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Quoc Lap Nguyen

lapnq344@gmail.com

University of Economics and Law

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Structural Absorptive Capacity and the Conditional Effectiveness of International Climate Finance: Evidence from Agricultural Labour Composition and Productivity

Quoc Lap Nguyen^{1,2*}

^{1*}University of Economics and Law, Ho Chi Minh City, Vietnam.

²Vietnam National University, Ho Chi Minh City, Vietnam.

Corresponding author(s). E-mail(s): lapnq344@gmail.com,
lapnqc26602-1-nc@sdh.uel.edu.vn;

Abstract

International climate finance channeled to developing economies has expanded rapidly, yet the countries receiving the largest transfers are frequently those least equipped, structurally, to convert that capital into measurable emissions reductions. This study investigates whether two observable features of an economy's agricultural sector, the share of the labour force still engaged in agriculture and the value added generated per agricultural worker, systematically moderate the relationship between climate finance and carbon emissions. Using an unbalanced panel spanning more than one hundred developing and emerging economies between 2000 and 2019, and a two-way fixed effects framework subjected to a wide battery of standard-error and specification checks, the analysis finds that the marginal effect of climate finance reverses sign between agriculture-dependent and structurally advanced economies, a pattern that survives the inclusion of income and trade-openness controls, alternative structural thresholds, and estimation on absolute rather than per-capita emissions. A composite readiness indicator built from the same two structural markers reveals that a substantial share of major finance recipients rank poorly on absorptive capacity. Identification limitations, including an imperfect placebo test, are reported candidly. The findings point toward a structural dimension of climate finance allocation that existing governance-centred screening frameworks currently overlook.

Keywords: Climate finance, absorptive capacity, structural transformation

1 Introduction

Climate finance directed at developing economies reached USD 115.9 billion in 2022, the first year in which developed countries met the long-standing pledge of USD 100 billion agreed under the United Nations Framework Convention on Climate Change ([Organisation for Economic Co-operation and Development 2024a](#)). The figure is frequently cited as a milestone, yet it answers only the question of how much capital is being delivered, not whether the capital, once delivered, is converted into lower emissions. That second question has proven considerably harder to settle. Panel evidence finds that climate finance reduces emissions on average, but with effects concentrated in mitigation rather than adaptation finance and varying sharply across sectors ([Wei 2025](#); [Fang et al. 2025](#)). Related work traces part of the mechanism through technology transfer and domestic research effort, treating absorptive capacity as the moderating force that determines how much of a given flow ultimately converts into emissions reductions ([Zhang et al. 2025](#)). A parallel literature documents that finance often fails to reach its intended use efficiently, with persistent gaps between the finance developing countries report needing in their Nationally Determined Contributions and the finance that actually arrives ([Isah et al. 2025](#)), and with disbursement speed shaped more by the length of approval processes than by the size of the grant itself ([Nor 2025](#)).

This paper asks a question that sits upstream of disbursement speed, sectoral targeting, and technology transfer: does the structural composition of the recipient economy determine whether climate finance can be converted into lower emissions at all? The candidate mechanism draws on a long-standing idea in economics. Absorptive capacity, defined as the ability of an economic unit to recognise, assimilate, and productively apply external resources, was formalised by [Cohen and Levinthal \(1990\)](#) and is understood to depend on the stock of related capability already present within the recipient. Development economics has long treated the transition of labour out of low-productivity agriculture as the central engine that builds such capability ([Lewis 1954](#)), a transition recent evidence shows is neither automatic nor guaranteed. A substantial share of developing economies shift workers into services well before building a meaningful industrial base, a pattern that carries productivity consequences persisting for decades ([Rodrik 2016](#)). Underlying this pattern is a large and persistent productivity gap between agricultural and non-agricultural value added per worker, documented across a wide range of developing countries even after correcting for measurable differences in hours worked and human capital ([Gollin et al. 2014](#)). If absorptive capacity operates in the way this literature suggests, agricultural labour share and agricultural value added per worker constitute two directly observable proxies for it, both collected annually and publicly for nearly every country by the International Labour Organization and the World Bank.

The empirical puzzle motivating this test is stated plainly in [Table 1](#). Economies where agriculture absorbs more than 40 percent of the labour force emit substantially less carbon dioxide per capita than economies where that share falls below 20 percent, a gap of an order of magnitude. Mean climate finance received, however, runs in the opposite direction: the most agriculture-dependent group receives more on average than the least agriculture-dependent group. This pattern alone proves nothing, since low-emitting economies may simply require less mitigation finance in absolute terms,

but it motivates a direct test of whether the marginal effect of finance on emissions differs across this structural divide, and whether that difference is large enough, and identified cleanly enough, to matter for how climate finance is targeted in practice.

The contribution is threefold. First, to the author’s knowledge, this is the first study to test agricultural structural composition, as distinct from institutional quality, governance, or income level, as a moderator of climate finance effectiveness in a global panel. Second, the interaction result is subjected to a battery of checks uncommon in this literature: income and trade-openness controls, alternative structural thresholds, estimation on absolute rather than per-capita emissions, a formal test of whether regime-specific coefficients differ statistically from one another, outlier winsorization, two-way clustered standard errors, and a placebo test using future finance flows, reported candidly including where the evidence does not fully resolve. Third, the two structural indicators are converted into a simple composite readiness score, extending the analysis from an academic interaction term into a screening statistic with a direct, quantifiable relationship to current climate finance allocation patterns.

The remainder of the paper proceeds as follows. Section 2 situates the study within the literature on climate finance effectiveness, carbon lock-in, and institutional allocation criteria. Section 3 describes the data and empirical strategy. Section 4 presents the main results. Section 5 subjects the main result to a wide set of robustness and identification checks. Section 6 discusses policy implications, and Section 7 concludes.

2 Related Literature and Institutional Context

2.1 Climate finance effectiveness

The literature on climate finance effectiveness has expanded quickly but remains inconclusive on both magnitude and mechanism. Fixed-effects panel estimates across developing countries find that a marginal increase in climate finance is associated with a small reduction in emissions, concentrated in mitigation finance (Wei 2025), a result extended to a larger sample with sector-level heterogeneity by Fang et al. (2025). Zhang et al. (2025) trace part of the effect to technology transfer and domestic research and development, explicitly invoking absorptive capacity as the governing mechanism. A separate strand asks whether finance reaches its intended use in the first place: Isah et al. (2025) document large gaps between the finance developing countries report needing and the finance that actually arrives, Nor (2025) show that disbursement performance depends more on the length of the approval process than on grant size, and a panel of 74 developing countries finds that climate finance reduces energy vulnerability specifically (Njangang et al. 2024).

A complementary literature studies who receives finance in the first place. Institutional quality, particularly political stability and control of corruption, shapes climate finance receipts (Lubinga and Mazenda 2024), while governance-conditioned finance has had limited traction because multilateral institutions frequently fail to account for local power structures (Browne 2022). In the broader foreign-aid literature, one of the most cited assessments finds that aid raises growth only where fiscal, monetary, and trade policy is already sound, with essentially no effect where policy quality is poor (Burnside and Dollar 2000), a conditional-effectiveness result later qualified

on data and specification grounds (Easterly et al. 2004). Work on the political economy of adaptation aid specifically finds that recipient governance and donor interests, rather than vulnerability or need alone, shape allocation patterns (Weiler et al. 2018; Betzold and Weiler 2017). The present study sits alongside this literature but tests a structural, rather than institutional or interest-based, moderator.

2.2 Carbon lock-in and structural transformation

Carbon lock-in originates in the literature on fossil-fuel infrastructure and the institutional complexes built around it (Unruh 2000; Seto et al. 2016). The mechanism examined here is structural and demographic rather than infrastructural: labour immobility rooted in subsistence agriculture, compounded in many countries by population ageing that raises fiscal pressure and narrows resources available for productive investment (Zheng et al. 2022; Organisation for Economic Co-operation and Development 2024b; Rezk et al. 2025). Agricultural economies are also disproportionately exposed to the output losses associated with temperature shocks (Dell et al. 2012), so the economies structurally least able to absorb climate finance productively are frequently the same economies most exposed to the damage that finance is intended to help them avoid. Agricultural value added per worker specifically has been linked to carbon-emission trajectories in fast-growing economies, where productivity gains free up labour and resources for cleaner activity elsewhere (Ye et al. 2023; Zhang and Li 2024).

2.3 Institutional allocation criteria

The Green Climate Fund, the largest dedicated multilateral climate fund, screens every proposal against six investment criteria: impact potential, paradigm-shift potential, sustainable development potential, recipient need, country ownership, and efficiency and effectiveness (Green Climate Fund 2024). Its Readiness and Preparatory Support Programme, which set a per-country ceiling of roughly USD 7 million over four years under its 2024 to 2027 strategy, strengthens institutional capacity and planning frameworks (Green Climate Fund 2025). A newer instrument, the Climate Finance Vulnerability Index, ranks 188 countries by combining climate-hazard exposure with financial and governance vulnerability (National Center for Disaster Preparedness, Columbia Climate School 2025). None of these three instruments, individually or together, incorporates the structural labour or productivity composition of the recipient economy, despite the underlying data being free, public, and updated annually. The Just Transition Mechanism agreed at COP30 in November 2025, which explicitly names smallholder farmers and rural economy actors as a priority population (COP30 Brazil Presidency 2025; Newell and Mulvaney 2013), gives this gap a concrete institutional opening: an allocation instrument built around agricultural populations is being actively designed at the point this study was written, without yet incorporating the indicators tested here.

3 Data and Empirical Strategy

3.1 Data

The underlying database covers 212 countries and territories with observations between 2000 and 2023, drawing on country-level series for carbon dioxide emissions per capita, total greenhouse gas emissions, international climate finance for clean energy in constant 2022 US dollars, agricultural labour share, agricultural value added per worker in constant 2015 US dollars, renewable electricity share, and the old-age dependency ratio. The three series central to this study, agricultural labour share, agricultural value added per worker, and climate finance, have their broadest joint coverage around 2015, with 128 countries observed, and no coverage after 2019 in this database. Every regression below therefore uses data no more recent than 2019, a pre-pandemic structural baseline. Because the two structural indicators move over decades rather than years, a country’s relative position in 2019 is a reasonable, though imperfect, guide to its position today, in a way a financial or macroeconomic indicator measured in the same year would not be. Once every variable required by a specification is jointly observed, the estimation sample narrows to between 110 and 125 countries. Research and development expenditure, missing for close to 60 percent of country-years, is excluded from the main specification.

Two additional series drawn from the World Bank’s World Development Indicators, gross domestic product per capita in constant 2015 US dollars and trade openness measured as total trade as a share of gross domestic product, are merged into the panel by country and year to serve as controls in Section 5 (World Bank 2025). Both variables are natural candidates for omitted-variable concern, since income level and trade exposure plausibly correlate with both agricultural structure and the propensity to receive climate finance.

Because roughly 30 percent of country-years record zero climate finance, the finance variable is transformed with the inverse hyperbolic sine,

$$\text{IHS}(F_{it}) = \ln\left(F_{it} + \sqrt{F_{it}^2 + 1}\right), \quad (1)$$

where F_{it} denotes climate finance for clean energy received by country i in year t , expressed in millions of constant 2022 US dollars. Expression (1) is defined at $F_{it} = 0$ and approximates $\ln(2F_{it})$ for large F_{it} , so it behaves like a log transform at the scale that matters for interpretation while retaining zero-finance observations.

3.2 Baseline specification

The baseline model is a two-way fixed effects regression,

$$\ln(\text{CO2}_{it}) = \alpha_i + \lambda_t + \beta_1 \text{IHS}(F_{it}) + \boldsymbol{\gamma}' \mathbf{X}_{it} + \varepsilon_{it}, \quad (2)$$

where α_i and λ_t denote country and year fixed effects, estimated by the within transformation, and $\mathbf{X}_{it}$ contains log renewable electricity share, log old-age dependency ratio, log agricultural labour share, and log agricultural value added per worker.

To test whether finance effectiveness depends on structural conditions, Equation (2) is extended with an interaction term,

$$\ln(\text{CO2}_{it}) = \alpha_i + \lambda_t + \beta_1 \tilde{F}_{it} + \beta_2 \tilde{Z}_{it} + \beta_3 (\tilde{F}_{it} \times \tilde{Z}_{it}) + \gamma' \mathbf{X}_{it} + \varepsilon_{it}, \quad (3)$$

estimated separately for two moderators Z_{it} , agricultural labour share and log agricultural value added per worker, with a tilde denoting standardisation to zero mean and unit standard deviation. Because Z_{it} enters directly, $\mathbf{X}_{it}$ excludes Z_{it} itself in each specification. The coefficient of interest is β_3 . A positive β_3 on the labour-share interaction together with a negative β_3 on the value-added interaction would indicate that finance loses effectiveness as agricultural labour dependence rises and regains it as agricultural productivity rises, consistent with an absorptive-capacity channel.

3.3 Inference

Two standard-error estimators are reported for the baseline and interaction models throughout: country-clustered errors, and Driscoll-Kraay errors, which remain valid under the cross-country dependence and serial correlation typical of long macro panels (Driscoll and Kraay 1998; Hoechle 2007). Section 5 adds a third estimator, two-way clustering by country and year, following Cameron et al. (2011),

$$V_{2\text{-way}} = V_{\text{country}} + V_{\text{year}} - V_{\text{White}}, \quad (4)$$

where V_{White} denotes the heteroskedasticity-robust, non-clustered sandwich estimator. A residual-based panel cointegration test, following Pedroni (1999), supports a levels specification over first-differencing and is reported in Appendix A.

3.4 Composite readiness score

A composite readiness score is constructed for the most recent year in which a country has complete data on agricultural labour share, ℓ_i , agricultural value added per worker, v_i , and finance received, a 2019 snapshot for most of the sample,

$$R_i = \frac{-z(\ell_i) + z(v_i)}{2}, \quad (5)$$

where $z(\cdot)$ denotes cross-country standardisation. A higher R_i corresponds to higher structural absorptive capacity. This score is intended as an illustrative screening heuristic rather than a validated predictive instrument; a five-year rolling-average version correlates with the single-year version at 0.997, indicating the ranking is not an artefact of one unusual year, but no project-level validation against realised finance performance is available.

4 Results

4.1 The empirical puzzle

Table 1 reports mean carbon dioxide emissions per capita and mean climate finance received across three structural stages. Figure 1 presents the same comparison graphically. Economies with more than 40 percent of their labour force in agriculture emit far less per capita than economies with less than 20 percent, while mean finance received moves in the opposite direction.

Table 1 Mean CO2 per capita and mean climate finance received, by structural stage (2000–2019 panel)

Structural stage	CO2 per capita, mean (t)	Finance received, mean (USD m)
Advanced (agr. labour share $\leq 20\%$)	8.02	50.67
Transition (20–40%)	2.59	61.89
Lock-in (agr. labour share $> 40\%$)	0.60	73.55

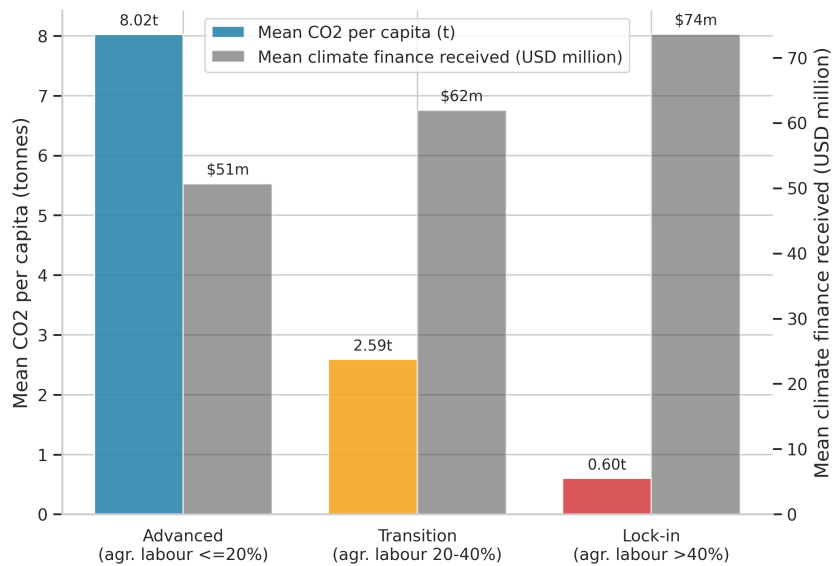


Fig. 1 Mean CO2 per capita and mean climate finance received, by structural stage.

Table 2 reports the descriptive statistics underlying the main sample.

Table 2 Descriptive statistics, main estimation sample

Variable	N	Mean	SD	Median	Max
CO2 per capita (t)	5,016	4.89	6.34	2.81	67.73
Old-age dependency ratio	5,076	12.74	8.34	9.33	50.12
Agricultural labour share (%)	3,640	28.01	23.54	22.08	91.76
Agricultural VA/worker (USD)	3,995	14,556.50	25,631.20	4,861.45	280,023.53
Climate finance (USD m)	3,552	61.30	215.67	0.36	4,832.06
Renewable electricity share (%)	4,735	31.73	32.83	19.05	100.00

4.2 Baseline and core interactions

Table 3 reports the baseline specification. The direct effect of finance on emissions is small and sensitive to the standard-error choice, positive but insignificant under country-clustered errors, positive and significant under Driscoll-Kraay errors. This instability motivates the interaction models that follow rather than reliance on a pooled average effect.

Table 3 Baseline two-way fixed effects estimates
(dependent variable: log CO2 per capita)

	Country-clustered SE	Driscoll-Kraay SE
IHS(finance)	0.0037 (0.0035)	0.0146*** (0.0029)
<i>N</i>	2,069	2,069
Countries	119	119

Standard errors in parentheses. *** $p < 0.01$. Controls (renewable electricity share, dependency ratio, agricultural labour share, agricultural value added per worker) and country and year fixed effects included but not shown.

Table 4 reports the central result. The interaction between finance and agricultural labour share is positive and significant under both standard-error estimators. The interaction with agricultural value added per worker is negative and significant, with a larger t -statistic in both specifications despite a somewhat wider standard error, reflecting a larger point estimate. The two moderators are negatively correlated with one another, $r = -0.501$, consistent with both indicators capturing the same underlying absorptive-capacity mechanism from two angles.

4.3 Regime-split evidence

Table 5 restates the interaction result as a discrete three-way split, and Figure 2 plots the three coefficients with 95 percent confidence intervals. The estimated effect of finance is positive and significant in the most agriculture-dependent group, falls to insignificance in the transition group, and is close to zero in the most structurally advanced group.

Table 4 Finance interacted with structural indicators (dependent variable: log CO2 per capita)

	Finance $\times$ agr. labour share		Finance $\times$ agr. VA/worker	
	Cluster-SE	DK-SE	Cluster-SE	DK-SE
Interaction term	0.0359*** (0.0100)	0.0337*** (0.0088)	-0.0524*** (0.0123)	-0.0631*** (0.0151)
N	2,069	2,069	2,520	2,520
Countries	119	119	122	122

Standard errors in parentheses. *** $p < 0.01$. VA denotes value added. Controls: log renewable electricity share plus, respectively, log agricultural value added per worker (left panel) and log old-age dependency ratio (right panel).

Table 5 Effect of finance on CO2 per capita, by structural regime, under three threshold choices

Threshold pair	Regime	N	Countries	Coefficient	p -value
15/35	Lock-in ($> 35\%$)	952	66	0.0130	0.020
15/35	Transition (15–35%)	758	65	0.0019	0.550
15/35	Advanced ($\leq 15\%$)	359	32	-0.0025	0.582
20/40 (main)	Lock-in ($> 40\%$)	766	57	0.0145	0.014
20/40 (main)	Transition (20–40%)	705	65	0.0039	0.124
20/40 (main)	Advanced ($\leq 20\%$)	598	50	0.0007	0.870
25/45	Lock-in ($> 45\%$)	636	46	0.0171	0.006
25/45	Transition (25–45%)	679	59	0.0043	0.123
25/45	Advanced ($\leq 25\%$)	754	56	-0.0007	0.858

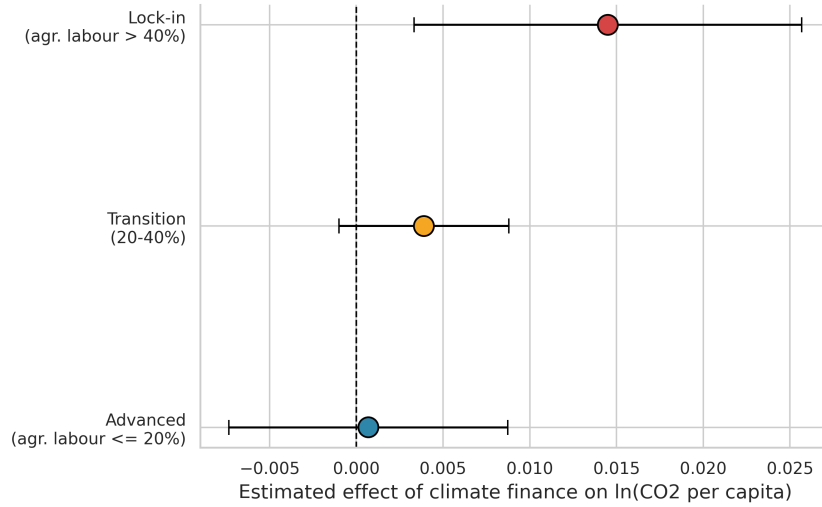


Fig. 2 Estimated effect of climate finance on log CO2 per capita, by structural regime, with 95 percent confidence intervals (main 20/40 threshold).

The qualitative ordering, largest and most significant in the most agriculture-dependent group, smallest and insignificant in the most advanced group, holds across all three threshold pairs, indicating the regime-split pattern is not an artefact of the specific 20/40 cutoff used in the main specification.

4.4 An illustrative application: the composite readiness score

Figure 3 plots the two structural indicators directly, with point size reflecting finance received and colour reflecting the composite readiness score defined in Equation (5). Among countries in the top 40 percent of the finance distribution in the 2019 cross-section, 25 of 51, just under half, score in the bottom two-fifths of the readiness distribution. Table 6 lists the six lowest- and six highest-readiness cases among these large recipients, and Figure 4 presents the full misallocation quadrant.



Fig. 3 Agricultural labour share and agricultural value added per worker, most recent available year per country. Point size reflects finance received; colour reflects the composite readiness score.

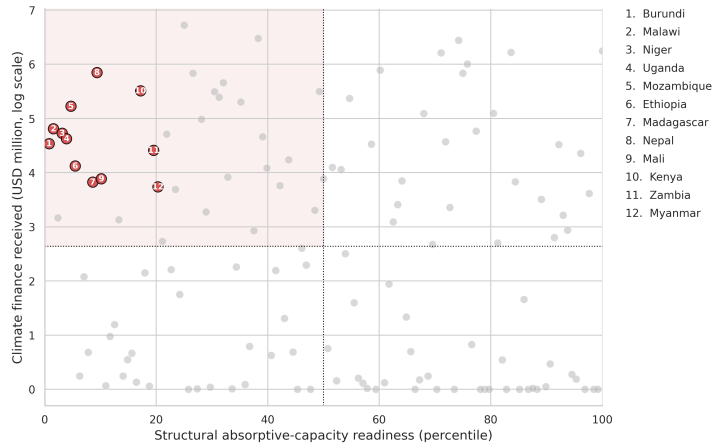
This application is illustrative rather than a core econometric result. It converts the interaction coefficients in Table 4 into a country-level statistic that could, in principle, be added to existing allocation screens at negligible cost, since both underlying series are already collected annually by existing multilateral statistical agencies.

4.5 Mediation through renewable electricity

Table 7 and Figure 5 test whether the pathway most commonly assumed to link finance to lower emissions, growth in renewable electricity generation, is empirically

Table 6 Lowest- and highest-readiness cases among top climate-finance recipients (2019)

Country	Lowest readiness			Country	Highest readiness		
	Fin. (USDm)	Agr. lab. (%)	Pctile		Fin. (USDm)	Agr. lab. (%)	Pctile
Burundi	92.0	86.2	0.8	Argentina	514.9	0.1	100.0
Malawi	121.5	76.4	1.6	Barbados	36.1	2.6	97.7
Niger	112.1	72.5	3.1	Jordan	77.0	2.5	96.1
Uganda	101.0	72.1	3.9	Montenegro	90.6	7.2	92.2
Mozambique	184.7	70.2	4.7	Mauritius	32.4	6.0	89.1
Ethiopia	60.8	66.6	5.5	Dominican Rep.	45.1	8.8	84.4

**Fig. 4** Structural readiness percentile against climate finance received, log scale. Highlighted points denote the twelve lowest-readiness cases among the top 40 percent of finance recipients.

detectable. In the full sample the indirect effect is indistinguishable from zero. Splitting by regime is more informative: the indirect effect is significant and negative in structurally advanced economies, and remains indistinguishable from zero in lock-in economies. The pathway most commonly assumed to carry finance into emissions reductions is therefore detectable only where it is least needed. This exploratory channel decomposition should be read as a descriptive pattern rather than a fully identified mediation result, since the interaction-based mechanism documented above plausibly alters both legs of the sequential pathway simultaneously.

5 Robustness and Identification

This section reports every robustness and identification check run, including where results are not fully clean, on the view that selective reporting would undermine rather than support the central claim.

Table 7 Exploratory channel decomposition: renewable electricity share as a candidate pathway

Sample	Indirect effect	Sobel z	p -value
Full sample	0.0001	0.43	0.666
Advanced (agr. labour $\leq 20\%$)	-0.0013	-2.09	0.036
Lock-in (agr. labour $> 40\%$)	0.0003	0.73	0.466

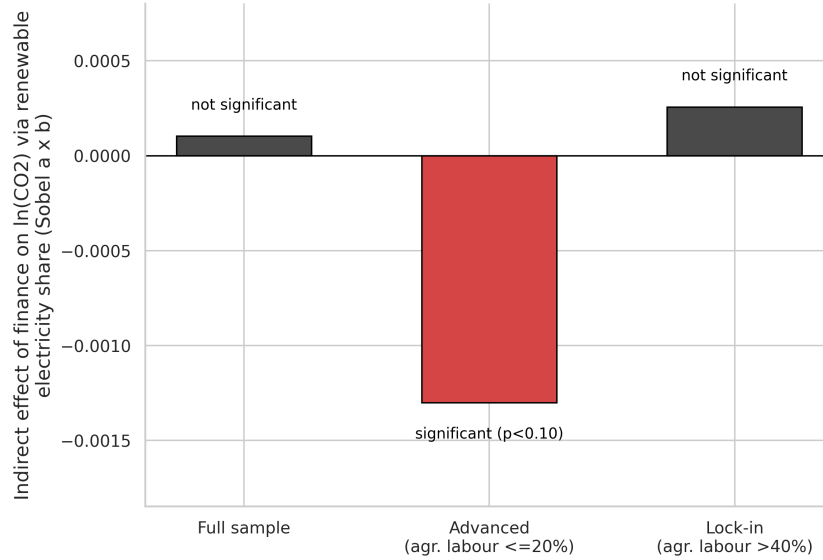


Fig. 5 Indirect effect of climate finance on log CO2 per capita through renewable electricity share, by structural regime.

5.1 Income and trade-openness controls

Table 8 re-estimates the two core interactions with gross domestic product per capita and trade openness added as controls, the two confounders most plausibly correlated with both agricultural structure and climate finance receipt. Both interaction coefficients retain their sign and remain significant at the 1 percent level, though the value-added interaction shrinks by roughly 30 percent in magnitude once income is controlled for, consistent with income capturing part, but not all, of the structural-productivity channel.

5.2 Absolute emissions

Per-capita emissions can fall simply because population grows while total emissions remain flat or rise. Table 9 re-estimates both core interactions using log total greenhouse gas emissions as the dependent variable instead of log per-capita CO2. The labour-share interaction remains significant and of the same sign, confirming the

Table 8 Core interactions with income and trade-openness controls added (Driscoll-Kraay SE)

	Finance $\times$ agr. labour share	Finance $\times$ agr. VA/worker
Without controls	0.0337*** (0.002)	-0.0631*** (0.001)
With GDP/capita and trade openness	0.0300*** (0.001)	-0.0446*** (0.001)

*** $p < 0.01$. p -values in parentheses. Both rows add log GDP per capita and log trade openness, each standardised, to the respective control set. Explanatory power rises sharply once income is added, with R^2 moving from approximately 0.20–0.41 without controls to approximately 0.55 with controls, indicating income level was an omitted confounder of meaningful magnitude, though the structural interaction survives its inclusion.

structural-lock-in channel is not an artefact of the per-capita transformation. The value-added interaction, by contrast, loses statistical significance on the absolute outcome, suggesting this particular channel operates partly, though not exclusively, through the denominator of the per-capita measure. Both results are reported here rather than only the one favourable to the paper’s central claim.

Table 9 Core interactions re-estimated on absolute total GHG emissions (Driscoll-Kraay SE)

Specification	Coefficient	p -value	N
Finance $\times$ agr. labour share (log GHG total)	0.0118***	0.003	2,061
Finance $\times$ agr. VA/worker (log GHG total)	-0.0067	0.249	2,509

*** $p < 0.01$. Compare with Table 4, where both interactions are estimated on log CO2 per capita.

5.3 Formal test of regime heterogeneity

Table 10 reports a Wald-type test of whether the three regime-specific coefficients reported in Table 5 differ statistically from one another,

$$z = \frac{b_1 - b_2}{\sqrt{SE_1^2 + SE_2^2}}, \quad (6)$$

using the cluster-robust standard errors already estimated for each regime subsample. The lock-in regime is statistically distinguishable from both the transition and advanced regimes, while the transition and advanced regimes are not statistically distinguishable from each other. The structural break, in other words, sits primarily between agriculture-dependent economies and the rest of the sample, rather than forming a smooth gradient across all three groups.

Table 10 Pairwise test of differences between regime-specific finance coefficients

Comparison	Difference	z -statistic	p -value
Lock-in vs. Transition	0.0107	1.706	0.088
Lock-in vs. Advanced	0.0138	1.964	0.050
Transition vs. Advanced	0.0032	0.665	0.506

5.4 Outlier robustness and two-way clustering

Agricultural value added per worker ranges from roughly 213 to 280,024 US dollars, a span exceeding a thousandfold, raising the concern that the interaction result is driven by a small number of extreme high-productivity observations. Table 11 reports the value-added interaction after winsorizing the moderator at the first and ninety-ninth percentiles, affecting 80 of 3,995 observations, alongside two-way clustered standard errors by country and year following [Cameron et al. \(2011\)](#). The coefficient is essentially unchanged in magnitude under both checks and remains significant at the 1 percent level throughout. The two-way clustered standard errors are close to the Driscoll-Kraay errors already reported in Table 4, indicating the Driscoll-Kraay choice was not understating uncertainty relative to the two-way standard.

Table 11 Robustness to outlier treatment and two-way clustering

	Finance $\times$ agr. labour share	Finance $\times$ agr. VA/worker
Two-way cluster SE (country $\times$ year)	0.0359*** (0.0098)	-0.0524*** (0.0131)
Winsorized, cluster-SE	—	-0.0527*** (0.0130)
Winsorized, DK-SE	—	-0.0624*** (0.0152)

5.5 Placebo test and a horse-race diagnostic

A falsification test interacts next year’s finance, rather than current finance, with each moderator. If the lead interaction still predicts current emissions, that outcome is inconsistent with a purely contemporaneous causal channel. Table 12 reports this test alongside two diagnostics constructed to interpret it. Both lead interactions are statistically significant, so the test does not pass cleanly. Climate finance is moderately persistent within a country, with a within-country correlation of 0.34 between finance in year t and year $t + 1$ and an AR(1) coefficient of 0.35, consistent with donor-recipient relationships that are sticky across years rather than reset annually. A horse-race specification including both the contemporaneous and the lead interaction in the same model shows the contemporaneous term remains significant in both specifications, but so does the lead term, and the two coefficients are similar in magnitude, particularly for the value-added specification. This pattern is consistent either with a genuine anticipatory or reverse-causality channel, or with the two regressors

being too collinear for the horse-race to separate cleanly. The data available here cannot adjudicate between these explanations, and this limitation is reported rather than resolved.

Table 12 Placebo test and horse-race: contemporaneous versus lead finance interaction

	Agr. labour share		Agr. VA/worker	
	Coefficient	<i>p</i> -value	Coefficient	<i>p</i> -value
<i>Panel A: placebo (lead only)</i>				
Lead(finance) $\times$ moderator	0.0262	0.004	−0.0583	0.001
<i>Panel B: horse-race (contemporaneous + lead)</i>				
Contemporaneous $\times$ moderator	0.0285	0.003	−0.0447	0.0003
Lead $\times$ moderator	0.0179	0.014	−0.0420	0.0005

Driscoll-Kraay standard errors. Both panels estimated on the same two-way fixed effects specification as Table 4.

An instrumental-variables specification using one- and two-period lags of IHS finance as instruments was also estimated. The first-stage F -statistic is 7.69, below conventional thresholds for instrument strength, and the resulting second-stage estimate, -0.0088 with $p = 0.155$, is not reported as a headline result for that reason. Combined with the imperfect placebo result, the paper cannot claim clean causal identification. A supplementary event-study around the 2015 Paris Agreement, reported in Appendix A, offers a complementary source of variation, but carries its own attribution concerns and is not treated as a substitute for a valid instrument.

6 Discussion and Policy Implications

The persistence of governance-only screening in current allocation practice is not simply an oversight. Governance and merit-based criteria give donors leverage over reform and reporting that a purely structural indicator does not, and empirical work on adaptation aid allocation finds that donor interests and recipient governance quality already shape who receives support, independently of vulnerability or need (Weiler et al. 2018; Betzold and Weiler 2017). A structural readiness score introduces its own risk. Because agricultural labour dependence and low-income status are correlated, a screen used mechanically could flag precisely the poorest, most agriculture-reliant economies, many in Sub-Saharan Africa, as least deserving of finance, penalising the countries with the least capacity to change their structural position quickly. The application in Table 6 is intended as an input to project design, a signal for what complementary investment a given amount of finance may need to be paired with, agricultural mechanisation and productivity growth (Ye et al. 2023; Zhang and Li 2024), pension and social-protection reform that reduces demographic pressure on subsistence agriculture (Zheng et al. 2022; Rezk et al. 2025), and support for rural-to-urban labour mobility (Lewis 1954), not as a threshold for exclusion.

The Just Transition Mechanism agreed at COP30 gives this proposal a concrete institutional opening. An allocation instrument built around agricultural and rural populations is being actively designed at the point this study was written, without yet incorporating either of the two indicators tested here ([COP30 Brazil Presidency 2025](#)). Adding them costs little, since the underlying data collection is already funded and published by existing multilateral statistical agencies ([World Bank 2007](#)).

7 Conclusion

Two structural indicators, agricultural labour share and agricultural value added per worker, both cheap, public, and updated annually, predict the effectiveness of climate finance with reasonable consistency across specifications. The pattern survives outlier winsorization, two-way clustering, the inclusion of income and trade-openness controls, alternative structural thresholds, and estimation on absolute rather than per-capita emissions, and the structural break is concentrated at the boundary separating agriculture-dependent economies from the rest of the sample rather than forming a smooth continuous gradient. A placebo test using future finance flows is not fully clean, and a horse-race diagnostic could not conclusively separate a genuine reverse-causality channel from the mechanical consequence of persistent donor-recipient financing relationships. This is reported as a genuine limitation rather than resolved definitively, and the paper’s evidence should accordingly be read as a robust, structurally grounded correlation rather than a clean causal estimate.

Three further limitations merit statement. First, the underlying agricultural series end in 2019, so all results describe a pre-pandemic structural baseline. Because the two indicators are structural rather than cyclical, moving over decades rather than years, this is a softer constraint than it would be for a financial or macroeconomic variable measured in the same year, but it remains a real one. Second, the composite readiness score operates at the level of the whole economy, while the appraisal decisions it is meant to inform happen at the level of individual projects. The score describes the macro-absorptive environment a project sits within, not a verdict on the project itself, and treating it as one would risk an ecological-fallacy error this study does not intend to license. Third, no data on project-level disbursement were available to validate the score against realised fund performance rather than country-aggregate emissions outcomes.

None of these limitations changes the basic shape of the finding. A substantial share of current climate finance is directed toward economies that a cheap, two-variable screen would flag as structurally unready to convert it into lower emissions, and the pathway most commonly assumed to carry finance into emissions reductions, renewable electricity expansion, is detectable only where it is least needed. Closing that gap does not require new data infrastructure. It requires treating structural readiness as one input among several in an allocation process that, at present, does not measure it at all.

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Declarations

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- **Competing interests:** The author declares no competing interests or conflict of interest.
- **Ethics approval:** Not applicable.
- **Consent to participate:** Not applicable.
- **Consent for publication:** Not applicable.
- **Data availability:** The dataset supporting the findings of this study is available from the corresponding author upon reasonable request.
- **Code availability:** The estimation code used to support the findings of this study is available from the corresponding author upon reasonable request.
- **Authors' contributions:** Quoc Lap Nguyen is the sole author of this manuscript and is responsible for conceptualization, methodology, formal analysis, data curation, writing, and editing.

Appendix A Event-study around the Paris Agreement

The 2015 Paris Agreement produced a plausibly exogenous shift in the global climate finance regime, through Nationally Determined Contribution-linked commitments and new reporting standards, independent of any single recipient country's own emissions trajectory. Table A1 interacts each core specification with a centred post-2015 indicator, and Figure A1 plots the implied pre- and post-2015 marginal effects with 95 percent confidence intervals, computed by the delta method from the full covariance matrix of the underlying Driscoll-Kraay estimator. The structural gradient strengthens sharply after 2015 in both specifications. This event-study lacks a full dynamic pre-trend analysis around the policy event and should therefore be read as suggestive corroborating evidence using a different source of variation, not as a substitute for the valid instrument this study lacks.

A residual-based Pedroni panel cointegration test on the baseline specification rejects the null of no cointegration ($N = 71$ countries, $t\text{-bar} = -3.50$, $Z = -20.89$, $p < 0.001$), supporting the use of a levels specification throughout the paper rather than first-differencing (Pedroni 1999).

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Table A1 Event-study: structural break in the finance-effectiveness gradient around 2015

	Finance $\times$ agr. labour share	Finance $\times$ agr. VA/worker
Baseline interaction (pre-2015 level)	0.0411***	−0.0516***
Triple interaction ($\times$ post-2015)	0.0951***	−0.0892***
(<i>p</i> -value)	(0.001)	(0.001)
Implied post-2015 effect	0.1362	−0.1408

Driscoll-Kraay standard errors. Post-2015 indicator centred at its sample mean before interaction.

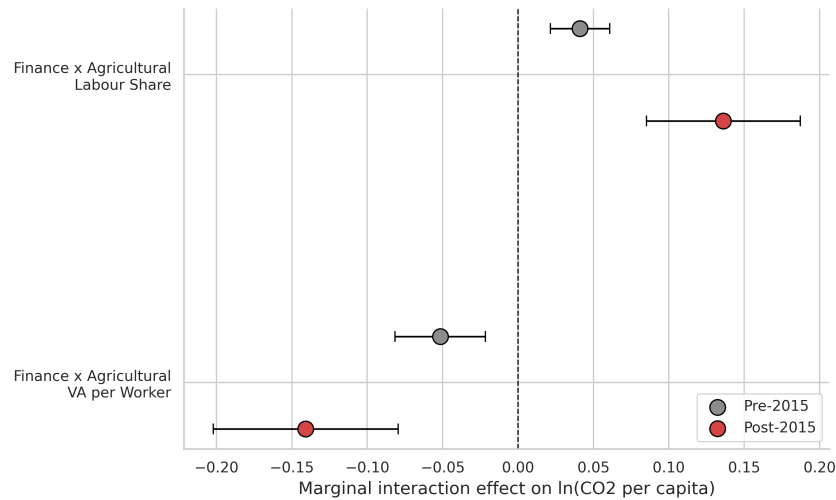


Fig. A1 Marginal interaction effect of climate finance on log CO2 per capita, pre- and post-2015, both core specifications, with 95 percent confidence intervals.

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