Answer to Reviewer 1

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.

We thank the reviewer for their comments and helpful suggestions, which we have addressed in a revised version. We have uploaded a revised version as well as a version with track changes. Below, we address each of the comments (original comments in black and answers in blue).

Main comments

<u>1. Overinterpretation of causality:</u> 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.

We thank the reviewer for raising this point about the use of the terminology “causal relationships”, and we have taken the opportunity to clarify what we mean by this terminology more explicitly in the revised manuscript. However, we would like to keep using the term "causal" in the manuscript in agreement with the terminology used in previous literature (e.g., in mediation analysis (MacKinnon et al. 2000) and broader causal inference methods (Pearl et al. 2016)). These methods are specifically designed to infer causal relationships from observational or model data, without requiring additional perturbation experiments. We therefore consider it appropriate to use the term "causal" in the sense established by this literature. We have added the following sentence in the method section to explain how we use the term “causal” throughout the analysis (L. 175-176): “In line with this literature, we use “causal" to describe a statistical relationship in which the direction of influence is established by a systematic causal inference methodology.” We have also changed “direct causal impact of ENSO on LAI” to “direct pathway” in the abstract, because this important abstract statement comes before we have established what we mean by causal.

Some features of our specific setup also strengthen the case for directional inference. The use of the piControl simulations excludes the impact of external forcing which could otherwise contaminate causal estimates in transient simulations or observations. Additionally, our partial spectral analysis goes beyond standard mediation analysis by directly resolving the temporal lead-lag relationship (and thus directionality) through the phase spectrum. Furthermore, our mediation/partial-spectral framework explicitly conditions on the dominant low-frequency confounder (the PDO) rather than treating the ENSO–LAI relationship as a simple bivariate problem.

Nonetheless, we acknowledge the limitations of our methods, which only looks at linear relationships, while excluding the potential impact of non-linear effects or interactions. To test the directionality and mechanisms suggested, future work could focus on targeted sensitivity experiments. For instance, soil moisture could be prescribed to its climatological seasonal cycle to test whether the spectral reddening collapses when the soil moisture memory reservoir is removed. However, such perturbation experiments might lead to unintended feedbacks as seen in other studies (Knutti and Rugenstein, 2015; Lewis et al., 2024).

Knutti, R. and Rugenstein, M. A. A.: Feedbacks, Climate Sensitivity and the Limits of Linear Models, Philos. T. R. Soc. A, 373, 20150146, https://doi.org/10.1098/rsta.2015.0146, 2015.

Pearl, J., Glymour, M., and Jewell, N. P.: Causal Inference in Statistics: A Primer, John Wiley & Sons, ISBN 978-1-119-18686-1, 2016.

Lewis, N. T., England, M. R., Screen, J. A., Geen, R., Mudhar, R., Seviour, W. J. M., and Thomson, S. I.: Assessing the Spurious Impacts of Ice-Constraining Methods on the Climate Response to Sea Ice Loss Using an Idealized Aquaplanet GCM, J. Climate, https://doi.org/10.1175/JCLI-D-24-0153.1, 2024.

2. 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.

We thank the reviewer for highlighting this potential point of confusion. We agree that the design of the methodology deserves a more explicit discussion, and we have updated Section 2.1 to clarify this. The reviewer is correct that the construction of TLAI is intentionally set up to maximally capture any vegetation variability that co-varies with ENSO across the full temporal record. In that sense, the index is designed to find a spatial pattern that yields high overall coherence. However, this is an explicit goal of the methodology rather than a limitation since if there is any vegetation variability coupled to ENSO, this projection is optimized to capture it. This is analogous to other standard techniques used to isolate co-varying fields in climate science, such as Maximum Covariance Analysis (MCA) applied to sea surface temperature variability and a field of interest.

Crucially, however, while the projection method maximizes the overall correlation, it does not impose any a priori constraints on the frequency dependence, spectral reddening, or spectral gain of the resulting time series. Because the spatial regression map r(**x**) (Eq. 1) is entirely constant in time, the temporal evolution and frequency content of TLAI are inherited strictly from the underlying vegetation anomalies, not from the construction of this vegetation index. Our finding of a clear spectral reddening and a multi-decadal gain significantly greater than 1 (Fig. 3d, e) is therefore not an artifact of index construction, but rather a demonstration that these are independent properties of the vegetation system itself.

We have added the following text to Section 2.2 to better explain this (L. 148-154): "This resulting time series represents the component of global vegetation variability linearly co-varying with the oceanic index across the full timeseries, similar to other standard pattern-extraction techniques like Maximum Covariance Analysis. Importantly, because the regression map $\mathbf{r}(\mathbf{x})$ is strictly constant in time, this methodology does not impose any explicit constraints or a priori assumptions on the temporal evolution or frequency profile of $T_V(t)$. The frequency content is inherited entirely from the underlying, time-dependent vegetation anomalies $\mathbf{v}_w(t, \mathbf{x})$. Consequently, frequency-domain features, like spectral reddening or a multi-decadal gain greater than 1, reflect the dynamical response of the terrestrial system rather than artefacts of the index construction."

3. 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.

We thank the reviewer for raising the question of inter-model agreement and the role of semi-arid regions in ENSO-vegetation coupling. We agree that model agreement on the sign of change is relatively weak over the Central Amazon (where anomalies approach zero) and the Congo basin (as previously mentioned). However, models robustly agree on the teleconnection sign over other regions with well-established ENSO teleconnections, and these span different biome types following the Köppen-Geiger climate classification: the northern and northeastern part of the Amazon (tropical), Southeast Asia and the Maritime Continent (tropical), India (monsoonal), Argentina (temperate), eastern United States (temperate), Australia (monsoonal to semi-arid), Iran (semi-arid) and western United States (semi-arid).

We have edited the text to refer to the northern and northeastern Amazon and the different biome regions more specifically as follows (L. 341-344): "The models agree on the strong teleconnection patterns over a range of different biome types, including the northern and northeastern Amazon, Argentina, India, Australia and the eastern United States. The notable exception is Central Africa (Fig. S1), where considerable inter-model spread likely reflects long-standing CMIP biases in central African climate (Akinsanola et al. 2026)."

Note that our new figure S2 also shows that in particular the tropical regions are the hotspots for coherence and gain at multi-decadal time periods, rather than semi-arid ones. While a detailed, cross-model assessment of biome-specific responses is beyond the scope of this study, we believe this regional breakdown helps to get a more detailed regional picture of the response.

<u>4. Observational uncertainties:</u> 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.

We have now added the following limitation statement to the discussion to better acknowledge the uncertainties in the LAI satellite data (L. 535-537): "However, satellite-derived benchmarks are themselves subject to uncertainties. For instance, LAI products based on spectral indices are sensitive to cloud cover and atmospheric conditions alongside impacts from orbital drift or sensor changes, which could potentially introduce low-frequency variability (Wolf et al. 2025, Frankenberg et al. 2021)."

<u>5. Inconsistency between piControl and historical/observational data:</u> 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.

We appreciate the reviewer's comment regarding the different setups of the piControl and historical simulations. We have updated Section 2.2 to clarify that the comparison with historical and observational data is intended as a spatial validation of the internal ENSO-vegetation coupling rather than a direct equivalence between the two simulation types. We now added (L. 160-162): "Additionally, it should be noted that this comparison focuses on the spatial consistency of the internal ENSO-vegetation coupling, acknowledging that the historical record also incorporates transient anthropogenic greenhouse gas and land use signals that are absent in the control simulations."

Regarding the concern about fixed land cover, we note that the models in our piControl ensemble employ dynamic vegetation that responds to climate variability. Furthermore, a comparison between models with fixed PFT distributions and those with dynamic PFT transitions (GFDL and UKESM) in piControl shows broadly consistent regression patterns and spectra (see e.g., Fig. S1), suggesting that PFT transitions are not the primary driver of the ENSO-LAI relationship at the timescales analyzed. We acknowledge that while transient forcings in the historical period (e.g., land-use change and CO_2 fertilization) could affect the magnitude of the response in specific regions undergoing transition, the core mechanisms of internal variability we examine remain robust across both setups.

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.

We thank the reviewer for mentioning this relevant work which we now included in the introduction (L. 26-29): "Beyond these direct structural effects, ENSO also modulates the interannual variability of vegetation resilience, thereby influencing not only the immediate vegetation response but how readily the biosphere returns to its baseline state following an ENSO event (Zhou et al. 2025)."

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.

For this study, the first 500 years of available monthly data were from the piControl simulations of each model were used. This procedure was chosen to ensure consistency across the multi-model ensemble, as 500 years represents the total available duration for several of the selected ESMs. We now specified this in L. 108-109. However, we would like to note that all modelling centres have discarded and not made available the substantial spin-up periods of the piControl simulations before the period made available via ESGF. Additionally, we would like to note that all terrestrial and oceanic variables were linearly detrended prior to our analysis to reduces the influence of any remaining long-term trend or model drift in the piControl simulations on the spectral results (L. 117-118).

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.

We thank the reviewer for pointing out this statistical error. We agree that because our maps assess agreement in both directions (identifying both positive and negative anomaly hotspots), a two-sided binomial test is the correct framework. For a two-sided binomial test with $n = 11$ and a null probability of $p = 0.5$, an agreement of 9 out of 11 models corresponds to a p-value of approximately 0.065. So, this meets the 10% significance level.

We have corrected the wording in Section 2.1 and any corresponding figure captions to explicitly state that the 9-out-of-11 model stippling represents a 90% confidence level under a two-sided binomial test.

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

We have corrected this line as follows (L. 156-157): “To validate the relationship between oceanic variability and vegetation dynamics found in the piControl simulations, we analyze historical CMIP6 simulations and satellite-based observations over the common historical period of 1982-2014

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

We now descript (L. 166-167): “The GLASS dataset, which is a product of the AVHRR multi-sensor data, was selected as it provides data for all three vegetation variables and temporal alignment with the CMIP6 historical period.”

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 (αβ) 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 ´ßüto exclude ENSO is (β αβ).

The β coefficient represents the effect of the PDO on LAI after removing the linear influence of ENSO, which is what we wanted to convey with this label in consistency with the notations used in Kolstad and O’Reilly (2024). However, to avoid ambiguity we have revised the figure caption to read “β (effect of PDO on LAI, excluding the linear influence of ENSO)”.

<u>Results</u>

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”.

The data has been detrended as mentioned in the method section (L. 171). We thank the reviewer for mentioning that terms like “greening” and “browning” might be misleading since they are usually used to describe long-term trends. We have now changed the wording to “positive / negative vegetation anomalies” (L. 326-328).

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.

Please see our answer to the General comment 3. We have added “northern and northeastern” before Amazon when mentioning the model agreement.

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.

We have now performed additional analysis and added a new supplementary figure (new Fig. S2) which shows the MEM gridcell-level coherence squared and gain between the Nino3.4 index and LAI in piControl, evaluated over an interannual (2-4 years) and decadal and longer (>10 years) period band. This figure shows that the coherence and gain of the ENSO-LAI signal is primarily concentrated in the tropics/subtropics, specifically Amazonia and Southeast Asia, with much weaker coherence over mid and high-latitude regions like Alaska and the Central USA.

We have edited the text as follows (L. 359-368): "To identify the geographical drivers of this global signature, we first conducted a gridcell-resolved analysis (Fig. S2), which confirms that the strongest coherence and reddening are concentrated in tropical hotspots like Amazonia and Southeast Asia. By resolving the signal at each pixel, we verify that the global T_{LAI} pattern reflects real local dynamics rather than an artifact of spatial cancellation between out-of-phase regional anomalies. These findings are supported by targeted regional spectral analysis of the major tropical rainforest regions (Amazon, Congo and Southeast Asia), which dominate global vegetation productivity and are strongly influenced by ENSO (Fig. S3). While the gains from this regional analysis are not directly comparable to the gain in the global T_{LAI} index in a quantitative sense, the qualitative behavior in the Amazon and Southeast Asia shows pronounced spectral reddening and significant decadal coherence, mirroring the global amplification pattern. In contrast, the Congo region shows no coherence on long timescales, which might be related to low model agreement in this region (Fig. 3a)."

Additionally, the uncertainty across models is quantified by the spread across individual model responses (thin black lines) in now Figure S3. We agree that the choice of fixed geographic regions while allowing spatial comparability can obscure some model-specific teleconnection patterns, which vary between ESMs. We are now mentioning this in the Figure caption of Figure S3.

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.

We mention in the method section 2.5.3 that we are also using the partial spectral analysis to look at mechanistic pathways by which ENSO variability can impact LAI, for instance via changes in radiation, soil moisture and net precipitation (L. 273-275). However, we would like to leave the introduction of the specific variables we are focusing on in the result section to ensure that readers will remember the meaning of the variables and can follow the results even without a close look at the method section.

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.

The variables mention in the mechanistic analysis were, like all other variables, deseasonalised and detrended to ensure consistency across the analysis. We are now mentioning this explicitly in L. 273-275: "Additionally, we use this partial spectral analysis to examine the mechanistic pathway by which ENSO variability can impact LAI, for instance via changes in radiation, soil moisture or net precipitation. As before, these variables were deseasonalised and linearly detrended."

We agree with the reviewer that after removing the climatological seasonal cycle, the remaining RSDS anomalies largely reflect changes in light availability. This is particularly relevant in energy-limited regions, where changes in light availability can offset or amplify the impacts of soil moisture stress on LAI. We edited Section 3.2 (L. 400-401) as follows "RSDS reflects fluctuations in surface light availability, acting as the primary energy-related driver for photosynthesis and vegetation growth." Note that without specifically checking the cloud and clear-sky contributions to RSDS, we believe it is more accurate to avoid specifically mentioning clouds in this sentence.

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 (m^3 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.

We have now added a specific statement of the units, RSDS in Wm^{-2} and MRSO and MRSOS both in kg m^{-2} in the figure caption of Fig. 4. MRSO and MRSOS are both in the same units expressing vertically integrated total water content.

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

We agree with the reviewer that the tight coupling between radiation, precipitation, and soil moisture makes it challenging to isolate unique causal pathways. We have added a statement to the Discussion (Section 4) acknowledging that these drivers are dynamically linked and that our partial spectral analysis identifies the 'dominant' linear pathway rather than a completely independent physical process (L. 527-530). We are also now explicitly mentioning the inter-model spread in Section 3.3 (L. 411).

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.

We would like to clarify that our spectral and mediation analyses are based on the global T_V indices. This is not a simple spatial average but instead a projection onto the regression map, which effectively weights the index toward regions where the ENSO signal is strongest and most consistent. Consequently, our global results are dominated by high-agreement regions ensuring that the inferred mechanisms are robust despite local-scale heterogeneity in some regions impacted less strongly by ENSO. Also note that we have added a new Figure S2 which enables a spatial assessment of the impacts.

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.

We agree that the synchronization between GPP and R_A might reflect the structural coupling of carbon fluxes within the models. We have updated Section 3.4 to clarify that this buffering may be a model feature and suggest that further research is needed to verify if this behaviour holds in real-world biological processes (L. 444-445). Nevertheless, this finding remains important as it identifies a mechanism where gross productivity memory could be "filtered" out of the net carbon flux.

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.
We thank the reviewer for these specific comments. Regarding the first comment, our analysis is relying on CMIP6 models but the comparison with observations helps to give a first estimation of the accuracy of CMIP6 models in capturing decadal variability. We are mentioning in Section 2.3 that the observational data is shorter than the piControl simulations which hinders an analysis of variability longer than 30 years (L. 159-160).

- 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.
We fully agree with the reviewer that satellite data themselves are subject to uncertainties as already mentioned in the discussion section (L. 535-537). We have extended this sentence mentioning that errors can also arise from orbital drift or sensor changes.

- 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.
Please see our answer in response to the General comment 5.

- 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.
Please see our answer in response to the General comment 2.

- 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.
We fully agree with the reviewer and had already mentioned this limitation in L. 531-532 as follows: "Thirdly, both the mediation analysis (MacKinnon et al. 2000) and the partial spectral analysis are inherently restricted to capturing linear relationships, thereby excluding potentially significant non-linear effects and interactions.