

This manuscript presents an ambitious and methodologically sophisticated analysis of the influence of ENSO on vegetation variability across interannual to multi-decadal timescales using CMIP6 piControl simulations. The use of long control runs, combined with spectral, mediation, and partial spectral analyses, provides a novel perspective on land-climate interactions and the potential role of vegetation as a low-frequency amplifier of ocean-atmosphere variability.

The study is timely and relevant, and several aspects are particularly strong. The multi-model framework enhances robustness compared to previous single-model studies, and the attempt to connect vegetation dynamics (LAI) with carbon fluxes (GPP, NPP) is valuable. The frequency-domain approach, including gain, coherence, and phase diagnostics, is innovative in this context and offers useful insights into the timescale-dependent behaviour of the system.

However, despite these strengths, there are a number of important methodological and interpretational issues that need to be addressed before the manuscript can be considered for publication. In particular, the treatment of causality, the robustness of the spectral analysis, and inconsistencies in the methodology and data description raise concerns. Several claims are currently overstated relative to what can be supported by the methods used.

Main comments

Overinterpretation of causality: The manuscript repeatedly refers to “causal relationships” (e.g. ENSO-LAI, PDO as mediator). However, the methods employed (regression, mediation analysis, partial spectral analysis) are based on linear statistical relationships and do not establish causality in a strict physical sense. This issue is particularly important given the strong coupling between ENSO, PDO, radiation, precipitation, and soil moisture. The language throughout the manuscript should be revised to reflect statistical association rather than causation.

Potential circularity in the methodology: The vegetation index (TLAI) is constructed via regression onto ENSO and is subsequently analysed in relation to ENSO. This raises a risk of circularity, whereby coherence, reddening, and gain may partly reflect the construction of the index rather than an independent dynamical response. This limitation should be explicitly acknowledged and discussed.

Model agreement and regional analysis: A more critical discussion of inter-model agreement is needed. While the manuscript emphasises coherent large-scale patterns, there is in fact substantial divergence among the 11 models over tropical forest regions (e.g. Amazon and Congo), as also visible in Figs. 3 and S1. In contrast, model agreement appears much stronger over semi-arid regions, where the sign and structure of the ENSO-vegetation relationship are more robust. This contrast is important and should be explicitly discussed. One possible explanation is the saturation of LAI in dense tropical forests, which limits sensitivity to climate variability, whereas semi-arid ecosystems are strongly water-limited and thus more responsive to ENSO-driven hydroclimatic fluctuations. These regions have been identified as key contributors to interannual variability in the global carbon cycle and vegetation dynamics (Poulter et al. 2014; Ahlström et al. 2015). The authors are encouraged to further explore and discuss the dominant role of semi-arid regions in driving robust ENSO-vegetation coupling, and to clarify how model uncertainty in tropical forests affects the overall conclusions.

Observational uncertainties: The comparison with satellite products is not sufficiently critically assessed. The GLASS datasets rely on AVHRR multi-sensor records, which are known to suffer from calibration drift, orbital changes, and cloud contamination (Kaufmann et al. 2000; Frankenberg et al. 2021). These issues are particularly problematic for low-frequency variability and should be explicitly acknowledged.

Inconsistency between piControl and historical/observational data: The comparison between piControl simulations (with fixed land cover) and historical/observational data (which include land-use change and PFT transitions) is not discussed. This inconsistency may affect the interpretation of vegetation variability and should be addressed.

Detailed comments

Introduction

The introduction would benefit from discussing recent work by (Zhou et al. 2025), which directly examines the influence of ENSO on global vegetation dynamics from a resilience perspective. This study shows that ENSO not only drives interannual variability but also modulates vegetation resilience and its variability under climate change, highlighting a pathway through which ENSO impacts can persist beyond immediate responses. Incorporating this reference would help position the present study within the emerging literature that links ENSO variability to longer-term ecosystem dynamics and memory effects.

Methods

Line 102 The statement that “500 years of monthly data was extracted” requires clarification. The selection criteria (e.g. last 500 years, post spin-up segment) should be specified and justified, as piControl simulations may contain drift or regime-dependent variability.

Lines 110-115 The claim that agreement among 9 out of 11 models corresponds to 95% confidence under a two-sided binomial test is not correct. For $n = 11$, this threshold does not meet the 5% significance level. This criterion should be revised or properly justified.

Line 141 Replace “observations” with “simulations” when referring to CMIP6 historical output.

Section 2.3 The observational datasets should explicitly state their AVHRR multi-sensor origin.

Figure 1 The label “(excl. ENSO)” in the top-right should be removed or revised. As currently formulated, this annotation is potentially misleading, since the mediation framework does not fully isolate ENSO effects in a strict causal sense. Only the term corresponding to the indirect pathway ($\alpha\beta$) explicitly represents the ENSO-mediated contribution. The current labeling could therefore be misinterpreted as indicating a complete removal of ENSO influence, which is not strictly correct within the statistical framework used. The correct way to exclude ENSO is ($\beta - \alpha\beta$).

Results

Section 3.1 It is unclear whether the data are detrended. This should be specified. If detrended, the use of terms such as “greening” and “browning” is inappropriate and should be replaced by “positive/negative correlation”.

Lines 312-313 The claim of model agreement over the Amazon is not supported by the figures, which show substantial inter-model spread. In contrast, agreement appears stronger over semi-arid regions.

Section 3.2 The regional spectral analysis (Fig. S2) is not sufficiently robust given the large model disagreement in the selected regions. The choice of regions should be justified, and uncertainty across models should be quantified. In addition, fixed geographical regions may obscure model-specific teleconnection patterns and should be discussed. More fundamentally, the spectral results may partly reflect the construction of the ENSO-based vegetation index, and this should be acknowledged.

Section 3.3

- First, some of the variables introduced in this section (e.g., RSDS, MRSOS, and deep-layer soil moisture defined as $MRSO - MRSOS$) are not clearly described in the Data and Methods section. It is essential that all variables used in the analysis are explicitly introduced earlier, including their definitions, sources, and preprocessing steps, to ensure transparency and reproducibility.
- Second, it is unclear whether these variables are treated consistently with the rest of the manuscript in terms of preprocessing (i.e., deseasonalization and detrending). This point is particularly important for the interpretation of the results. If the data are deseasonalized and detrended, then for surface downwelling shortwave radiation (RSDS), the remaining variability would primarily reflect cloud-induced anomalies rather than the direct seasonal radiative forcing. This should be explicitly acknowledged, as it affects the physical interpretation of RSDS as a driver of vegetation variability.
- Third, the definition of deep-layer soil moisture as the difference between $MRSO$ and $MRSOS$ requires clarification regarding units. If soil moisture is expressed in units of kg m^{-2} (total water content), then this subtraction is physically meaningful. However, if the variables are expressed in volumetric units ($\text{m}^3 \text{m}^{-3}$), the difference may not be physically interpretable without accounting for soil depth. The authors should therefore explicitly state the units and justify the calculation.
- More generally, the interpretation of partial spectral analysis as evidence for specific physical “memory” mechanisms should be treated with caution. The removal of the linear influence of a variable does not necessarily isolate a unique causal pathway, especially given the strong coupling between radiation, precipitation, and soil moisture in the climate system. For example, RSDS, precipitation, and soil moisture are not independent drivers but are dynamically linked through atmospheric processes. This interdependence complicates the attribution of causality based solely on partial coherence or gain. Additionally, the interpretation that near-surface soil moisture acts as the primary memory source is plausible but may be somewhat overstated given the relatively modest differences in partial gain and the presence of considerable inter-model spread (visible in Fig. 4). Providing uncertainty estimates or model-by-model diagnostics would strengthen this conclusion.
- Finally, it would be helpful to clarify whether the results are robust across regions with strong model agreement, as the mechanisms inferred here may be sensitive to the spatial heterogeneity and inter-model variability highlighted earlier in the manuscript.

Section 3.4 The apparent buffering of NPP by autotrophic respiration may not reflect an active compensatory mechanism, but rather the fact that GPP and respiration are both driven by common climatic forcing and are tightly coupled in the model structure. This distinction should be clarified to avoid overinterpretation.

Discussion and conclusions

The Discussion would benefit from a clearer and more explicit acknowledgement of the limitations of the study.

- First, the results are predominantly model-based, relying heavily on CMIP6 piControl simulations. While these simulations are well suited to isolate internal variability, they do not include observational constraints on multi-decadal to centennial timescales. The comparison with observations is limited to a relatively short period, which restricts the ability to validate the key findings on low-frequency variability and amplification. This limitation should be more clearly stated.
- Second, the observational datasets themselves carry important uncertainties. The vegetation products used (GLASS) are based on AVHRR multi-sensor records, which are known to suffer from calibration drift, orbital changes, and atmospheric contamination. These issues are particularly critical when analysing long-term variability and may affect the robustness of the inferred ENSO-vegetation relationships.
- Third, there is an inherent inconsistency between piControl simulations and historical/observational data. The piControl experiments assume fixed land cover and plant functional types, whereas historical simulations and observations include land-use change and vegetation shifts (e.g. deforestation, cropland expansion). This difference may influence vegetation variability and its coupling to ENSO and should be explicitly discussed.
- Fourth, the study relies on indices derived from ENSO itself (e.g. ENSO-regressed LAI), which introduces potential methodological dependence. As a result, some of the reported relationships, particularly in the spectral analysis, may partly reflect the construction of the indices rather than independent dynamical behaviour. This limitation should be acknowledged when interpreting the results.
- Finally, the analysis is restricted to linear statistical methods, which do not capture potential non-linear interactions between climate variability and vegetation dynamics. This may be particularly relevant in regions where ecosystem responses are threshold-dependent or strongly non-linear.

References

- Ahlström A, Raupach MR, Schurgers G, et al (2015) The dominant role of semi-arid ecosystems in the trend and variability of the land CO₂ sink. *Science* 348:895–899. <https://doi.org/10.1126/science.aaa1668>
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Closing remark

Despite the comments above, I find the overall scientific narrative compelling. The manuscript presents an original and well-structured approach, and the combination of spectral and mediation analyses is particularly innovative. I thank the authors for the clarity of the work and the methodological developments, which represent a valuable contribution to the field.