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Government effectiveness and Climate finance in sub-Saharan Africa: Nonlinear evidence on climate resilience and economic growth

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Abstract

This study investigates whether government effectiveness conditions the association between climate finance and two dimensions of sustainable development in sub-Saharan Africa: measured climate resilience and real per capita economic growth. The analysis uses a panel of 46 countries over 2013–2021 and combines country and year fixed effects with panel threshold regression and panel smooth transition regression. The main resilience specification detects a candidate government-effectiveness threshold near − 1.250, with a wild-cluster bootstrap p-value of 0.0275. Yet an alternative vulnerability-based measure yields no comparable break, with a p-value of 0.9375. For economic growth, the threshold test provides only marginal evidence of nonlinearity, with a p-value of 0.0775. A grid-based smooth-transition test using 999 bootstrap replications indicates improved fit, with a p-value of 0.004, and the estimated short-run association of lagged climate finance with growth becomes less negative as government effectiveness increases. These findings underscore that the empirical role of institutions depends on the outcome definition, model and inference procedure. Neither the candidate thresholds nor the smooth-transition profile identify causal institutional effects. The paper calls for evaluation frameworks that distinguish resilience preparedness from realised disaster losses and verify whether recorded climate-finance commitments translate into completed investments.

1. Introduction

The effectiveness of climate finance cannot be established solely by observing a higher volume of financial commitments. Between the initial allocation of funds and their economic or environmental consequences lie project selection, budgeting, procurement, disbursement, completion, operation and maintenance. Each stage depends on institutions that coordinate actors and discipline the use of public resources. This sequence is particularly important for climate adaptation because many benefits emerge as avoided losses rather than immediately measured output. Institutional capacity may therefore explain why a similar amount of recorded finance is associated with different outcomes across countries (Stern, 2007).

Sub-Saharan Africa provides a useful setting for examining this proposition. Climate exposure, economic structures, administrative resources and the quality of public services differ considerably across countries. Adaptation projects range from drainage systems and resilient roads to agricultural early-warning networks and coastal protection. Their effectiveness is not governed by a universal production function. The institutional conditions required to translate resources into operational assets may be particularly consequential where investment programmes encounter procurement delays, gaps in technical capacity and fragmented coordination among public agencies and external partners (Hallegatte et al., 2019; North, 1990; Trisos et al., 2022).

A second difficulty concerns the dependent variable. Climate resilience is sometimes measured through composite indicators combining vulnerability, social capacity and adaptive readiness. Such indices capture structural circumstances and preparedness, but they are not direct records of flood losses

avoided or agricultural output preserved after a particular shock. Government effectiveness may already be associated with components of these indices. Consequently, an estimated interaction or threshold involving both governance and a composite resilience outcome can partly reflect overlapping constructs rather than a new causal transmission mechanism (Adger, 2000; Folke, 2006; Holling, 1973).

Economic growth adds a distinct horizon. A country can strengthen preparedness through long-lived investments without displaying an immediate increase in real gross domestic product per capita. Conversely, spending, imported capital equipment and administrative implementation costs can precede the gains generated by a completed project. When flows are recorded before projects become operational, their short-run association with growth may be small or negative even though subsequent benefits are possible. This temporal separation cautions against treating resilience and growth as interchangeable proxies for sustainable development.

The literature provides plausible mechanisms without resolving their empirical strength in the same sample. North (1990) explains how institutional rules shape transaction costs and incentives, while Rodrik et al. (2004) emphasise the importance of institutions for development outcomes. Studies of climate-finance allocation and institutional design indicate that financing opportunities are subject to decisions on governance, implementation and donor priorities (Bhandary, 2024; Halimanjaya, 2015). Evidence from the wider aid literature also disputes the premise that external financial inflows necessarily translate into a robust average growth response (Rajan & Subramanian, 2008). What is less clear is whether these mechanisms appear as discrete institutional regimes or as gradual changes in the conditional association between finance and outcomes.

This study investigates whether government effectiveness conditions the relationship between climate finance and climate resilience or real per capita growth in 46 sub-Saharan African countries over 2013–2021. Two related hypotheses are examined. Hypothesis H1 concerns differences in the finance–resilience association across levels of government effectiveness. Hypothesis H2 concerns the attenuation of a negative short-run association between lagged climate finance and real growth as government effectiveness rises. Panel threshold regression, abbreviated PTR, and panel smooth transition regression, abbreviated PSTR, provide complementary representations of institutional heterogeneity.

The contribution is deliberately conditional. The article compares thresholds for a principal resilience index and a vulnerability-based alternative; it reports wild-cluster bootstrap uncertainty for searched threshold models; and it assesses smooth-transition evidence for growth with a documented grid search and 999 bootstrap replications. By foregrounding the differences across dependent variables and test procedures, the analysis provides a more informative account of the evidence than a single interaction coefficient. The results are associations subject to endogeneity, sample restrictions and outcome construction, not estimates of a universal causal effect of climate finance.

The article proceeds as follows. Section 2 develops the institutional mechanisms and competing empirical interpretations. Section 3 presents the data, measurement and estimation procedures. Section

4 reports the empirical findings and the corresponding tables and figures. Section 5 discusses economic mechanisms, policy implications and limitations. Section 6 concludes.

2. Conceptual framework and related literature

2.1. Institutional capacity as a complement to climate investment

The economic rationale for climate finance begins with underinvestment in adaptation where private incentives do not reflect the social value of preparedness. Infrastructure resilience, weather information and protection of shared assets generate positive externalities and often require public coordination. Yet the marginal value of additional financing depends on whether it finances suitable interventions that can be implemented and maintained. Financial resources and administrative capability are thus potentially complementary inputs: more money expands the feasible project set, while stronger institutions raise the likelihood that selected projects become effective services. This complementarity motivates the inclusion of government effectiveness as a possible conditioning variable rather than merely as another control.

Government effectiveness is not identical to the absence of corruption, administrative size or a country's income. The Worldwide Governance Indicators construct seeks to summarise perceptions of public service quality, civil service performance, policy formulation and implementation. As an aggregate estimate, however, it is an imperfect proxy for operational capacities at the project level. A high score does not establish that any particular climate expenditure is well managed. A low score does not imply that every funded project is unsuccessful. Interpreting its estimated threshold requires attention to this gap between broad institutions and project-specific mechanisms (Acemoglu et al., 2001; North, 1990; Rodrik et al., 2004).

Climate-finance allocation also creates an observational selection problem. Vulnerable countries may attract assistance precisely because their adaptive capacity is low; countries with credible institutions may attract funding because they can manage programmes. Both processes generate correlations between financing, governance and outcomes even in the absence of a causal funding effect. Research on climate-finance determinants and the wider development-aid literature therefore motivates specifications that distinguish conditional relationships from causal estimates and that avoid reading a statistically significant interaction as proof of an implementation mechanism (Bhandary, 2024; Halimanjaya, 2015; Rajan & Subramanian, 2008).

2.2. Resilience indices, institutional overlap and hypothesis H1

Resilience incorporates the ability to absorb, adapt to and recover from disruptions. Holling (1973) emphasises persistence under disturbance in his foundational ecological conceptualisation. Subsequent

social-ecological perspectives stress institutions, collective action, learning and adjustment (Adger, 2000; Folke, 2006). At the national scale these properties are difficult to observe directly. Composite indices therefore combine several indicators of vulnerability and readiness. Their usefulness for cross-country comparison comes at the cost of potential overlap: economic and governance variables may be present in both the explanatory environment and the measured outcome.

A positive coefficient on an interaction between climate finance and government effectiveness is consistent with an institutional complementarity mechanism, but it does not automatically show that administrative quality transmits funding into adaptation benefits. The problem is sharper if the outcome's readiness component already embeds institutional performance. For this reason, the first hypothesis is formulated in terms of conditional heterogeneity rather than a predetermined increase in resilience caused by governance. H1 states that the estimated finance–resilience slope varies with government effectiveness in the principal composite measure, subject to alternative-outcome checks.

An economically informative threshold should identify meaningful heterogeneity that does not disappear when the dependent variable is reconstructed. A threshold derived from a readiness-heavy index might indicate a relation involving administrative preparedness, while a vulnerability-based alternative may reflect exposure and sensitivity more directly. Divergence between the two estimates is therefore not a minor technical inconvenience. It provides evidence about the substantive interpretation of the result and the limits of labelling the coefficient a general climate-resilience effect.

2.3. Growth adjustment, nonlinear responses and hypothesis H2

The growth channel is analytically distinct from resilience preparedness. Climate expenditure can involve substantial planning costs, imported machinery and local resource reallocations before benefits materialise. The timing of commitments and disbursements is central: recording finance in a country-year does not ensure contemporaneous productive investment. Moreover, public capacity may influence whether a project is implemented on schedule, whether maintenance finance is secured and whether complementary infrastructure is available. This makes the short-run slope between lagged finance and growth a plausible candidate for institutional moderation. Related African evidence examines how regulatory quality conditions the finance–inclusive-growth relationship, although that outcome differs from the two analysed here (Ayana, 2026).

A discrete-regime account assumes that the finance slope changes when an observed governance indicator crosses an unknown cut-off. A gradual account assumes that improvements in governance progressively reweight two regimes. Both are testable representations, not institutional laws. PTR is useful for locating a candidate discontinuity, whereas PSTR represents a logistic continuum. The latter can model attenuation of a negative finance slope without requiring that the coefficient ever become positive. Its parameters must nevertheless be interpreted cautiously when identified by exploratory grid searches (González et al., 2005; Hansen, 1999).

H2 therefore predicts that the negative association between lagged finance and short-run growth is less pronounced at higher levels of government effectiveness. This expectation does not imply that low-quality institutions cause the negative association. Weak growth can itself shape fiscal management, external financing needs and observed governance scores. The results must consequently be read as conditional associations in the studied sample, with sample-specific thresholds rather than internationally applicable benchmarks.

3. Data and empirical strategy

3.1. Sample, source series and measurement

The analysis covers 46 sub-Saharan African countries from 2013 to 2021, giving a maximum of 414 potential country-year combinations. Climate finance is measured as the reported country-year amount in millions of United States dollars and is expressed in units of US$100 million for the nonlinear models. Government effectiveness, abbreviated GE, is measured using the Worldwide Governance Indicators. The outcomes are a composite national climate-resilience score and annual real per capita economic growth. Covariates include foreign direct investment, renewable-energy indicators and population, depending on the specification. The recorded finance measure does not necessarily correspond to disbursements or completed investments.

The growth measure is based on annual changes in constant-price gross domestic product per capita. Climate finance enters the growth regressions with a one-year lag. The principal resilience measure is a national composite indicator; the alternative specification draws on the Notre Dame Global Adaptation Initiative, abbreviated ND-GAIN, vulnerability information. Because readiness and vulnerability describe different dimensions of climate conditions, the two outcomes cannot be treated as interchangeable scales of the same underlying construct. Their comparison therefore provides a substantive assessment of outcome measurement.

Table 1 identifies the principal variables and the regression samples. The threshold models for the principal and alternative resilience measures each use 409 country-year observations. The growth threshold and transition models use 364 observations because of data availability and the lagged finance variable. Sample sizes are reported separately for each specification to clarify the population to which the estimated associations refer.

Table 1
Sample and variable definitions

Measure	Operational description	Coverage / unit
Climate finance (FC)	Recorded climate-finance amount	US$ millions; /100 in threshold models
Government effectiveness (GE)	Worldwide Governance Indicators estimate	Approximately − 2.5 to 2.5 scale
Main resilience outcome	Composite national resilience index used as the principal outcome	409 observations in PTR
Alternative resilience	Vulnerability-based ND-GAIN proxy	409 observations in PTR
Real per capita growth	Annual real GDP per capita growth	364 observations in nonlinear tests
Controls	FDI, renewable energy, log population, GE	As available by model
Note: The unit and operational interpretation of finance depend on the original reporting series. The table records inputs and analysis samples rather than implying that all 414 cells are complete.		

Source: Authors.

3.2. Linear benchmark and institutional interactions

The initial analysis describes the conditional link between finance and the outcome using country and year fixed effects and a financing-by-government-effectiveness interaction. For country i in year t, the generic linear representation is as follows. Its slope should be interpreted as the finance association at a given observed level of GE, not as the effect of exogenously changing government effectiveness.

$$Y_{i,t} = \mu_i + \lambda_t + \beta FC_{i,t} + \theta(FC_{i,t} \times GE_{i,t}) + \delta GE_{i,t} + X_{i,t}'\gamma + \varepsilon_{i,t} \quad (1)$$

Here Y denotes the relevant outcome, μ and λ denote country and year effects, X contains observed controls, and ε represents the residual. For the growth application FC is lagged by one year. Fixed effects absorb time-invariant country differences and common year shocks, but they do not remove reverse causality, measurement error or unobserved time-varying confounders. Country-clustered inference is used in the original analysis to recognise within-country dependence. Dynamic panel estimators raise additional specification and instrument-validity questions that are not resolved by these fixed-effects comparisons (Arellano & Bond, 1991).

3.3. Panel threshold regression and searched break inference

The PTR model follows the premise that the conditional finance slope may differ across values of the government-effectiveness indicator. The unknown threshold c partitions the panel into two regimes,

while all specifications retain common controls and fixed effects. The model is written as follows, where I is an indicator function and the growth version uses lagged finance.

$$Y_{i,t} = \mu_i + \lambda_t + \beta_1 FC_{i,t} I(GE_{i,t} \le c) + \beta_2 FC_{i,t} I(GE_{i,t} > c) + \delta GE_{i,t} + X_{i,t}'\gamma + \varepsilon_{i,t} \quad (2)$$

Following the threshold estimation logic of Hansen (1999), candidate cut-offs are searched over a trimmed portion of the observed GE distribution. A standard t-test applied after searching the cut-off is not sufficient: the threshold is unidentified under the null hypothesis of a common slope. The estimation therefore re-estimates a maximum test statistic on 41 candidate values and uses wild-cluster bootstrap inference at the country level. The reported p-values refer to the searched model and its specified outcome; they do not establish confidence intervals for individual causal effects or for a universal governance cut-off.

3.4. Smooth transition regression and bootstrap design

The PSTR alternative replaces the discontinuous regime indicator with a logistic transition weight. It permits the association with lagged finance to move continuously between regime-specific slopes as GE changes. The specification is particularly relevant when institutional capabilities accumulate gradually rather than changing at a precise cut-off. The simple logistic representation used in the reported comparison is:

$$Y_{i,t} = \mu_i + \lambda_t + \beta_0 FC_{i,t-1} + \beta_1 FC_{i,t-1}\, G(GE_{i,t}; c, \gamma) + X_{i,t}'\delta + \varepsilon_{i,t} \quad (3)$$

$$G(GE; c, \gamma) = 1 / \{1 + \exp[-\gamma(GE - c)]\}, \gamma > 0 \quad (4)$$

The grid analysis considers 104 combinations of candidate centres and transition speeds, and the wild bootstrap procedure contains 999 replications. It reports the maximum reduction in residual sum of squares relative to the corresponding linear model. The inferred p-value is conditional on this predefined grid and bootstrap implementation. It does not constitute a full post-selection inference procedure for a continuously optimised PSTR, nor does it by itself validate the economic interpretation of the estimated transition slope (González et al., 2005).

4. Empirical results

4.1. Distribution of government effectiveness and initial regimes

Figure 1 presents the distribution of government effectiveness in the pooled country panel. Its mass lies above the candidate H1 threshold of approximately − 1.2496, which is situated in the lower part of the empirical distribution. This matters for interpretation: the two regimes have very different observation counts, and the estimated lower-regime slope may be more sensitive to particular countries and years. A

reported threshold must not be read as a binding institutional performance standard or as a ranking of permanently weak and strong states.

The dashed line represents the candidate threshold in the main resilience specification; the underlying histogram is descriptive.

Source: Authors.

Table 2
Observations on either side of the principal resilience cut-off

GE regime	Country–years	Countries	Mean resilience index	Mean real growth (%)
GE ≤ − 1.2496	62	11	32.09	−0.28
GE > − 1.2496	352	41	40.18	0.72
Note: Regime counts are descriptive and may overlap across countries over time. These descriptive contrasts must not be interpreted as treatment–control comparisons.				

Source: Authors.

The descriptive comparison in Table 2 shows a lower mean resilience score in the below-threshold observations than in the above-threshold group. It also shows distinct average growth rates. None of these unconditional differences proves that institutional quality causes higher resilience or growth. They can reflect income, development history, exposure, financing selection or components shared between the governance measure and the resilience index. The purpose of the descriptive comparison is to characterise the heterogeneity for which the conditional model searches, not to replace the regression with a group-mean narrative.

4.2. Main resilience outcome: a detectable but measure-sensitive break

The principal PTR specification detects a candidate break at GE of − 1.2496. Its maximum test statistic is approximately 19.476, and the country-cluster wild-bootstrap p-value is 0.0275 from 399 replications. Within the specified model, this rejects an equal finance slope across the two regimes at the conventional 5% level. The result is consistent with institutional heterogeneity in the recorded finance–resilience association. However, the p-value evaluates a model comparison after a threshold search; it is not a probability that institutions are the true transmission mechanism.

Changing the dependent variable alters the inference sharply. For the alternative vulnerability-based resilience outcome, the candidate break shifts to approximately − 0.4875, while the maximum statistic falls to 4.105 and the bootstrap p-value rises to 0.9375. The model provides no corresponding evidence of a threshold for this outcome. This contrast is the most consequential robustness result for H1. It

prevents a general claim that governance uniformly amplifies the effect of climate finance on all dimensions of resilience.

Table 3
Searched PTR results across resilience and growth outcomes

Outcome	N	Candidate GE	supF	Bootstrap p	B
Main resilience score	409	−1.2496	19.476	0.0275	399
Alternative vulnerability score	409	−0.4875	4.105	0.9375	399
Real per capita growth	364	−1.1134	10.903	0.0775	399

Note: Each test searches 41 candidate thresholds and uses country-level wild-cluster inference. Outcomes and usable samples may differ.

Source: Authors.

Reference line indicates a 5% threshold for rejecting a common financing slope. The p-values are not effect sizes.

Source: Authors.

Figure 2 makes the differences across outcomes immediately visible. The main resilience score falls below the 5% reference line, the alternative outcome is far above it and the growth result lies between conventional 5% and 10% cut-offs. This arrangement does not rank the quality of the measures or select a preferred structural model. It indicates that the evidence for a threshold depends on the dependent variable and inference question. A researcher who reported only the strongest result would substantially overstate the empirical stability of the proposed institutional mechanism.

The divergence also has a conceptual explanation worth considering, though not confirming. A composite resilience score contains readiness-related information, which may itself be correlated with administrative capabilities. Vulnerability can be more closely tied to exposure and sensitivity, which do not necessarily respond within the study period to finance or changes in public administration. In the indicator diagnostics, the composite score correlates 0.8797 with readiness and − 0.6873 with vulnerability. This evidence supports a careful interpretation in terms of measurement composition, not a claim that readiness overlap fully explains the estimated threshold.

4.3. Economic growth: marginal PTR evidence

For real per capita growth, the searched PTR specification produces a candidate cut-off near − 1.1134 and a supF statistic of 10.903. The bootstrap p-value is 0.0775. At a 5% significance level the null of a common slope is not rejected; the evidence can be characterised as marginal at a 10% reference level. The distinction matters because a threshold model can offer an appealing economic story even when the reported test provides only weak support for the implied discrete change between regimes.

The economic direction suggested by the exploratory estimates is nevertheless relevant. The short-run association between lagged climate finance and real growth appears more negative at lower levels of government effectiveness and less negative at higher levels. Interpreting that pattern as attenuation does not require the high-governance slope to be significantly positive. It also does not establish whether observed financing depresses activity, since allocation to distressed or vulnerable countries and delayed investment returns could generate similar patterns. These interpretations become more compelling only with evidence on disbursement timing, project implementation and economic shocks.

4.4. Smooth institutional transition and the bootstrap comparison

The grid-based PSTR comparison offers stronger evidence of nonlinearity in the growth specification than the discrete PTR test. The preferred grid point has a centre of approximately − 1.1667 and a logistic speed parameter of 5.0. The observed maximum reduction in residual sum of squares is 120.937. In 999 wild-bootstrap replications, the estimated p-value is 0.004; the estimated 95th and 99th percentiles of the simulated maximum gain are 89.705 and 107.108, respectively. Thus the observed improvement exceeds both critical values within the specified grid-testing procedure.

Table 4
Grid-based PSTR growth comparison

Statistic	Estimated value
Countries / observations	46 / 364
Years with growth-model observations	8
Candidate centre, GE	−1.166736
Candidate transition speed, γ	5.000
Grid combinations	104
Observed maximum RSS reduction	120.9372
Wild-bootstrap replications	999
Grid bootstrap p-value	0.0040
Bootstrap 95th percentile	89.7054
Bootstrap 99th percentile	107.1082
Note: This is a fixed-grid comparison and not complete post-optimisation inference for continuously estimated PSTR parameters.	

Source: Authors.

Figure 3 depicts the logistic weight for the selected grid centre and speed. Its gradual increase in GE illustrates how a PSTR model can interpolate between two limiting finance slopes. The graph is a

visualisation of the fitted transition weight, not a direct graph of causal policy impacts. Its location and steepness depend on the chosen grid. Consequently, the centre should be reported as an estimate tied to these data and model choices; it must not be represented as a universal minimum requirement for effective governance.

This visualises the selected candidate centre and speed. It does not report confidence bounds for the finance coefficient.

Source: Authors.

The empirical bootstrap distribution in Fig. 4 provides a separate view of the significance calculation. Most simulated maximum gains fall below the observed improvement, which is the basis for the small reported p-value. Importantly, a low bootstrap p-value for model fit does not establish the statistical precision of every regime coefficient, nor does it demonstrate a particular institutional causal channel. Formal inference after continuous model selection, alternative bootstrap designs and sample sensitivity would be needed to provide stronger claims about the exact transition shape.

The vertical reference lines show the observed statistic and the bootstrap 95th percentile from 999 wild-bootstrap replications.

Source: Authors.

The combined evidence supports a differentiated interpretation of H2. The discrete threshold test alone is inconclusive under a strict 5% criterion. The predefined-grid smooth-transition test provides more pronounced evidence against a single linear relationship. The estimated finance slope becomes less negative with stronger GE, but this should be described as an attenuation pattern rather than proof that better institutions convert climate finance into positive near-term growth. These distinctions are crucial for a transparent account of exploratory nonlinear models.

4.5. Joint assessment and robustness boundaries

Table 5
Assessment of the two institutional hypotheses

Hypothesis	Result supported by reported tests	Important qualification
H1: GE moderates finance–resilience	Threshold detected for main score, p = 0.0275	Not replicated for vulnerability outcome, p = 0.9375
H2: GE moderates lagged finance–growth	PSTR grid test, p = 0.0040; slope attenuates	PTR evidence marginal, p = 0.0775; endogeneity unresolved
Note: Hypothesis appraisal reflects the available tests and does not imply causal identification or universal thresholds.		

Source: Authors.

Table 5 summarises the implications for the two hypotheses without collapsing their distinct evidentiary standards. The evidence for H1 is tied to the principal composite resilience score; it is not stable across the alternative measurement. H2 receives support from a grid-based transition comparison and the direction of conditional finance slopes, but the discrete threshold test is weaker. Reporting these differences is more scientifically informative than marking either hypothesis simply “accepted” or “rejected.”

The underlying sample also raises limitations of statistical leverage and precision. The lower governance regime contains a relatively small share of country-years and only 11 distinct countries in the tabulated descriptive comparison. The nonlinearity tests rely on country-level bootstrap procedures precisely because repeated observations from the same country are not independent. However, finite-sample inference may remain sensitive to the distribution of explanatory variables, the placement of the trimming interval and the concentration of finance in a few observations. Without comprehensive model-level replication and sensitivity analysis, these limitations should remain explicit.

The three outputs are further not interchangeable as estimators. An interaction term in the linear fixed-effects model reports an average variation in slope proportional to GE. The PTR represents a discrete regime split found through a search. The PSTR allows a smooth weighting function whose parameters are identified jointly with slope differences. Disagreement across them can arise because the underlying shape is not captured equally well, because of finite-sample noise or because model assumptions differ. A consistent substantive claim must therefore refer to the particular estimated object rather than treat all three procedures as interchangeable corroboration.

5. Discussion and economic implications

5.1. Administrative conversion rather than financing volume alone

The results support a research agenda in which institutional quality is studied as a condition of financial conversion rather than an automatic determinant of programme success. A country can obtain funding and still face delays in procurement, coordination challenges or inadequate maintenance provisions. Conversely, strong project design and implementation can preserve physical and productive assets in ways that are difficult to detect immediately in GDP figures. This distinction is consistent with an institutional economics interpretation of coordination costs and enforceable rules (North, 1990; Rodrik et al., 2004), although the regressions do not observe the individual stages of the financing-to-investment chain.

The empirical value of the institutional hypothesis lies in its contrast across outcome dimensions. The resilience model indicates a candidate threshold only when a composite index is employed; the vulnerability alternative does not support the same threshold. If institutions matter especially for planning, information and readiness, a composite preparedness measure may reflect those channels

more readily than a vulnerability series dominated by physical exposure. This is a testable explanation, not an identified mechanism. Future studies could investigate separate readiness components, administrative budgets, procurement records and exposure-adjusted disaster outcomes to assess this possibility.

For growth, the attenuation of a negative finance coefficient as GE rises is compatible with the premise that capable institutions reduce implementation bottlenecks. Yet it is equally compatible with the selection of finance toward countries facing different economic conditions. An effective interpretation must distinguish a financing commitment from the associated net inflow, from an expenditure, and ultimately from productive capital. Without project timing and composition, a country-year association with lagged finance cannot identify the economic return on adaptation investments. Claims about policy effectiveness must therefore remain proportional to the level of measurement.

5.2. What the two empirical outcomes can and cannot establish

The term sustainable growth can encourage an interpretation that combines environmental, social and economic progress. The empirical variables in this paper, however, represent two narrower and distinct dimensions: national climate preparedness and short-run real per capita growth. Neither is sufficient to establish a country's intergenerational sustainability. A composite resilience index can increase as governance-related readiness rises even if hazard exposure remains high; real GDP can increase even while ecological assets deteriorate. The choice to analyse these dimensions separately is therefore a measurement discipline and an important limitation.

The observed difference between the main resilience result and its alternative measure illustrates why construct validity is as important as statistical significance. The robustness question is not only whether a coefficient changes sign or p-value; it is also whether the dependent variables continue to represent the same economic concept. If a governance threshold is identified mainly in a readiness-oriented score, its interpretation should foreground preparedness and administration. The failure to detect a threshold in vulnerability should not be dismissed as an inconvenient check, because it puts a boundary on the scope of the result.

The growth findings raise a parallel inferential issue. A flexible nonlinear model may improve fit by allocating different weights to observations at different governance levels. That property is valuable for characterising heterogeneity, but it is not sufficient to justify setting a governance cut-off as an aid eligibility condition. A mechanical funding rule might exclude highly vulnerable countries precisely because their administrative capabilities are constrained. The evidence instead favours treating institutional support, monitoring and project preparation as complementary components of financing programmes.

5.3. Relation to earlier evidence and contribution

A central insight from the development-aid literature is that financing inflows have no guaranteed, stable growth coefficient across countries and models (Rajan & Subramanian, 2008). The current analysis extends the issue to climate finance by considering whether institutional conditions change the short-run association. The results are suggestive but do not resolve the old causal identification problem. The finance variable can respond to recipient needs, while government effectiveness may be jointly determined with income and state capacity. Accordingly, the contribution is a conditional description of cross-country patterns rather than a causal claim that governance reforms alter the return to aid.

Institutional interpretations also connect with the literature on investment selection and national climate funds (Bhandary, 2024; Halimanjaya, 2015). In practice, programme preparation and data transparency may be as consequential as the headline funding amount, particularly where climate investments take years to complete. The empirical analysis gives a rationale for measuring these intermediate steps more carefully, but cannot establish which mechanism explains the discovered nonlinear patterns. This distinction between plausible mechanisms and tested channels avoids converting broad theoretical arguments into unwarranted descriptions of the estimated coefficients.

The African climate-adaptation literature stresses heterogeneity in exposure, vulnerability and capacity (Trisos et al., 2022). The present comparison reflects that diversity, but the panel cannot map all geographic or sectoral differences. One million dollars of concessional support may have very different consequences in a small island economy, an arid agricultural region or a large diversified state. The study therefore cautions against reading sample-average or regime-specific slopes as a common marginal project return. Its methodological contribution is to make outcome construction, threshold uncertainty and model comparison visible in one analytical framework.

5.4. Limits, transparency and future replication

Several limitations define the boundary of inference. First, finance observations do not consistently distinguish commitments from disbursements and completed expenditures. Second, the index of government effectiveness is an aggregate proxy that does not measure the functioning of climate-project institutions directly. Third, the resilience measure partly overlaps with readiness and governance-related concepts. Fourth, missing observations and lagged variables change the effective regression sample. Finally, model search affects the statistical distribution of test statistics, making finite-sample bootstrap design and replication especially important.

The reported threshold statistics, model grids and bootstrap estimates can be checked against the retained estimation output tables. However, the complete executable estimation routines underlying every panel threshold and smooth-transition coefficient are not available. Consequently, the manuscript distinguishes between documented numerical outputs and procedures for which independent end-to-end replication remains incomplete. Figure-generation scripts reproduce the four graphical displays from the retained inputs; they do not independently re-estimate all nonlinear coefficients. This limitation should be resolved before making stronger claims about structural effects.

Future research would benefit from linking project-level disbursement dates, implementing agencies and completion status to country-year outcomes. A design exploiting plausibly exogenous allocation changes or staggered programme implementation could better address reverse causality. An independent measure of resilience based on observed damage recovery would reduce construct overlap with public-sector readiness. Alternative threshold searches, cluster-bootstrap designs and out-of-sample model comparisons could also establish whether the estimated nonlinearities are stable beyond the original data and search ranges.

6. Conclusion

This study investigates whether government effectiveness conditions the relation between climate finance, resilience preparedness and short-run real growth in sub-Saharan Africa. The main resilience specification reveals a candidate institutional threshold around − 1.25, significant under a country-cluster bootstrap. The corresponding test with an alternative vulnerability-based outcome does not reveal a comparable break. For growth, evidence of a discrete threshold is marginal at conventional levels, whereas a fixed-grid PSTR test using 999 bootstrap replications indicates a more pronounced improvement in nonlinear fit. Across the growth specifications, the finance association becomes less negative as the government-effectiveness indicator increases.

The policy implication is not that a unique governance threshold determines whether climate finance "works." Rather, the findings motivate closer investigation of how financial commitments become effective investments and why environmental preparedness and economic output respond on different horizons. Robust evaluation requires disbursement and project-completion information, independent measures of realised climate outcomes and transparent documentation of model selection. The present evidence is consistent with institutional heterogeneity but cannot isolate causal effects of governance or funding.

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Figures

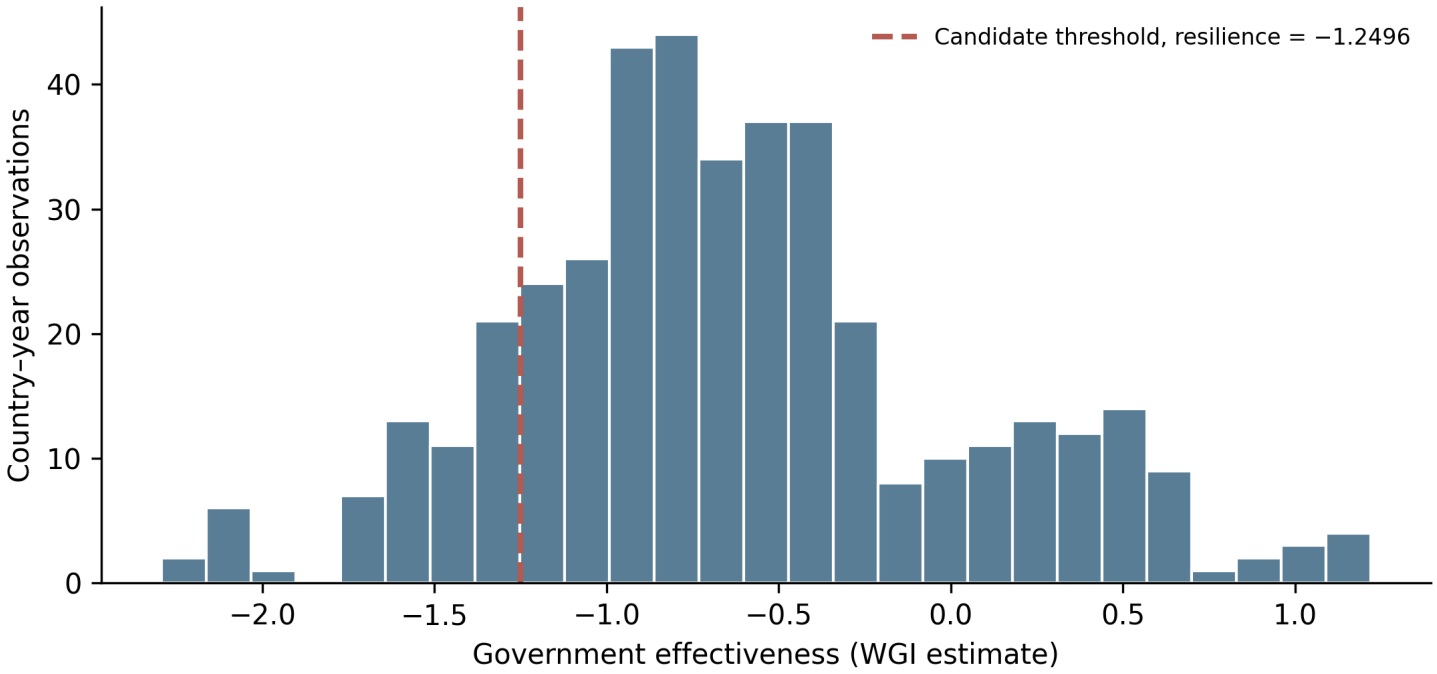

Figure 1

Figure 1. Distribution of government effectiveness and the resilience threshold.

The histogram illustrates the distribution of government effectiveness across the sampled Sub-Saharan African countries. The vertical dashed line identifies the estimated threshold of −1.2496 obtained from the panel threshold regression for the principal climate resilience indicator. The distribution highlights the unequal concentration of observations across institutional regimes.

Source: Authors.

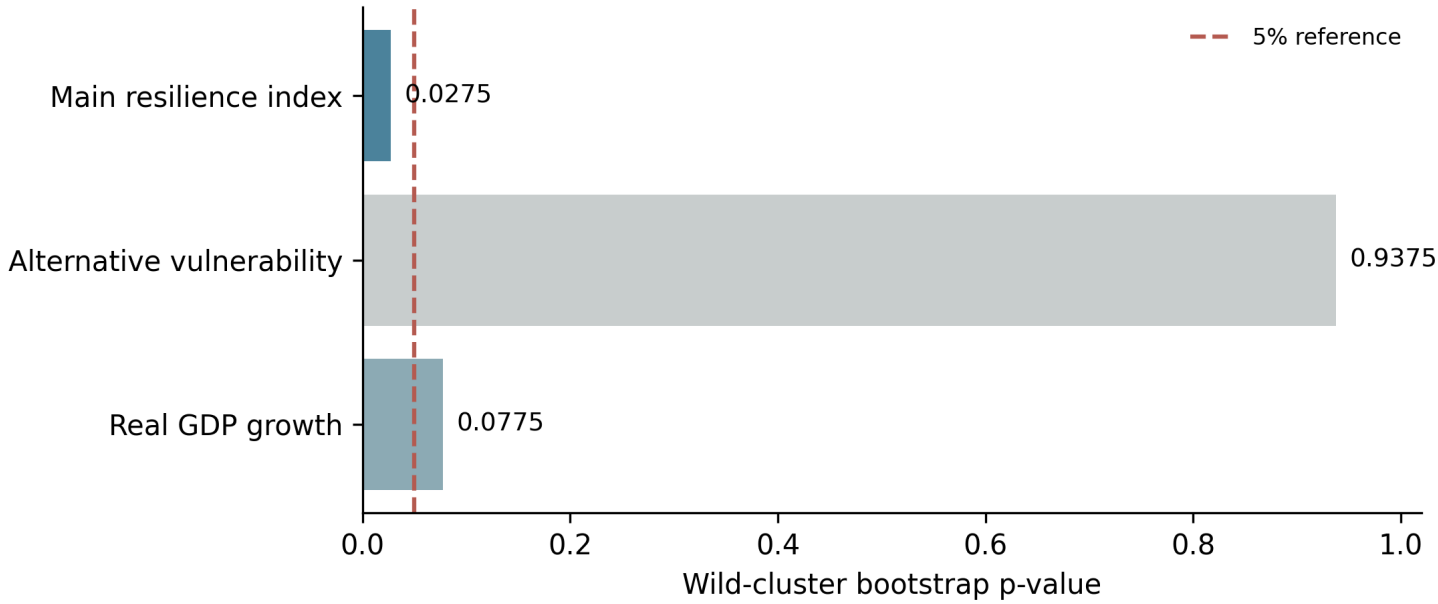

Figure 2

Figure 2. Bootstrap significance of institutional thresholds across outcome variables.

The figure compares the bootstrap p-values from panel threshold regressions for the principal climate resilience indicator, the alternative vulnerability measure, and real per capita economic growth. The corresponding p-values are 0.0275, 0.9375, and 0.0775, respectively. The horizontal reference line indicates the 5% significance level. The results demonstrate that evidence of institutional threshold effects depends on the outcome considered.

Source: Authors.

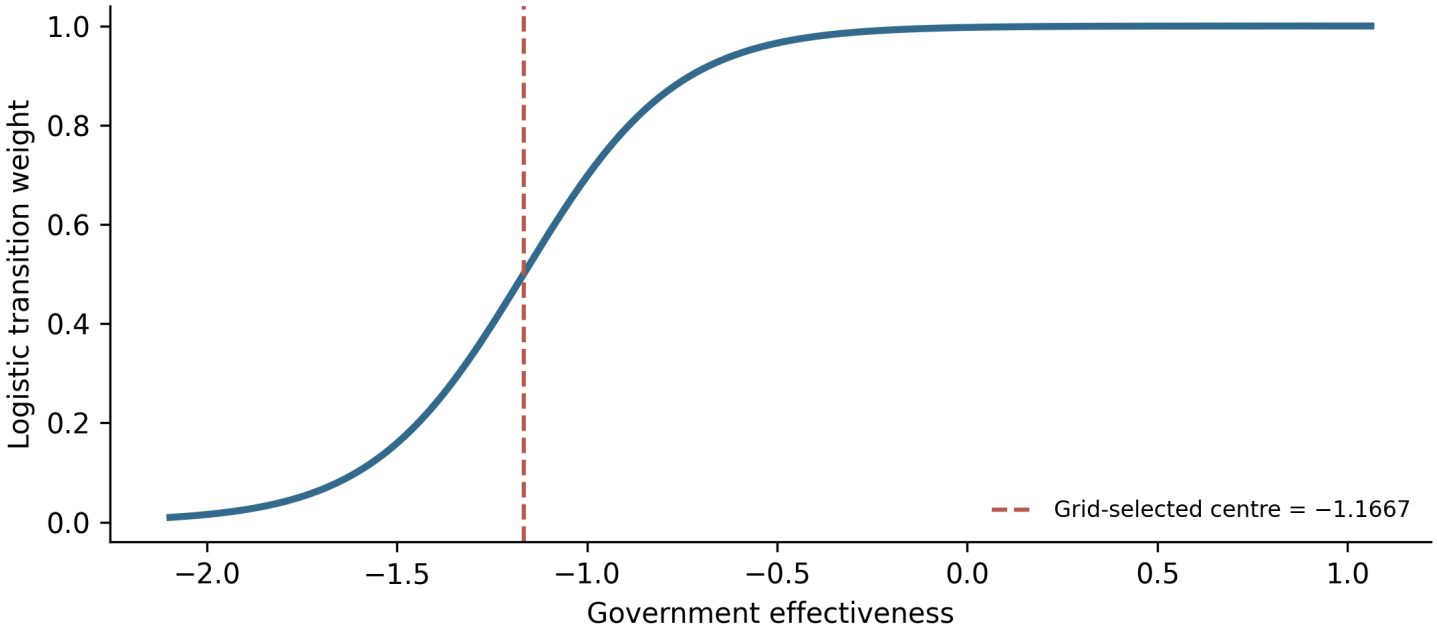

Figure 3

Figure 3. Logistic transition function of government effectiveness.

The figure presents the estimated logistic transition function used in the panel smooth transition regression. The selected transition centre is −1.1667, with a transition speed parameter of 5.0. The function illustrates the gradual change in regime weights as government effectiveness increases, allowing the association between climate finance and economic growth to vary across institutional conditions.

Source: Authors.

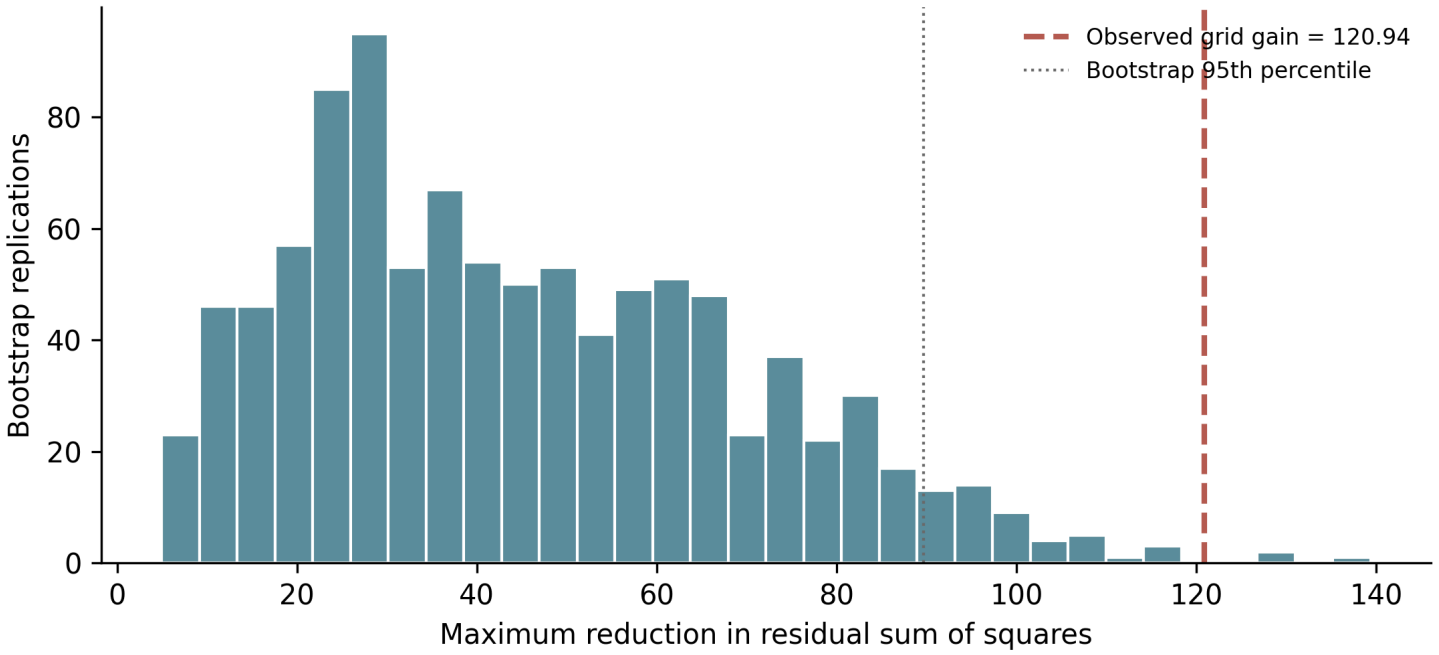

Figure 4

Figure 4. Bootstrap distribution of the maximum improvement in model fit.

The figure displays the bootstrap distribution of the maximum reduction in residual sum of squares obtained from the panel smooth transition regression. The observed statistic is 120.9372, compared with a bootstrap 95th percentile of 89.7054. The distribution is based on 999 wild bootstrap replications. The observed statistic exceeds the critical value, providing evidence against the linear specification within the selected transition grid.

Source: Authors.