged under winsorisation, alternative controls, the exclusion of high-income recipients or fragile states, restriction to well-screened aid, or a post-2010 window (Appendix B). Taken at face value, which is where the literature typically stops, this supports H1: a one-within-country-standard-deviation increase in lagged green finance per capita (0.522 log points) would raise green patenting by about 6%.

Test (b) of Corollary 1 fails immediately. Excluding China reduces the coefficient by 82% and removes significance (0.020, SE 0.023, $p = 0.375$); excluding India as well leaves it at 0.023 ($p = 0.247$). A simple size cap (*TotalInv* < 50,000, removing five Chinese country-years) does the same (0.016, $p = 0.524$), whereas winsorising the treatment changes nothing (0.113, $p = 0.012$). The effect survives country-specific linear trends with China included (0.114, $p = 0.003$) but not their combination with China's exclusion (0.018, $p = 0.495$): the result is not a trend artefact but a China artefact.

Columns (4) and (5) locate the channel. Freeing China's own *treatment* slope, so that it can have a different β while still contributing to the year effects and control slopes, leaves the pooled coefficient almost untouched at 0.109 ($p = 0.010$), with a large and significant China-specific increment of 0.530 ($p = 0.008$) implying a slope for China of about 0.64. Freeing China's *control* slopes instead, so that it remains in the sample and in the year effects but no longer constrains γ, cuts the pooled coefficient to 0.039 (SE 0.020, $p = 0.053$). Test (c) therefore fails, and it fails in the direction Proposition 1 predicts: China's influence on the coefficient for the other 117 countries runs through the nuisance parameters, not through its own identifying weight, which is 3.7%.

The year effects are the other nuisance parameter, and columns (6) to (9) show that they matter in the same way. Dropping common time effects altogether gives 0.101 ($p = 0.076$) on the full sample but 0.048 (SE 0.018, $p = 0.007$) excluding China; replacing them with a single common linear trend gives 0.116 ($p = 0.044$) and 0.047 ($p = 0.026$). Excluding China, in other words, the count model returns a significant positive coefficient whenever common time variation is left unabsorbed, and an insignificant one whenever it is absorbed. Green patenting and climate finance both rose globally over the sample period; a specification without year

effects attributes their coincidence to the treatment. This is why the treatment of common time variation is carried as a full dimension of the specification curve.

The mechanism behind China's own trajectory is visible in the raw data: its green share rises from 7.3% in 2004 to 17.5% in 2021, its last sample year, and its green patenting expanded 36-fold over 2004–2021, while its green finance receipts per capita fluctuate without trend. Fig. 2 makes the influence structure explicit by re-estimating both models once for each of the 132 economies in the lag-complete sample. Under the count model the estimate collapses when, and only when, China is dropped; under the share model it stays between 0.08 and 0.15 whichever country is dropped, including China and India. Equal likelihood weighting solves the dominance problem. Section 5.4 shows what it costs.

Table 5 Green finance and green innovation: the baseline estimate and the tests of Corollary 1.

	(1)	**(2)**	**(3)**	**(4)**	**(5)**
	Baseline	Excl. CHN	*TotalInv* < 50k	CHN treat. slope	CHN ctrl. slopes
Green finance p.c., $t-2$	0.113*	0.020	0.016	0.109*	0.039
	(0.044)	(0.023)	(0.025)	(0.042)	(0.020)
p-value	0.011	0.375	0.524	0.010	0.053
China-specific increment	—	—	—	0.530**	—
				(0.198)	
	(6)	**(7)**	**(8)**	**(9)**	**(10)**
	No year eff.	No year eff., excl. CHN	Linear trend	Trend, excl. CHN	Ctry. trends, excl. CHN
Green finance p.c., $t-2$	0.101	0.048**	0.116*	0.047*	0.018
	(0.057)	(0.018)	(0.057)	(0.021)	(0.026)
p-value	0.076	0.007	0.044	0.026	0.495
Controls	log GDP p.c., FDI, renewables				
Country FE, offset log *TotalInv*	Yes in all columns				
Observations	1,624 (columns 2, 7, 9, 10: 1,606; column 3: 1,619)				

Note: quasi-Poisson estimates of Eq. (6); standard errors clustered by country in parentheses. * $p < 0.05$, ** $p < 0.01$. Columns (1)–(5) and (10) include year fixed effects. Column (4) adds an interaction of the treatment with a China indicator; column (5) interacts every control with a China indicator. Columns (6) and (7) omit common time effects; columns (8) and (9) replace them with a single common linear trend; column (10) adds country-specific linear trends. The offset restriction is not rejected (free coefficient on log *TotalInv*: 1.060, SE 0.074).

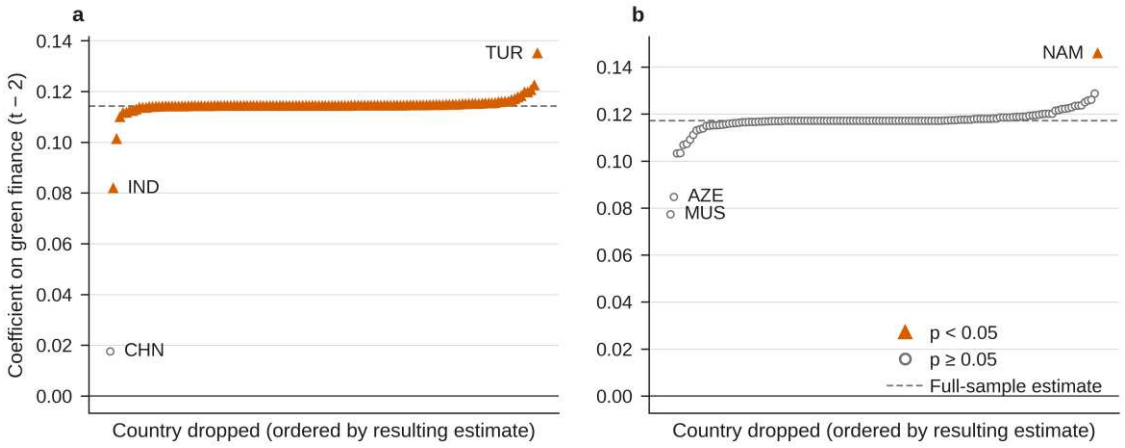


Fig. 2 Leave-one-country-out influence under both estimators: **a** quasi-Poisson count model; **b** fractional-response share model (denominator floor of two). Each marker is the coefficient on green finance ($t - 2$) from one regression with one country removed, ordered by size; triangles denote $p < 0.05$ and open circles $p \geq 0.05$; the dashed line is the full-sample estimate; the three most influential countries are labelled with ISO 3166-1 alpha-3 codes. Country-specific trends and year fixed effects; SE clustered by country; vertical scales differ between panels

5.4 The share estimator and the small-denominator problem

The share model's borderline estimate (0.117, $p = 0.085$ with country trends, and identical to three decimal places when China and India are excluded) rests on its own fragile foundation, and Section 5.2 already named it: its identifying weight sits with economies whose green share is computed from very few families. Fig. 3 and Table 6 trace the coefficient as the minimum-denominator requirement rises. At a floor of one invention the estimate is 0.133 and significant ($p = 0.032$); at three it is 0.104 ($p = 0.108$); at five it is −0.012 ($p = 0.774$). The sign flips between floors of three and five, and no floor of five or above yields an estimate remotely close to significance.

It is worth being precise about the mechanism, because it is not the one the count model exhibits. Country-years with fewer than five total inventions are 26.2% of the share-model sample and carry 26.8% of its identifying weight; the problem is not that they are overweighted but that they are *not* downweighted. Equal likelihood weighting by construction declines to discount observations whose outcome is measured with the greatest relative error, and an outcome computed from two or three patent families moves by 25–50 percentage points when a single family is added. The lag profile corroborates the noise interpretation: under the share model, only lag two among lags zero to four approaches significance ($p = 0.085$; Fig. 4), a pattern more consistent with one chance result in five tests than with a

research-to-filing mechanism, whereas the count model's uniformly positive lags one to four simply restate its dependence on China's trending series.

Table 6 Denominator-floor sensitivity of the fractional-response estimate.

Min. total inventions	Coefficient	SE	*p*-value	Obs.	Countries
1	0.133	0.061	0.032	1,414	112
2	0.117	0.067	0.085	1,176	95
3	0.104	0.064	0.108	1,016	79
4	0.048	0.060	0.421	931	72
5	−0.012	0.043	0.774	861	66
7	0.037	0.038	0.329	767	58
10	0.030	0.048	0.534	676	52
15	0.022	0.051	0.661	572	43

Note: fractional-response estimates of Eq. (7) with country-specific trends and year fixed effects; SE clustered by country. The coefficient is significant only at a floor of one, loses significance once country-years with fewer than two families are excluded, and changes sign at a floor of five.

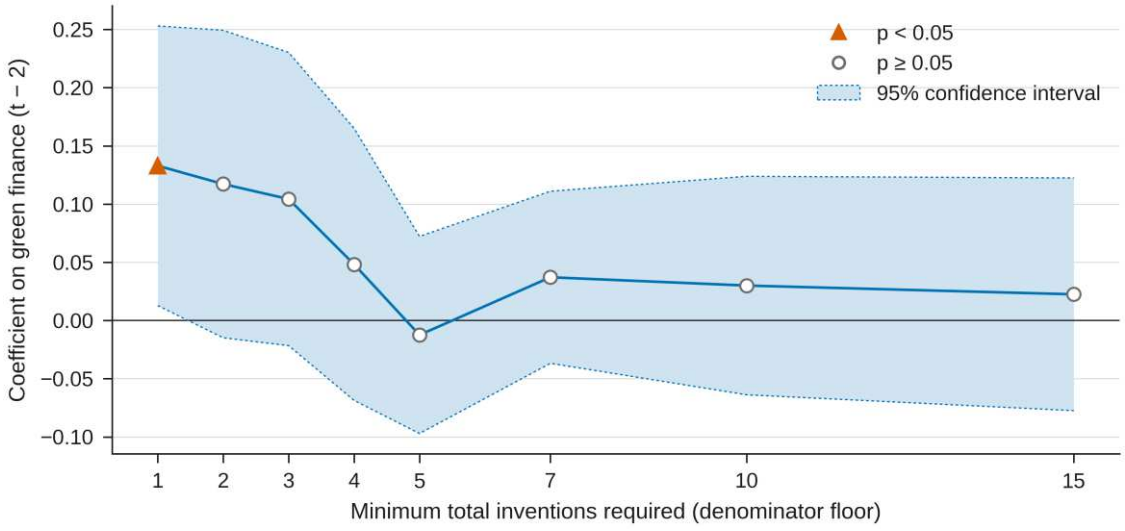


Fig. 3 Denominator-floor sensitivity of the fractional-response estimate. Coefficient on green finance ($t-2$) as the minimum-denominator requirement rises; the shaded band bounded by dotted lines is the 95% confidence interval, the triangle marks $p < 0.05$ and open circles $p \geq 0.05$. Sample sizes are reported in Table 6. The sign flips between floors of three and five

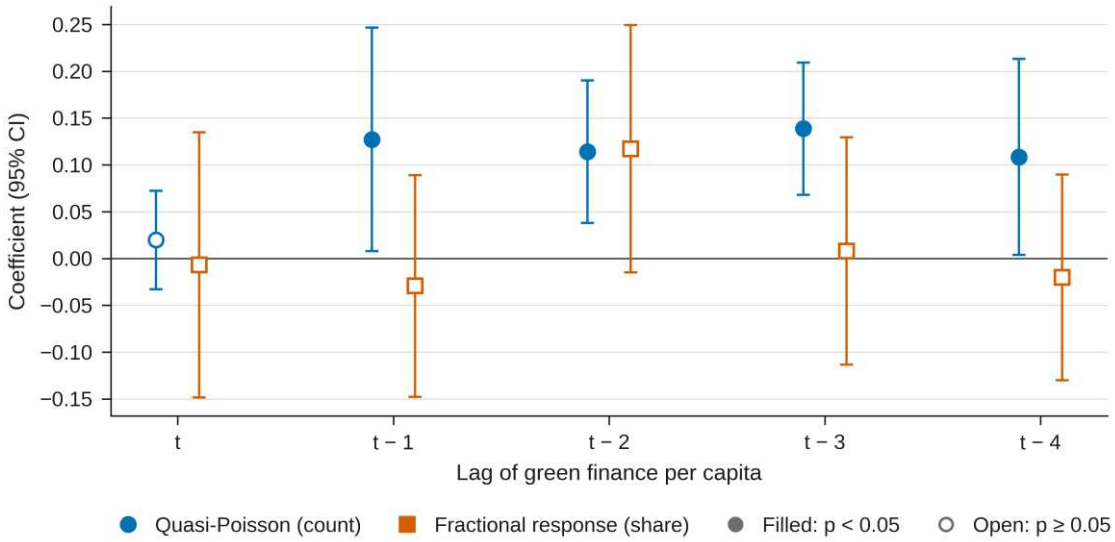


Fig. 4 Lag profile of the green finance coefficient. Coefficients on green finance per capita at lags 0–4, estimated separately under the quasi-Poisson count model (circles) and the fractional-response share model (squares); filled markers denote $p < 0.05$, open markers $p \geq 0.05$, and whiskers 95% confidence intervals. Country-specific trends, year fixed effects and a denominator floor of two throughout; SE clustered by country

5.5 The specification curve

Fig. 5 assembles the evidence. The upper panel plots all 540 standardised estimates in ascending order with 95% confidence intervals; the lower panel maps each estimate to the analytical choices that produced it. Three joint facts summarise the curve. First, the sign distribution leans positive: 96.9% of estimates exceed zero and none is significantly negative. Second, 240 specifications (44.4%) are significant at the 5% level. Third, and decisively, significance is not diffuse. It concentrates along exactly the three dimensions that Sections 5.2 to 5.4 identify: by sample and estimator, by denominator floor (84 significant estimates at a floor of one against 33 at five and 22 at ten), and by the treatment of common time variation (99 under a common linear trend and 78 with no time control, against 63 under year fixed effects). The treatment scaling matters in the same way: 107 of the significant estimates use the constant-USD scaling, which leaves country size inside the treatment, against 77 for per cent of GDP and 56 for per capita. The two estimators contribute almost equally (122 count, 118 share), but for the different reasons already documented.

Table 7 gives the decomposition. Within the 180 specifications that impose year fixed effects, which correspond to the design a conventional analysis would run, 63 are significant, and the count model's pattern is stark: 30 of 30 on the full sample, 1 of 30 excluding China, 0 of 30 excluding China and India. Relax the time control, however, and the count model excluding China is significant in 42 of 60 specifications rather than 1 of 30. The share model behaves

differently again: it is close to invariant to the sample, as Fig. 2 anticipated, and its significance is governed by the denominator instead, with 101 of its 118 significant estimates at a floor of three or below and only 3 of 54 surviving at a floor of ten.

The paper's central quantity follows. Consider the specifications that are free of all three channels at once: they exclude both dominant inventors, require at least five total inventions per country-year, and scale the treatment by country size rather than leaving it in dollars. *Of these 48 specifications, none is significant at the 5% level*, and this holds under all three treatments of common time variation. Their median standardised effect is 0.044, with a median standardised standard error of 0.037. Every route to statistical significance in this design passes through a dominant inventor, a denominator of fewer than five patents, an unabsorbed global trend, or an unscaled treatment.

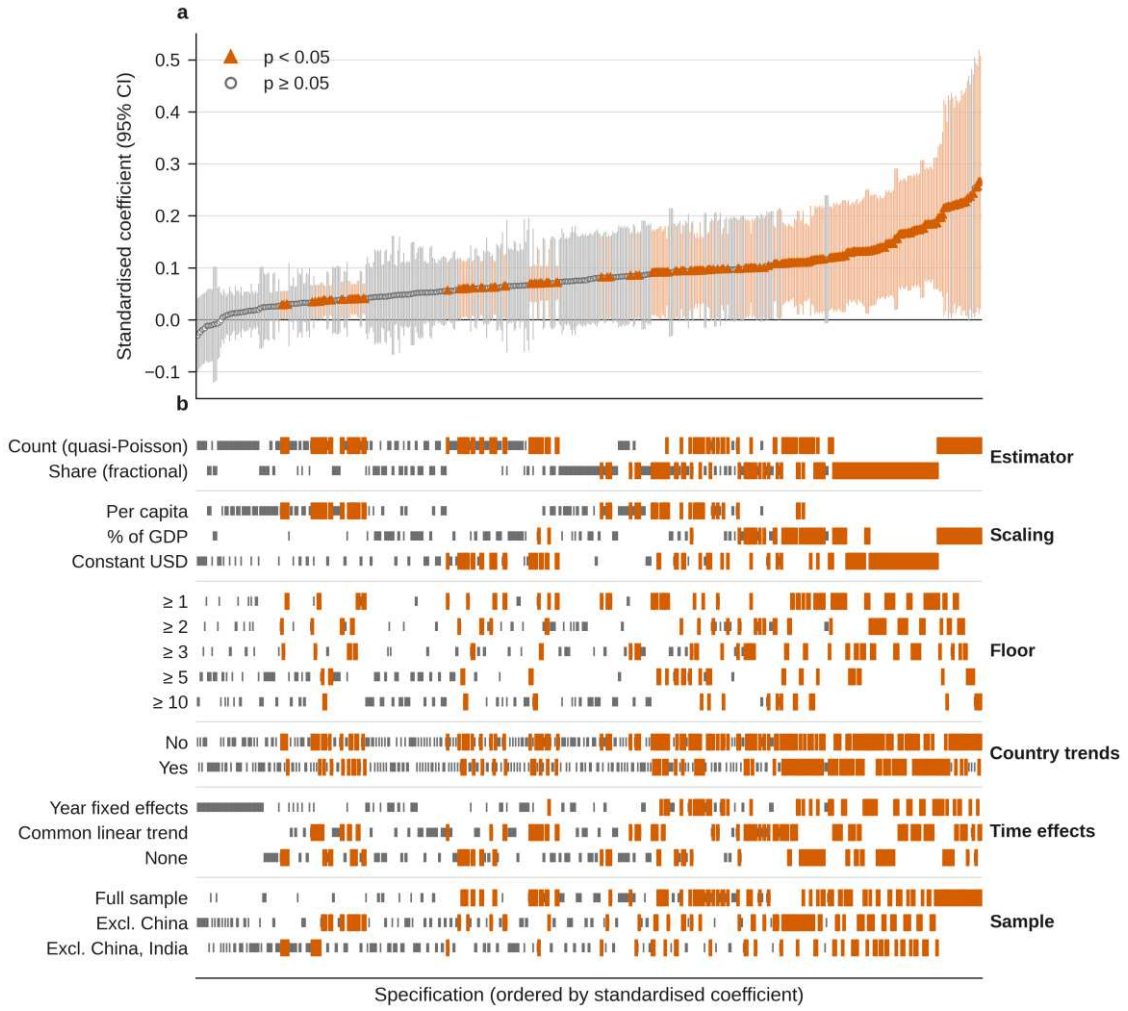


Fig. 5 Specification curve: 540 estimates of the effect of green finance ($t-2$) on green innovation. **a** Coefficients standardised by the standard deviation of their own regressor, sorted in ascending order, with 95% confidence intervals; triangles denote $p < 0.05$ and open circles $p \geq 0.05$. **b** The analytical choices (estimator, treatment scaling, denominator floor,

country trends, common time variation, sample) that generate each estimate; longer, heavier ticks mark specifications significant at the 5% level. SE clustered by country throughout

Table 7 Where significance lives: decomposition of the 540 specifications.

Estimator	Time control	Sample	Signif. / total	Median est.
Quasi-Poisson (count)	Common linear trend	Full sample	22 / 30	0.097
Quasi-Poisson (count)	Common linear trend	Excl. China	20 / 30	0.061
Quasi-Poisson (count)	Common linear trend	Excl. China & India	6 / 30	0.053
Quasi-Poisson (count)	No time control	Full sample	17 / 30	0.084
Quasi-Poisson (count)	No time control	Excl. China	22 / 30	0.058
Quasi-Poisson (count)	No time control	Excl. China & India	4 / 30	0.037
Quasi-Poisson (count)	Year fixed effects	Full sample	30 / 30	0.106
Quasi-Poisson (count)	Year fixed effects	Excl. China	1 / 30	0.013
Quasi-Poisson (count)	Year fixed effects	Excl. China & India	0 / 30	0.019
Fractional response (share)	Common linear trend	Full sample	17 / 30	0.099
Fractional response (share)	Common linear trend	Excl. China	17 / 30	0.099
Fractional response (share)	Common linear trend	Excl. China & India	17 / 30	0.099
Fractional response (share)	No time control	Full sample	11 / 30	0.082
Fractional response (share)	No time control	Excl. China	13 / 30	0.087
Fractional response (share)	No time control	Excl. China & India	11 / 30	0.088
Fractional response (share)	Year fixed effects	Full sample	10 / 30	0.092
Fractional response (share)	Year fixed effects	Excl. China	11 / 30	0.093
Fractional response (share)	Year fixed effects	Excl. China & India	11 / 30	0.093
Excl. China & India, floor ≥ 5, size-scaled treatment			**0 / 48**	**0.044**

Note: significance at the 5% level, SE clustered by country; each cell of the upper block collects 30 specifications (three treatment scalings × five denominator floors × two country-trend settings). Median estimates are standardised by the standard deviation of the treatment. The final row is the subset described in the text; it spans all three time controls.

5.6 Reverse causality, power, and absorptive capacity

Three further results complete the picture. First, the weakness of the evidence is not attributable to reverse causality masking a true effect: regressing green finance per capita at $t + 2$ on current green innovation (OLS with country and year fixed effects, controlling for log GDP per capita and FDI) yields a coefficient of −0.074 (SE 0.050, $p = 0.139$) with a within-R^2 of 0.0019, indicating that donors do not allocate mitigation finance toward recipients exhibiting green innovation; if anything the reverse holds, consistent with need-based allocation.

Second, the null must be read against the design's power. Across the 48 surviving specifications the median standardised estimate is 0.044 with a median standardised standard error of 0.037, implying a minimum detectable effect of approximately 0.104 standard deviations at 80% power; the median upper 95% confidence bound is 0.111 and the maximum 0.181. The honest statement is therefore twofold. The data cannot distinguish a zero effect from a small positive one, so H1 is not refuted for effects below roughly a tenth of a standard deviation. But they do exclude the moderate-to-large effects that the artefact-driven specifications mimic: the 240 significant estimates range from 0.028 to 0.265 with a median of 0.110, and 111 of them (46%) lie above the median upper confidence bound of the surviving specifications, 35 of them above its maximum.

Third, H2 is not identifiable in this design, for a structural reason worth reporting rather than concealing. The natural test interacts the treatment with institutional quality, the moderator prominent in this literature. Within-country variation in rule of law is minimal (median within-country SD 0.130 against a between-country SD of 0.730), so fixed-effects interactions with time-varying institutions test only tiny within-country movements. Interacting instead with time-invariant country-mean rule of law, the interaction is positive but insignificant (0.042, SE 0.041, $p = 0.310$), and the minimum detectable moderation effect is 0.085 per standard deviation of rule of law, or 85.9% of the baseline elasticity: only a moderation effect nearly as large as the main effect itself could be detected. Compounding this, country-mean rule of law and country size are strongly negatively correlated among aid recipients ($r = −0.54$ across the 132 economies of the lag-complete sample; Fig. 6): well-governed recipients are systematically small island and micro-states, poorly governed ones systematically large. Institutions and absorptive scale are therefore collinear in this sample, and which of them appears to moderate depends on estimator and treatment scaling; interactions of a per-capita treatment with population are in addition partly mechanical.

The indirect evidence on H2 is nonetheless suggestive. The twelve recipients that record no green patent family in any year they appear span the full institutional range, from Afghanistan (rule of law −1.75) to Saint Vincent and the Grenadines (+0.98), and seven of them are small island states, several receiving substantial green finance per capita (Appendix A). Absence of inventive output tracks scale, not governance, which is what Cohen and Levinthal's [17] argument implies at the country level. Establishing it properly requires a design with within-country variation in absorptive capacity, which this one does not have. The threshold and moderation results reported in prior work should be interpreted with these constraints in mind.

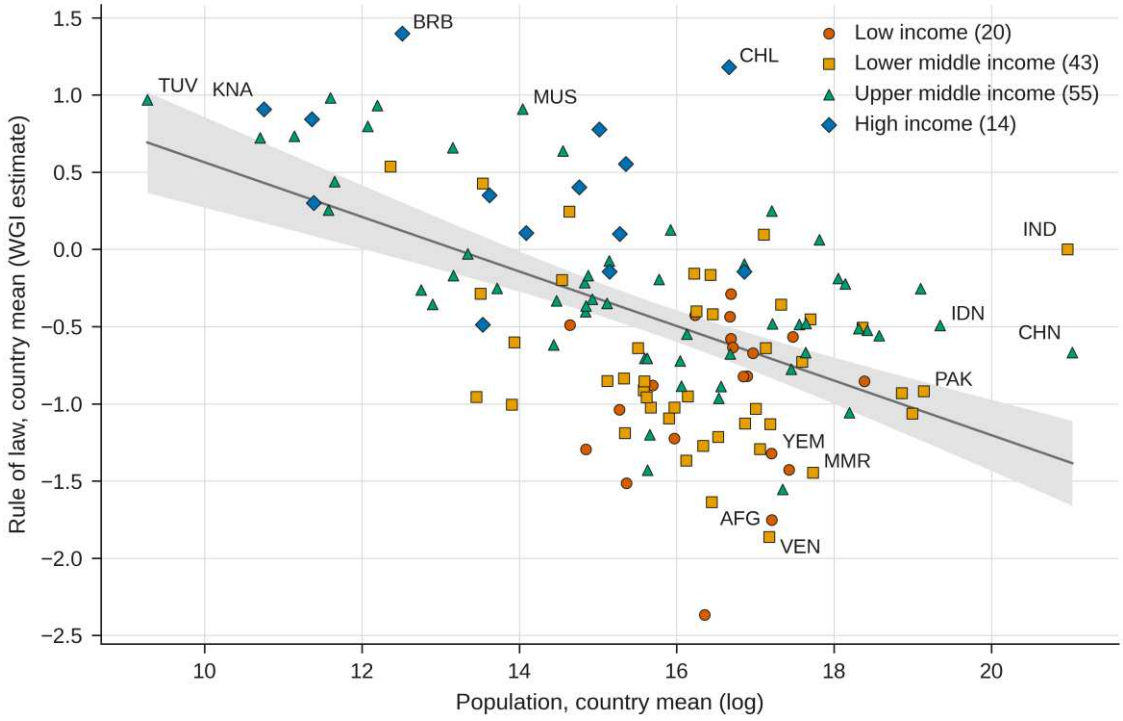


Fig. 6 Institutional quality and country size among the 132 economies of the lag-complete sample ($r = -0.54$). Markers denote income groups (circles: low; squares: lower middle; triangles: upper middle; diamonds: high income; group sizes in parentheses); the line is an OLS fit with its 95% confidence band; selected economies are labelled with ISO 3166-1 alpha-3 codes. Well-governed recipients are systematically small, and large recipients systematically score low on rule of law

6 Discussion

6.1 What the results establish

The results do not show that international climate finance is useless, nor that it never stimulates invention. They establish something narrower and, we think, more useful: that in this class of panel the pooled coefficient is determined by the estimator's weighting structure

rather than by the finance–innovation relationship, and that once the weighting channels are closed the relationship is not detectable at the magnitudes the literature reports.

H3 and H4 are confirmed directly. The likelihood and identifying weights diverge by an order of magnitude under the count estimator (Herfindahl 0.6509 against 0.0763), and the two estimators identify β from country sets that share one member out of ten. China's influence on the pooled coefficient runs through the nuisance parameters and not through its own identifying weight of 3.7%, as the contrast between freeing its treatment slope (pooled coefficient 0.109) and freeing its control slopes (pooled coefficient 0.039) demonstrates. This is a general point about nonlinear fixed-effects estimation in concentrated panels, and the diagnostics that reveal it cost a few lines of code once the weights of Eq. (2) are in hand.

H1, accordingly, is not supported by evidence that survives those diagnostics, and H2 is not identifiable here. The positive consensus emerges from three artefacts that operate at different points of the design. Count estimators, by weighting observations in proportion to their conditional means, let one economy determine the year effects and control slopes for all the others; that economy's green patenting expanded 36-fold between 2004 and 2021 for reasons (industrial policy, domestic green credit, patent-subsidy programmes, sheer scale of R&D; cf. [37]) that have little to do with the modest and trendless bilateral aid flows it received. Share estimators, by equalising likelihood weights, decline to discount observations whose outcome is computed from two or three patent families. And specifications that do not absorb common time variation attribute the coincident global rise in climate finance and green patenting to the treatment. That 96.9% of 540 estimates are positive while none is significantly negative suggests a genuinely non-negative underlying association, but one whose magnitude, wherever it can be estimated from informative observations under absorbed time effects, is statistically indistinguishable from zero and economically small.

This diagnosis explains the prior literature's findings rather than merely contradicting them. Studies employing 30–80 developing countries with patent-count outcomes and no weight decomposition or denominator diagnostics will, if a dominant inventor is in the sample, recover that country's contribution to the nuisance parameters; studies employing shares without denominator floors will recover small-sample noise with a positive drift; and studies that model long-run relationships without common time effects will recover coincident global trends. Each is a reasonable choice taken alone, which is precisely why the joint display is necessary.

Substantively, the pattern is what the absorptive-capacity and innovation-geography literatures predict [17, 18]: invention in mitigation technologies is concentrated in a few economies with deep scientific and industrial bases, and marginal concessional flows to economies without such bases fund deployment, adaptation, and capacity-building rather than patentable invention.

6.2 Implications for policy and for sustainable growth

For climate finance policy, the findings caution against loading the innovation objective onto instruments designed for deployment. If bilateral mitigation finance is to be justified, it should be justified by what it demonstrably funds (renewable capacity, efficiency retrofits, enabling infrastructure) rather than by an innovation dividend the data do not reveal. The results also bear on allocation debates: conditioning finance on governance indicators finds no support in the innovation evidence, both because the moderation effect is unidentifiable at achievable power and because governance is confounded with country size among recipients. In the language of SDG 8, the evidence supports treating bilateral mitigation finance as an instrument for target 8.4 (resource efficiency and the decoupling of growth from emissions, delivered through deployment) rather than for target 8.2 (productivity growth through technological upgrading and innovation), for which domestic innovation systems, absorptive capacity, and scale are more plausible binding constraints.

For research on sustainable finance and sustainable growth in emerging and developing economies, the implication is methodological and concrete. Outcome distributions as concentrated as this one are the norm rather than the exception in cross-country work on innovation, emissions, energy, and finance. Wherever a nonlinear fixed-effects estimator is used on such data, the two weights of Eq. (4) should be reported alongside the coefficient, and the two tests of Corollary 1 should accompany the usual robustness table. Neither requires additional data. Single-specification results, including, symmetrically, the single-specification null that a less exhaustive version of this study might have reported, should be discounted relative to evidence displayed across the specification space.

6.3 Limitations

Five limitations bound the conclusions. (i) The treatment covers bilateral commitments only; multilateral flows (Green Climate Fund, development banks), private finance mobilised, and South–South flows are outside the Rio Marker system, and commitments overstate disbursements. (ii) Rio Marker coding is donor-self-reported and politically inflected [19, 21];

the principal-only baseline and the unscreened-share restriction mitigate but cannot eliminate this. (iii) Patents are an imperfect and internationally uneven proxy for innovation [22]; the two-office family filter trades coverage of informal innovation for quality, and non-patented adaptation of green technology is invisible here. (iv) The design is observational; lags and reverse-causality tests discipline but do not establish causal order, and no credible external instrument for climate finance receipts at this coverage is available. (v) Statistical power outside the dominated and small-denominator regions is limited; the null is a statement about the absence of detectable moderate-to-large effects, not proof of a zero. Proposition 1 itself is a statement about what an estimator identifies, not a solution: it says which countries carry the coefficient, and the remedy where the policy parameter concerns the middle of the distribution is a design in which that middle supplies the variation.

7 Conclusions

This paper asked whether international climate-mitigation finance produces green innovation in developing economies, and found that the answer depends less on the data than on how the estimator weights them. Separating each country's likelihood weight from its identifying weight shows that in a panel where one economy holds four-fifths of the outcome mass the two diverge by an order of magnitude, that count- and share-based estimators identify the coefficient from almost disjoint sets of economies, and that the dominant economy shapes the pooled estimate through the year effects and control slopes it determines for every other country rather than through its own identifying weight. The tests implied by this decomposition fail for the baseline estimate: it survives freeing China's treatment slope (0.109) and does not survive freeing China's control slopes (0.039) or dropping China altogether (0.020).

Across 540 specifications spanning two estimators, three treatment scalings, five denominator thresholds, two trend structures, three treatments of common time variation, and three samples, 96.9% of estimates are positive and 240 are significant, but significance requires retaining a dominant inventor, admitting denominators below five inventions, leaving common global trends unabsorbed, or leaving country size inside the treatment. Among the 48 specifications free of all four, none is significant, with a median standardised effect of 0.044 and a minimum detectable effect of 0.104. Reverse causality is rejected; the offset restriction of the rate model is validated; and the institutional-threshold hypothesis prominent in this

literature proves unidentifiable given minimal within-country institutional variation and a −0.54 correlation between governance and country size among recipients.

The constructive contribution is portable. The weight decomposition of Eq. (4) and the two tests of Corollary 1 apply to any generalised linear model with fixed effects and cost nothing beyond access to the converged fit, and they answer a question that applied work in this field asks only implicitly: which countries does this coefficient describe? Future work on the finance–innovation nexus would profit from firm- and project-level designs within large recipients, where identification does not hinge on cross-country weighting; from outcome measures that capture non-patented technological upgrading; and from the machine-learning reconstructions of climate finance now emerging [21], which promise treatment measurement less exposed to the Rio Markers' known biases. Until such evidence accumulates, claims that international green finance accelerates green innovation, and thereby the technological-upgrading component of SDG 8, should be regarded as unproven.

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Declarations

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Competing interests. The authors declare no competing financial or non-financial interests.

Ethics approval and consent to participate. Not applicable. The study uses publicly available, country-level secondary data and involves no human participants or animals.

Consent for publication. Not applicable.

Author contributions. A.A.: conceptualisation, methodology, software, validation, formal analysis, investigation, data curation, writing — original draft, writing — review and editing, visualisation. N.S.S.A.-K.: validation, writing — review and editing, project administration. M.S.U.H.: validation, writing — review and editing. All authors read and approved the final manuscript.

Data availability. All data analysed in this study are publicly available. OECD ENV-TECH patent indicators and the OECD DAC RioMarkers dataset are available from the OECD Data Explorer (https://data-explorer.oecd.org); the World Development Indicators and the Worldwide Governance Indicators from the World Bank (https://data.worldbank.org); and the US GDP implicit price deflator from FRED (https://fred.stlouisfed.org, series GDPDEF). The assembled panel used for estimation is available from the corresponding author on reasonable request.

Code availability. The data-assembly pipeline and the Python estimation and figure code, which together reproduce every table and figure in the paper, are available from the corresponding author on reasonable request.

Use of generative AI. Disclosed in the Acknowledgements and in Sect. 4.4.

Appendix A: Sample construction and composition

Table 8 traces the attrition from the merged patent–finance panel to the lag-complete sample, Table 9 reports that sample's income and regional composition, and Table 10 lists the recipients that record no green patent family in any sample year.

Table 8 Sample attrition from the merged panel to the lag-complete sample.

Step	Obs.	Countries	Years	Obs. lost	Ctys. lost
0. Merged patent–finance panel	2,396	147	2002–2023	—	—
1. Total inventions > 0	2,396	147	2002–2023	0	0
2. Green finance observed	2,341	144	2002–2023	55	3
3. Rule of law observed	2,341	144	2002–2023	0	0
4. Baseline controls observed	2,129	143	2002–2022	212	1
5. Two-year lags available	1,682	132	2004–2022	447	11

Note: step 4 loses 2023 because the WDI renewable-energy series is unavailable for that year; step 5 requires a treatment observation exactly two calendar years earlier, so a country-year following a reporting gap is dropped. The baseline count sample is the 1,624 observations of step 5 that remain after countries with no green invention in any year are absorbed by the country fixed effects.

Table 9 Composition of the lag-complete sample (132 economies).

Income group	Countries	Region	Countries
Low income	20	Sub-Saharan Africa	42
Lower middle income	43	Latin America & Caribbean	31
Upper middle income	55	Europe & Central Asia	18
High income	14	MENA, Afghanistan & Pakistan	18
		East Asia & Pacific	17
		South Asia	6

Note: World Bank income classification. High-income recipients are economies that graduated during the sample period; their exclusion leaves the baseline estimate unchanged (Appendix B).

Table 10 Recipients with zero green patent families throughout the sample.

Economy	Income group	Years	GF p.c. (mean)	Rule of law
Afghanistan	Low income	7	0.46	−1.75
Angola	Lower middle	11	0.10	−1.29
Comoros	Lower middle	6	0.36	−0.95
Benin	Lower middle	7	0.55	−0.40
Maldives	Upper middle	1	12.58	−0.36
Malawi	Low income	4	0.48	−0.29
Solomon Islands	Lower middle	2	9.77	−0.29
Suriname	Upper middle	2	0.00	−0.17
Vanuatu	Lower middle	2	0.60	0.54
Dominica	Upper middle	12	11.75	0.73
Saint Lucia	Upper middle	1	0.00	0.80
St. Vincent & Grenadines	Upper middle	1	0.00	0.98

Note: these observations cannot enter Eq. (6), whose country fixed effects require within-country outcome variation. They span the full institutional range and include seven small island states, several with substantial green finance per capita: evidence that scale rather than governance binds (Sect. 5.6).

Appendix B: Additional robustness tables

Tables 11–14 report the sensitivity of the baseline count estimate to the control set, sample restrictions, lag structure, and alternative outcome and treatment measures; Table 15 collects the post-estimation diagnostics cited in the main text.

Table 11 Sensitivity of the baseline count estimate to the control set.

Specification	Coef.	SE	*p*	Obs.	Countries
No controls	−0.004	0.023	0.857	1,624	118
+ log GDP per capita	0.106	0.041	0.011	1,624	118
+ FDI, renewables (baseline)	0.113	0.044	0.011	1,624	118
+ credit, trade	0.090	0.035	0.011	1,272	101
+ tertiary enrolment	0.097	0.041	0.021	904	79
Baseline without renewables	0.106	0.041	0.011	1,624	118

Note: quasi-Poisson, country and year FE, offset, SE clustered by country. The no-controls null reflects omission of log GDP per capita, which absorbs income-driven scale in patenting.

Table 12 Sensitivity of the baseline count estimate to sample restrictions.

Sample	Coef.	SE	*p*	Obs.	Countries
Full sample (baseline)	0.113	0.044	0.011	1,624	118
Excl. China and India	0.023	0.020	0.247	1,588	116
Excl. high-income recipients	0.113	0.048	0.020	1,484	105
Excl. fragile/conflict states	0.115	0.045	0.012	1,374	98
Countries with >= 10 years	0.116	0.045	0.012	1,424	86
Total inventions >= 2	0.113	0.045	0.013	1,176	95
Unscreened aid < 30%	0.096	0.041	0.023	1,039	111
2010 onwards	0.088	0.040	0.028	1,063	107

Note: every restriction *except* the exclusion of China (and India) leaves the estimate intact, the signature of single-country dependence rather than broad-based association.

Table 13 Lag structure of the treatment (separate regressions, both estimators).

	Count model			Share model			
Treatment timing	**Coef.**	**SE**	***p***	**Coef.**	**SE**	***p***	**Obs.**
Contemporaneous (t)	0.020	0.027	0.462	−0.007	0.072	0.924	1,332
$t-1$	0.127	0.061	0.039	−0.029	0.060	0.628	1,255
$t-2$ (baseline)	0.114	0.039	0.004	0.117	0.067	0.085	1,176
$t-3$	0.139	0.036	<0.001	0.008	0.062	0.899	1,098
$t-4$	0.108	0.053	0.045	−0.020	0.056	0.720	1,025

Note: country-specific linear trends, year fixed effects, and a denominator floor of two throughout, so that the two estimators are compared on the same observations; SE clustered by country. Observation counts are for the count model; the share model differs by at most two observations. Lags are built on an exact calendar-year grid.

Table 14 Alternative outcome and treatment measures.

Specification	Coef.	SE	*p*	Obs.
Baseline: quasi-Poisson, principal marker	0.113	0.044	0.011	1,624
Rio marker: principal + significant	0.068	0.029	0.019	1,624
Fractional response on green share	0.089	0.058	0.126	1,176
Treatment in % of GDP	1.214	0.562	0.033	1,624
Treatment in constant USD	0.016	0.005	0.004	1,624
OLS on log(1 + *GreenInv*)	0.012	0.017	0.482	1,682
Cumulative finance stock ($t-2$)	−0.256	0.192	0.185	1,624

Note: country and year fixed effects throughout; SE clustered by country. Coefficients are not comparable across treatment scalings without standardisation, which the specification curve applies.

Table 15 Post-estimation diagnostics.

Diagnostic	Result
Offset restriction (coef. on log *TotalInv*, free)	1.060 (SE 0.074); H_0: $\beta = 1$ not rejected, $p = 0.42$
Dispersion parameter $\hat{\varphi}$ (quasi-Poisson)	1.125 (close to equidispersion)
Fractional counts (share of non-integer values)	58.7% of green-invention counts; 93.2% of total-invention counts
Residual AR(1) within country	$\hat{\rho} = 0.204$ (SE 0.026), $p < 0.001 \Rightarrow$ cluster by country
Reverse causality: $GreenInv_t \rightarrow GF_{t+2}$	−0.074 (SE 0.050), $p = 0.139$; within-$R^2 = 0.0019$
Weight concentration, count model (Herfindahl)	likelihood 0.6509 vs identifying 0.0763
Weight concentration, share model (Herfindahl)	likelihood 0.0145 vs identifying 0.0432
Within/between SD, rule of law	0.130 / 0.730 (ratio 0.18)
Within/between SD, green finance p.c. ($t-2$, log)	0.522 / 0.441 (ratio 1.18)
MDE for institutional moderation (80% power)	0.085 per SD of rule of law = 85.9% of baseline elasticity

Appendix C: Additional figures

Fig. 7 shows the distributions of the outcome and the treatment in the lag-complete sample, and Fig. 8 plots their sample trends over 2004–2022.

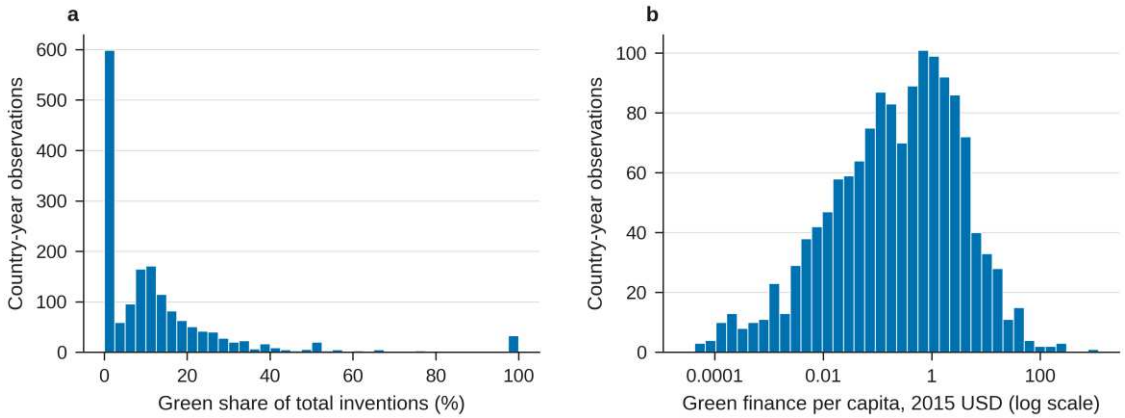


Fig. 7 Distributions of the outcome and the treatment in the lag-complete sample (1,682 country-years). **a** Green invention share; the spike at zero represents the 34.7% of country-years that record no green patent family despite positive total inventive activity (reconstructed true zeros). **b** Green finance per capita (constant 2015 USD, positive values only, logarithmic scale); the 15.2% of country-years with no principal-marked mitigation finance are omitted. The heavy right skew motivates the $\log(1 + x)$ transformation and the winsorised robustness check

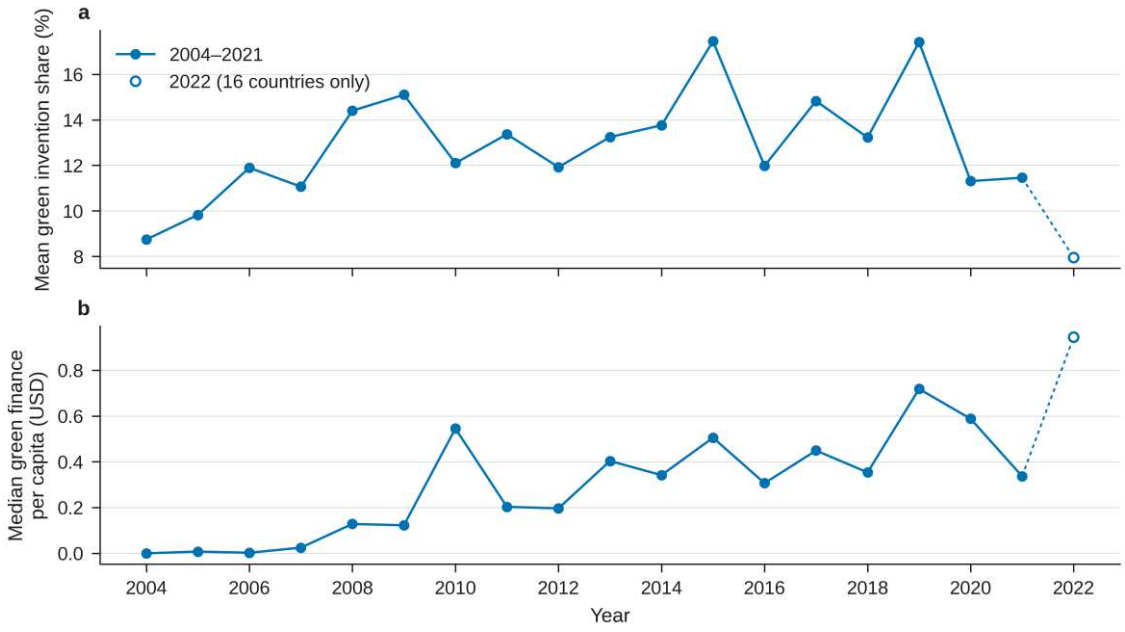


Fig. 8 Sample trends, 2004–2022: **a** mean green invention share; **b** median green finance per capita (constant 2015 USD). The open marker and dashed segment flag 2022, when only 16 countries are observed

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