Response to Editor and Referees Manuscript

Title: Explaining monthly precipitation anomalies in northwestern South America by integrating vertical dynamics and energetics
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Part I: Responses to the editor

(1) General comments from editor: This manuscript explores the relative contributions of thermodynamics vs. dynamics to precipitation anomalies in northwestern South America using the buoyancy work rate. This diagnostic allows the author to bridge the gap between themodynamic and dynamic drivers in modulated precipitation anomalies over the region. A machine algorithm is applied to validate previously established methodologies. The author is able to show that the dynamic (circulation patterns) act to modulate precipitation anomalies, even when the thermodynamic profiles across events/years might be quite similar. Overall, this manuscript presents a robust analysis for a region that is incredibly sensitive to large-scale atmospheric and oceanic dynamics. I recommend this manuscript for publication after some minor revisions.

(2) Author's response: Dear Editor, we deeply appreciate your recommendation for publication. We have carefully considered and addressed each of your insightful minor revisions, which have undoubtedly helped to tighten the manuscript, eliminate redundancies, and improve its overall clarity and impact. A detailed, point-by-point response to all your specific comments—along with the corresponding modifications—is provided below.

(3) Author's changes in manuscript: Please refer to the specific point-by-point responses below for the detailed changes implemented throughout the revised manuscript.

Point 1

(1) Comment from editor: L329: By this point in the text, I found myself convinced of the main argument and thinking that manuscript might be stronger if it was streamlined and tightened/shortened a bit. I don't have a great recommendation onto how or what should be cut, but I do think that some of the analysis can be cut.

(2) Author's response: We sincerely thank the editor for this highly constructive feedback. It is very encouraging to know that the main physical argument is convincingly established by this point in the text. We completely agree that streamlining the subsequent analysis improves the overall impact of the manuscript. Following your advice, we critically reviewed the "Validation of extremes and signal persistence" section. We identified that the detailed analysis of the upper tail dependence of individual, isolated variables (such as mid-level vertical velocity, local evaporation, and SST)—which originally included extensive discussions and citations regarding energy balance mechanisms—was a digression that distracted from our

primary focus on the BWR index. Consequently, we have substantially cut and condensed this section. We removed the paragraphs discussing the empirical copulas of these isolated variables and tightened the section into two highly concise paragraphs that focus strictly on demonstrating the asymptotic tail dependence and the signal persistence of the BWR index itself. This significantly streamlines the narrative without losing the core validation of our proposed index.

(3) Author's changes in manuscript: The Section 4.4 has been drastically shortened. The detailed discussions regarding the upper tail dependence of individual variables (isolated ω500 , evaporation, and SST) have been entirely removed. The section has been condensed into two paragraphs focused exclusively on the BWR's performance in extremes and its sub-seasonal signal persistence.

Point 2

(1) Comment from editor: L7: PCMCI+ should be defined in the abstract or just say you use a causal discovery algorithm and then introduce it later as you do.
(2) Author's response: We sincerely thank you for your insightful comment. We have chosen to just say you use a causal discovery algorithm and then introduce it later.
(3) Author's changes in manuscript: We have replaced "PCMCI+" with "causal discovery algorithm" in the abstract, to introduce it in the Introduction and Section 3.3.1.

Point 3

(1) Comment from editor: L82: Why not use observed SST?

(2) Author's response: We sincerely thank tou for this comment, which highlights the need to clarify our dataset selection. It is important to emphasize that the SST field in ERA5 is not a model prediction, but rather a prescribed boundary condition derived directly from high-quality observational products (such as HadISST2 and OSTIA, as documented by Hersbach et al., 2020). We specifically used the ERA5 SST field instead of an independent observed SST dataset to guarantee strict physical and thermodynamic consistency. Because our proposed BWR index is constructed entirely from ERA5 atmospheric variables (ω and ΔT), it is physically necessary to evaluate this atmospheric response against the exact same SST boundary field that the IFS model 'saw' and was forced by during the assimilation process. Using an external SST product would introduce artificial spatial and temporal mismatches between the surface forcing and the atmospheric response. Following your observation, we have clarified this methodological requirement in the manuscript.

(3) Author's changes in manuscript: We have added the following clarification to Section 2.1: "The SST data from the ERA5 reanalysis—which strictly prescribes observation-based products such as HadISST2 and OSTIA \citep{Hersbach_2020}—were used to characterize the oceanic boundary conditions. The use of ERA5 SST, rather than an independent observational product, ensures strict thermodynamic consistency, as it represents the exact surface forcing that generated the atmospheric response (ω and ΔT) evaluated in this study."

Point 4

(1) Comment from editor: L85-86: It might be nice to add some shading or contours to highlight the bounds of the Niño1+2 region in Fig. A3.

(2) Author's response: We sincerely thank you for this suggestion to improve the spatial context of our study. We agreed that the domains needed better visualization, so we have significantly upgraded this figure and moved it from the Appendices to the main text (Section 2.2). To prevent any spatial and conceptual confusion between the standard oceanic Niño 1+2 region (0°–10°S, 90°–80°W) and our specific atmospheric analysis domain (81.25°W–78.5°W, 8°S–0.75°N), we have renamed the latter simply to "coastal subdomain". Furthermore, because our updated figure now provides a high-resolution nested zoom specifically focused on this smaller coastal atmospheric domain to highlight the steep Andean topography, plotting the full Niño 1+2 bounds is geometrically impractical, as its western boundary (90°W) falls far outside the zoomed-in panel. We believe that elevating this figure to the main text and incorporating the complex orography effectively provides the robust spatial context you requested.

(3) Author's changes in manuscript: We have upgraded the former Figure A3, incorporated the complex Andean topography, and moved it to Section 2.2 as a primary figure. The new figure explicitly shows the two nested spatial domains (the broader NWSA and the zoomed-in coastal subdomain). Additionally, we have updated the text in Section 2.2 to consistently use the precise term "coastal subdomain" instead of referencing the Niño 1+2 area.

Point 5

(1) Comment from editor: Figure 5: It is hard to read the legends, might consider moving the legends outside of the time series panel so they could be stacked and have larger font?

(2) Author's response: We sincerely thank you for this practical suggestion to improve the readability of the figure. We completely agree that the previous layout was too constrained and difficult to read. Following your suggestions, we have moved the legend to the center of the top panel of the time series, arranged the elements in a more orderly fashion, and significantly increased the font size. This structural adjustment noticeably improves the clarity and overall visual impact of the figure.

(3) Author's changes in manuscript: Figure 5 has been updated. The legend have been moved to the center of the top panel of the time series, and their font size has been significantly increased to ensure optimal readability.

Point 6

(1) Comment from editor: Figure 5: Might consider some vertical shading to draw the readers attention to key time periods of interest.

(2) Author's response: We sincerely thank you for this excellent aesthetic and structural suggestion. Adding vertical shading is indeed a effective way to visually guide the reader through the narrative. We have updated Figure 5 by incorporating vertical shading to explicitly highlight the key periods of interest. This addition elegantly connects the time series data directly with our historical event analysis, making the figure much more intuitive for the reader.

(3) Author's changes in manuscript: Figure 5 has been further updated to include vertical shading that highlights the key time periods of extreme hydroclimatic anomalies, directly aligning the visual representation with the events discussed in the manuscript.

Point 7

(1) Comment from editor: L234-236: This information might be better suited in a table rather than just listed in text (or you could do both).

(2) Author's response: We sincerely thank you for this practical suggestion. In the original manuscript, organizing that extensive historical information into a table would have indeed been the best approach. However, following a major structural recommendation from Referee 2 (Comment 18), we have entirely removed that detailed literature review. Instead, we have condensed the information into a highly concise, three-line summary in Section 4.3 that simply pinpoints the specific extreme years and their oceanic drivers, supported by key citations. Because the extensive descriptive text no longer exists, creating a table to display what is now just a brief list of years would be redundant. We believe the newly condensed paragraph effectively provides the necessary context without requiring an additional table.

(3) Author's changes in manuscript: As requested by Referee 2, the detailed text in Section 4.3 has been drastically condensed into a brief summary paragraph that directly identifies the extreme events. Consequently, a table was not added to avoid redundancy.

Point 8

(1) Comment from editor: L238-239: More of a curiosity, but do you know what led to the rain if the expected SST relationship did not exist?

(2) Author's response: We sincerely thank you for this insightful question. To satisfy your curiosity from a physical standpoint: when local coastal SSTs are cool (often associated with La Niña-like conditions), precipitation in this region is typically not driven by local oceanic forcing, but by large-scale atmospheric teleconnections. Cool central Pacific SSTs cool the entire tropical free troposphere, which reduces tropospheric stability and facilitates convection. Simultaneously, this state induces easterly near-equatorial upper-air wind anomalies over South America that enhance moisture transport from the Amazon basin across the Andes. This mechanism forces dynamic lifting and convection over the region despite the unfavorable local coastal SSTs (as documented by Lavado-Casimiro and Espinoza, 2014; Takahashi and Martínez, 2017).
However, regarding the manuscript text, as noted in our response to your previous comment, the detailed description of historical events—which included the specific reference to the March 2022 anomaly—has been entirely removed from Section 4.3. This deletion was executed to comply with the strict conciseness requested by Referee 2 (Comment 18). Consequently, the sentence that prompted your curiosity no longer exists in the revised manuscript, rendering further textual clarification unnecessary.

(3) Author's changes in manuscript: No changes were made in response to this specific comment, as the referenced text regarding the March 2022 event was

previously removed from Section 4.3 to comply with Referee 2's structural recommendation.

Point 9

(1) Comment from editor: L254-255: I wonder if this finding fits in with the results from Huang et al. (2026)?

Huang, A. T., Kuo, Y.-N., & Lehner, F. (2026). Notable influence of internal variability on the perceived difference between Central and eastern Pacific El Niño teleconnections. Geophysical Research Letters, 53, e2025GL118422. https://doi.org/10.1029/2025GL118422

(2) Author's response: We sincerely thank you for bringing this recent and highly relevant study to our attention. You are right; the theoretical framework provided by Huang et al. (2026) fits here. The internal atmospheric variability they highlight elegantly explains why expected teleconnections can exhibit such notable deviations, exactly as observed in the "failed" coastal response during the 2016 strong global El Niño. While the detailed mechanistic description of the 2016 teleconnections was previously condensed to comply strictly with Referee 2’s request for brevity (Comment 18), we agree that citing Huang et al. (2026) adds value. To incorporate your suggestion while maintaining the strict conciseness requested by the referee, we have added a targeted concluding sentence to the paragraph, explicitly citing Huang et al. (2026) to frame this deviation as a manifestation of internal atmospheric variability.

(3) Author's changes in manuscript: We have added the following sentence at the end of the first paragraph in Section 4.3: "This deviation underscores the notable influence that internal atmospheric variability can exert in altering regional teleconnections during distinct El Niño flavors \citep{Huang_2026}".

Point 10

(1) Comment from editor: L261-262: SST is initially defined at L22, all other instances except maybe the one in the discussion, you can just use SST and there no need to re-define it (other examples are at L82, L143).

(2) Author's response: We sincerely thank you for catching this redundancy and for the meticulous review of the text. We have carefully reviewed the entire manuscript to ensure that the term "Sea Surface Temperature" is defined only once at its first appearance, and the acronym "SST" is strictly and consistently used thereafter. We have corrected the remaining redundant definitions you pointed out (e.g., at L82 and L143). Regarding the specific sentence at L261-262, as mentioned in our responses to your previous comments, that entire paragraph was already removed from the revised manuscript to comply with the strict condensation requested by Referee 2 (Comment 18). Therefore, that specific re-definition no longer exists, and all other active instances in the text have been properly corrected.

(3) Author's changes in manuscript: We have systematically removed all redundant definitions of "Sea Surface Temperature" throughout the manuscript, preserving only the initial definition. The specific text previously located at L261-262 was removed during the major condensation of Section 4.3 requested by Referee 2.

Point 11

(1) Comment from editor: L309-311: I wonder if it would be useful for the discussion here to overlay some metric of atmospheric circulation over Figure 12?

(2) Author's response: We sincerely thank you for this valuable suggestion. We completely agree that overlaying a circulation metric would be highly useful to complement the discussion. Specifically, superimposing the divergent component of the wind or mid-level wind streamlines (e.g., at 500 or 600 hPa) over the BWR anomalies would perfectly link the vertical motion captured by our index with the mesoscale horizontal forcing, providing a comprehensive 3D picture of the convective environment. Following your advice, we have gladly accepted this idea and added a brief sentence in the manuscript to highlight the utility of evaluating these specific circulation fields in conjunction with the BWR maps for future operational and diagnostic applications.

(3) Author's changes in manuscript: We have added the following sentence in the second paragraph of the Section 4.3.2: "For a more comprehensive diagnosis, it would be useful to overlay the divergent component of the wind or mid-level wind streamlines over the BWR anomaly maps, as this would explicitly visualize the 3D coupling between the vertical convective engine captured by the index and the mesoscale horizontal circulation".

Point 12

(1) Comment from editor: The author has satisfactorily addressed the reviewer comments, and I agree with the reviewers' assessment that the manuscript is now nearly ready for publication. I would only ask the author to perform the technical correction requested by reviewer 3 to the Fig. 6 caption, describing the vertical grey shading in the caption.

(2) Author's response: We sincerely thank you for this valuable suggestion. We agree.

(3) Author's changes in manuscript: We have added the following comment “The vertical gray shadings show the four most extreme events: 1983, 1998, 2017 and 2023” in the caption of the Fig. 6.

Part II: Responses to referee 1

(3) General comments from editor: This study focuses on hydroclimatic extremes over Northwestern South America (NWSA), particularly during El Niño events, and highlights the limitations of thermodynamic indices in explaining precipitation anomalies. To address this, the authors propose a new diagnostic metric, the Buoyancy Work Rate (BWR), which integrates thermodynamic instability and large-scale dynamically forced vertical motion. Using the PCMCI+ causal discovery framework, the study demonstrates that precipitation variability in the region is primarily controlled by large-scale dynamics, with vertical motion exerting a stronger causal influence than local thermodynamic processes. The BWR is further shown to outperform traditional indices in capturing extreme precipitation through tail dependence analysis, while also providing enhanced signal persistence. Overall, the study presents BWR as a physically grounded and potentially more reliable predictor

for sub-seasonal hydroclimatic extremes in NWSA. The topic is interesting, and the whole research is logical and well organized. Thus, this paper could be suitable for publication after addressing the following questions.

(4) Author's response: Dear Referee 1, We sincerely thank you for your positive feedback highlighting that our study is logical, well-organized, and presents the Buoyancy Work Rate (BWR) as a physically grounded predictor for sub-seasonal hydroclimatic extremes in NWSA. We are submitting this updated response to provide a complete and consolidated overview of the revisions made to our manuscript. The points below detail the exact and concise updates applied to the revised manuscript.

(4) Author's changes in manuscript: Please refer to the specific point-by-point responses below for the detailed changes implemented throughout the revised manuscript.

Point 1

(1) Comment from referee: Precipitation and ascending motion are closely coupled, each influencing the other in a two-way interaction. In particular, latent heat released during precipitation formation warms the surrounding atmosphere and promotes local upward motion. Conversely, ascending motion creates favorable dynamical conditions for moisture condensation and precipitation development. The authors should clarify whether the interaction between precipitation and upward motion may influence the applicability of BWR over the NWSA.

(2) Author's response: We sincerely thank you for your insightful comment. We wish to expand on the specific regional mechanisms that guarantee the applicability of the index over the coastal subdomain. While we agree that the two-way interaction between latent heat release and upward motion operates efficiently once convection is active, its initiation in the coast between Peru and Ecuador is strictly governed by the regional background state. The coastal subdomain is a dynamically limited environment characterized by persistent subsidence. In such heavily capped regimes, intense surface warming and thermodynamic instability (ΔT) are necessary but insufficient conditions to trigger deep convection. Localized instability cannot autonomously initiate the latent heat-ascent feedback loop because the atmospheric column remains mechanically suppressed.
Consequently, the bidirectional interaction you accurately described can only be activated when mesoscale vertical ascent (ω) provides the dynamic forcing required to overcome the background subsidence, lifting air parcels to their Level of Free Convection (LFC). In this regional context, mid-level ω acts as the mesoscale boundary condition that permits the convective engine to operate, triggering the convergence feedback. By mathematically abstracting this dynamic mechanism (coupling ΔT and ω), the BWR explicitly captures this two-way interaction. This formulation allows the index to accurately discriminate between actively coupled convective environments and those that possess high thermodynamic potential but remain dynamically suppressed.

(3) Author's changes in manuscript: We have explicitly recognized and detailed this two-way circular causality (convergence feedback) in the Section 5.1. The revised text now clearly states how the BWR index mathematically abstracts this exact dynamic mechanism to operate in the dynamically limited environment of the coastal subdomain.

Point 2

(1) Comment from referee: Since the PCMCI+ causal discovery algorithm is one of the core methodologies of this study, the current description of this method is rather vague, which may hinder the reader’s understanding of the subsequent results, particularly those presented in Fig. 6.

(2) Author's response: We sincerely thank you for highlighting this. We agree that a more comprehensive explanation of the PCMCI+ algorithm is necessary, given its central role in isolating the dynamic and thermodynamic drivers of precipitation in our study. We believe these additions provide the necessary context for readers to fully understand the robustness of the causal hierarchy presented in the results.

(3) Author's changes in manuscript: In the revised manuscript (Section 3.3.1), we significantly expanded the PCMCI+ methodology to ensure the framework and Figure 6 are fully transparent. We explicitly detail its two-step process: using a PC algorithm variant for condition-selection to filter spurious autocorrelations, followed by the Momentary Conditional Independence (MCI) test to estimate causal strength. Furthermore, we justify the critical inclusion of contemporaneous links (lag-0) to capture rapid atmospheric adjustments—such as the thermodynamic triggering of convection—that operate faster than our monthly sampling resolution. Finally, the updated Figure 6 caption now explicitly defines causal directions (arrows), MCI causal strength (node colors/labels), and link reliability derived from 3000 bootstrap samples.

Point 3

(1) Comment from referee: I wonder whether BWR is applicable beyond the NWSA, and whether its performance varies across regions with different dynamical and moisture regimes, such as the tropics, subtropics, and midlatitudes.

(2) Author's response: We sincerely thank you for your insightful comment. The application of the BWR beyond the dynamically limited NWSA is physically restricted by differing atmospheric regimes. Within the tropics, its diagnostic skill decreases over continental regions like the Amazon basin, where deep convection is primarily constrained by Convective Inhibition (CIN) that the index does not account for \citep{Fu_1999}, and over the eastern Andean slopes, where precipitation is driven by fine-scale mechanical orographic lifting unresolved by global reanalysis \citep{Trachte_2018, Insel_2009, Fernandez_2021}. Beyond the tropics, applying the BWR becomes theoretically inconsistent, as precipitation in the midlatitudes and subtropics is predominantly governed by baroclinic instability, frontal lifting, and Coriolis forcing rather than the tropical convective heat engine \citep{Sobel_2001}. Ultimately, the BWR is not a universal precipitation metric, but a targeted diagnostic proxy that excels specifically in environments where intense macro-scale dynamic forcing and boundary-layer instability strictly dominate the atmospheric response.

(3) Author's changes in manuscript: We have expanded the Section 5.3 to explicitly address the index's applicability beyond the NWSA across other tropical, subtropical, and midlatitude regions.

Point 4

(1) Comment from referee: Line 80: ERA5 exhibits strong correlations with both RAIN4PE and PISCO in the coastal region between Peru and Ecuador, whereas the correlations weaken in other regions. What accounts for this spatial discrepancy?

(2) Author's response: We sincerely thank you for your insightful comment. This spatial discrepancy arises from both the physical resolution limits of global reanalyses and the observational uncertainties inherent in gridded datasets. Along the coastal strip, the strong correlation occurs because precipitation is governed by large-scale synoptic and dynamic boundaries that ERA5 effectively resolves, coupled with a high density of meteorological stations that ensures the reliability of RAIN4PE and PISCO in this subregion. Conversely, the correlation weakens significantly over the Andes because ERA5's coarse spatial resolution fails to adequately capture fine-scale mechanical orographic lifting, systematically leading to precipitation overestimations compared to the gauge-adjusted regional products. Over the Amazon basin, the discrepancy is twofold: physically, precipitation is largely driven by localized Mesoscale Convective Systems (MCS) and intense moisture recycling that challenge ERA5's sub-grid convective parameterizations; observationally, the severe scarcity of rain gauges in this continental region sharply increases the spatial interpolation uncertainty in both PISCO and RAIN4PE.

(3) Author's changes in manuscript: We have expanded the Section 2.1 to explain the reasons that account for this spatial discrepancy in the revised manuscript.

Point 5

(1) Comment from referee: Line 170–175: Why do CAPE, BWR, and GDI exhibit strong positive correlations with precipitation along the coastal region, while these relationships weaken or even become negative over the Amazon?

(2) Author's response: We sincerely thank you for your insightful comment. This spatial divergence reflects fundamental physical differences between convective regimes. Along the dynamically limited coastal region, deep convection is a rare, binary process; precipitation strictly requires the synchronized activation of both surface instability and dynamic lifting, causing the monthly averages of all indices to perfectly align in phase with rainfall to yield strong positive correlations. Conversely, the Amazon basin is a thermodynamically abundant regime governed by Mesoscale Convective Systems (MCS) and intense local moisture recycling, where these indices encounter distinct physical limitations. CAPE collapses to negative correlations due to a thermodynamic phase-shift: maximum instability accumulates during the spring transition due to high Convective Inhibition (CIN), but is actively destroyed during the peak wet season by extensive cloud shading and convective consumption. While the GDI (via persistent mid-level moisture) and the BWR (via the large-scale ascent ω physically required to balance diabatic heating) survive this phase-shift to maintain positive correlations, their diagnostic skill slightly weakens over the continent. This occurs because their macro-scale variables cannot fully resolve the fine-scale orographic lifting and sub-grid MCS dynamics that ultimately dictate precipitation over the Amazon and the eastern Andes.

(3) Author's changes in manuscript: We have expanded the explanation of the strong correlations in the coastal and the weak correlations over the Amazon in the Section 4.1.

Point 6

(1) Comment from referee: Line 85: ‘(81.95° W–67.05° W, 18.95° S–1.95° N)’ instead of ‘(18.95° S–1.95° N, 81.95° W–67.05° W)’.
(2) Author's response: We sincerely thank you for catching this formatting detail. We agree completely.
(3) Author's changes in manuscript: We have corrected the coordinate order to follow the standard (Longitude, Latitude) convention in the revised manuscript.

Part III: Responses to referee 2

(5) General comments from editor: This manuscript explores the relative contributions of thermodynamics vs. dynamics to precipitation anomalies in northwestern South America using the buoyancy work rate. This diagnostic allows the author to bridge the gap between themodynamic and dynamic drivers in modulated precipitation anomalies over the region. A machine algorithm is applied to validate previously established methodologies. The author is able to show that the dynamic (circulation patterns) act to modulate precipitation anomalies, even when the thermodynamic profiles across events/years might be quite similar. Overall, this manuscript presents a robust analysis for a region that is incredibly sensitive to large-scale atmospheric and oceanic dynamics. I recommend this manuscript for publication after some minor revisions.

(6) Author's response: Dear Referee 2, we sincerely thank you for your feedback highlighting that our manuscript is clear and it provides interesting results, including the diagnostic potential of the BWR index. Below, we address each of your comments.

(5) Author's changes in manuscript: Please refer to the specific point-by-point responses below for the detailed changes implemented throughout the revised manuscript.

Point 1

(1) Comment from referee: In general, the BWR already contains the development of convection, via the ascending motion quantified with omega. This fact alone could explain the larger consistency (via correlations, PCMI and other indices) between BWR and precipitation, compared to other indices like CAPE, GDI, etc. In this sense, the BWR is an interesting proxy diagnostic (as the author clearly states it), that could be useful for simplified monitoring and diagnosis, e.g. for using in a similar way as a stream-function (not similar in mathematical nature, though).

From a physical point of view, the relationship between omega (and therefore BWR) and precipitation is more direct, since ascending motion is already an atmospheric response to more fundamental drivers, like convective instability and/or horizontal convergence. In this sense, the BWR index would not be on the same grounds as CAPE or GDI. While the latter attempt to quantify the potential for convection and precipitation (via precursor conditions), the BWR index already includes a measure of the atmospheric response (not potential), via omega.

Taking into account these considerations, some parts of the current text could be misleading, suggesting that BWR has predictive power, when physically it already contains the response, not only the drivers. I suggest to the author to make more clear the relationship between omega and the atmospheric response, e.g. with ascent already being part of the atmospheric response, similar to precipitation.

(2) Author's response: We sincerely thank you for this profound and constructive physical insight. We completely agree with your assessment. You have correctly pointed out a fundamental physical asymmetry in how the BWR operates compared to thermodynamic indices like CAPE or GDI. We acknowledge that CAPE and GDI are designed to quantify the potential for convection based on precursor thermodynamic conditions. In contrast, by incorporating the vertical velocity (ω), the BWR inherently includes the kinematic realization of convection. Because ascent and precipitation are tightly coupled and functionally part of the same atmospheric response, the BWR will naturally exhibit a stronger direct relationship with precipitation. However, your excellent point regarding the BWR measuring the "atmospheric response" rather than just precursor drivers motivated us to test whether this characteristic could actually be leveraged for seasonal prediction. To address the question of "predictive utility," we evaluated the BWR in a true prognostic framework. We computed the BWR using predicted variables from the ECMWF SEAS5 model (for the 2017–2025 operational period) and compared its predictive skill against the model's direct precipitation output. The new findings demonstrate that the predicted BWR systematically outperforms the model's native precipitation predictions. In general, climate models have more difficulty to predict the local response (via parameterization schemes) than the mesoscale response. Therefore, by applying the BWR to predicted fields, we bypass the model's precipitation schemes and instead predict the mesoscale atmospheric response. In this specific prognostic context, the BWR does possess predictive skill. We appreciate your comment, as it helped us to clarify that the BWR is a diagnostic of the realized state when used with reanalysis data, but also could be used as a predictive proxy when it is applied to climate model predictions.

(3) Author's changes in manuscript:

We have expanded the first paragraph of the Section 3.3.2 for explicitly addressing the physical distinction you highlighted. We now clearly state that while CAPE and GDI measure convective potential, the BWR measures the realized mesoscale atmospheric response because omega captures the ascending motion coupled with precipitation.

We have introduced a new section (Section 4.5: Predictive skill of the predicted BWR anomalies) and an accompanying figure detailing the comparative predictive skill of the predicted BWR versus SEAS5 predicted precipitation anomalies.

We have refined our terminology throughout the manuscript. We now explicitly differentiate the index's applications: we describe it as a "diagnostic proxy" or "explanatory metric" when discussing its application to ERA5 reanalysis data, but we explicitly define its "predictive skill" or "prognostic utility" in the context of its application to climate models predicted fields (SEAS5), supported by the new empirical evidence in Section 4.5.

Point 2

(1) Comment from referee: The author says that the BWR could also be useful for early warning, which is associated with prediction. Maybe, filtering the smaller horizontal scales in omega (to make sure that only the larger scale environment is left) before computing BWR, could lead to a more transparent formulation of an index

that attempts to measure precursor conditions leading eventualy to precipitation anomalies. In the case of prediction, a filtered version of BWR could be an indicator of large-scale conditions favorable (or unfavorable) for precipitation anomalies. However, the BWR index in its current form is purely diagnostic. In this sense, one could think that predicted precipitation anomalies from models could be as useful as the BWR index from predicted fields. I suggest the author to clarify those parts of the text where he refers to the predictability capabilities of the BWR index.

(2) Author's response: We appreciate your critical assessment and physical reasoning regarding the diagnostic nature of ω at local scales. We agree that the BWR, as computed historically in this manuscript, serves primarily as a diagnostic proxy of the mesoscale convective environment. Following your suggestion, we have revised the text to remove overreaching claims of "early warning" based solely on historical reanalysis data, and we acknowledge that spatial filtering of ω is a valuable strategy for future refinement. However, your specific hypothesis—that predicted precipitation from models might be just as useful as the BWR index computed from predicted fields—motivated us to explicitly test the index's predictive capabilities. To address your concern, we computed the BWR using predicted variables from the ECMWF SEAS5 model for its operational period (2017–2025) and compared its predictive skill (Pearson correlation against PISCO observations) against the SEAS5 direct precipitation output. The new findings demonstrate that the predicted BWR anomalies systematically outperforms the model's predicted precipitation anomalies. Specifically, the analysis reveals two critical improvements:

- Mitigation of the May Predictability Barrier: For predictions initialized in May, the predicted BWR anomalies demonstrates a marked improvement in correlation ($\Delta R > 0.3$) at lead times of 5 and 6 months (targeting October and November) compared to the model's direct precipitation anomalies.
- Enhanced Skill for the Peak Rainy Season: The predicted BWR anomalies yields consistent improvements in predictability (ΔR approx 0.1 to 0.2) for the most critical hydro-climatic period (February, March, and April) when initialized in November and December.

In general, climate models resolve large-scale vertical dynamics and energetic variables with greater fidelity than local scales. By calculating the BWR from SEAS5 field predictions, we bypass the model's precipitation schemes and instead rely on the mesoscale. Therefore, we have adapted the terminology throughout the manuscript. we describe it as a "diagnostic proxy" or "explanatory metric" when discussing its application to ERA5 reanalysis data, but we explicitly define its "predictive capibility" or "early warning capibility" in the context of its application to climate models predicted fields (SEAS5), supported by the new empirical evidence in Section 4.5.

(3) Author's changes in manuscript:

We have added a new subsection to the Results (Section 4.5) and a new figure detailing the comparative predictive skill of the predicted BWR anomalies versus SEAS5 predicted precipitation anomalies. This section explicitly presents the empirical evidence showing how the BWR improves seasonal predictability, particularly overcoming the May predictability barrier and enhancing skill during the peak rainy season.

We have clarified the text regarding "early warning" to specify that the BWR achieves this predictive utility specifically when applied to the output fields of climate models (like SEAS5), rather than as a standalone observational metric.

In the second paragraph of the Section 5.3, we have formally introduced your excellent suggestion regarding the spatial filtering of the omega field. We state that future efforts to further optimize the BWR will explore spatial filters to completely isolate the large-scale precursor environment from any residual localized convective response.

Point 3

(1) Comment from referee: The PCMCI+ algorithm is used first to verify the hypothesis that: "the large scale dynamics [omega] exert a stronger control on precipitation than local surface fluxes [evaporation]". However, this analysis is not novel. Studies as old as the one by Cornejo-Garrido and Stone already demonstrated this type of result. In fact, in their study, they emphasize on the role of horizontal convergence. In general, the tremendous importance of horizontal convergence (at low- and/or mid-levels), moisture transport from remote sources, and anomalies in regional circulation structures (e.g. the regional branch of the Walker circulation), has been documented in many papers. I suggest the author to review such literature in orther to update or to provide a different focus to the component associated with surface vs. dynamic drivers.

(2) Author's response: We agree completely with you. The dominance of large-scale dynamics—specifically horizontal convergence and regional Walker circulation anomalies—over local surface fluxes in this region is a well-established physical reality, fundamentally documented by Cornejo-Garrido and Stone (1977) and extensively supported by subsequent literature. Consequently, we no longer use the PCMCI+ algorithm to 'test' or 'discover' this hypothesis, as treating a historically proven fact as a novel finding was a conceptual error in our original manuscript. Instead, directly following your recommendation in the Point 16, we now utilize this established literature as a priori knowledge to constrain the algorithm. We have implemented 'physical link assumptions' within the PCMCI+ framework to explicitly force the contemporaneous causal orientation from vertical dynamics (ω) to precipitation. By embedding this physical boundary condition into the algorithm, the focus of the causal analysis shifts entirely: it no longer attempts to prove that dynamics dominate, but rather evaluates how effectively the proposed BWR index integrates and preserves this established causal route compared to isolated variables.

(3) Author's changes in manuscript:

We will significantly expand the introduction to thoroughly document the historical and current literature regarding horizontal convergence, moisture transport, and the Walker circulation's role in the NWSA, explicitly citing Cornejo-Garrido and Stone (1977) and related works as the foundational physics of our region.

We have rewritten the last paragraph of the introduction, forth and fifth paragraph of the Section 3.3.1 and Section 4.2 to explicitly state that the PCMCI+ analysis is only used to empirically evaluate the preservation of the BWR causal route to precipitation, not as a novel physical discovery.

Point 4

(1) Comment from referee: (L42-43): Please describe briefly what these indices are.

(2) Author's response: We thank you for this suggestion. We agree that introducing these traditional indices early on helps establish the context of our study.
(3) Author's changes in manuscript: We have expanded the third paragraph of the Introduction defining what CAPE, GDI, and other mentioned indices physically represent.

Point 5

(1) Comment from referee: (L81): "being more strong" -> "being stronger"
(2) Author's response: We appreciate the correction.
(3) Author's changes in manuscript: The text has been corrected to "being stronger."

Point 6

(1) Comment from referee: L85-86: A figure with the domains should be included in the main text, in order to facilitate the reading.
(2) Author's response: This is a very helpful suggestion to improve readability. We agree that readers should immediately visualize the spatial boundaries of the dynamically limited (NWSA) and thermodynamically abundant (Amazon) regimes discussed throughout the paper.
(3) Author's changes in manuscript: We have updated and moved Figure A3 to Section 2.2 for explicitly showing the two domains: NWSA and coastal subdomain.

Point 7

(1) Comment from referee: L94: "window where dynamic" -> "window when dynamic"
(2) Author's response: Thank you for the correction.
(3) Author's changes in manuscript: The phrase has been updated to "window when dynamic."

Point 8

(1) Comment from referee: (L100-105): please provide a brief justification for the selection of the thresholds, e.g. do they correspond to special percentiles? or are they associated with specific impacts or relevant events (Fig. 7)? These thresholds should not be so "arbitrary".
(2) Author's response: We fully agree that the selection of thresholds must be physically and statistically justified. The thresholds used were the percentiles 5 and 95.
(3) Author's changes in manuscript: We have replaced the "arbitrary" term by the percentiles 5 and 95 in Section 3.1.

Point 9

(1) Comment from referee: (L106-109): It would be interesting to see some figures about this, e.g. as supplementary material.

(2) Author's response: We thank you for this suggestion and their interest in visualizing this spatial core. The latitudinal and longitudinal cross-sections illustrating the vertical motion field—which confirm the axes of maximum vertical ascent at 80°

W and 5° S—are actually already included in the main text of the manuscript as Figures 9 and 10. We realize, however, that because these figures are presented later in the Results section, mentioning the methodology at lines 106-109 without an immediate visual reference may have caused confusion. To resolve this and ensure a smoother reading experience, we have added an explicit forward reference to these figures directly in this paragraph.

(3) Author's changes in manuscript: We have updated the text in Section 3.1 to explicitly point the reader to the corresponding figures in the Results section. The revised sentences now read: "To determine the spatial core of these anomalies, the vertical motion field was analyzed through latitudinal and longitudinal cross-sections (see Figures 9 and 10 in Section 4). The longitude of 80° W and latitude of 5° S were identified, approximately, as the axes of maximum vertical ascent over the coastal subdomain."

Point 10

(1) Comment from referee: (L113-L114): The author says that, for precipitation, vertical velocity is its trigger, and buoyancy is its fuel. This statement could be misleading. Instead, buoyancy can lead to vertical flow, i.e. instability is a trigger for convection.

(2) Author's response: We appreciate this critical physical insight. You are correct that the "trigger/fuel" metaphor is an oversimplification that can be misleading. We agree profound instability (buoyancy) can indeed drive vertical acceleration on its own once convective inhibition is overcome. Our original intent was to emphasize that in the specifically "capped" environment of the coastal NWSA, where persistent subsidence acts as a barrier, vertical ascent, above the level of free convection (LFC), is required in order to maintain the deep convection. However, we agree that the metaphor lacks physical precision.

(3) Author's changes in manuscript: We have removed the "trigger/fuel" analogy in Section 3.2 and another sections of the manuscript.

Point 11

(1) Comment from referee: (L125-128): It would be instructive to briefly compare the definitions of BWR and CAPE.
(2) Author's response: This is an excellent suggestion. Comparing their mathematical and conceptual definitions directly helps the reader understand exactly why BWR outperforms CAPE at monthly timescales in this specific region.
(3) Author's changes in manuscript: We have inserted a brief comparison in the last paragraph of the Section 3.2. We explain that while CAPE integrates the temperature difference between a rising air parcel and its environment over the depth of the free convection layer (LFC), the BWR integrates the product of this temperature difference and the vertical velocity (ω). Thus, BWR explicitly penalizes environments where high instability exists but dynamic ascent, over the LFC, is absent.

Point 12

(1) Comment from referee: (L151-152): The author refers to El Niño-related precipitation anomalies as "extreme precipitation". In order to distinguish from

"meteorological extreme precipitation" (hours-days), it would be more clear to use the term "climate extremes", or "extreme hydroclimatic anomalies" (L233) instead.
(2) Author's response: We thank you for pointing out this important distinction in temporal scales. We completely agree that using "extreme precipitation" can inadvertently imply daily or hourly mesoscale weather events, whereas our study strictly analyzes monthly climatic signals.
(3) Author's changes in manuscript: We have systematically reviewed the entire manuscript and replaced "extreme precipitation" with "extreme hydroclimatic anomalies" or "climate extremes" wherever we refer to monthly or seasonal El Niño-driven accumulations.

Point 13

(1) Comment from referee: (L182-185). The RMSE is commonly used for two quantities representing the same variable, because in this case it provides a clear measure of the distance or "error" of one variable with respect to the other (one of them being considered as a reference or ground truth). However, in the case of different variables (e.g. BWR vs. precipitation) it is easier to interpret the correlation coefficient than the RMSE obtained from the standarized anomalies. The latter compare non-dimensional variables, which convey information about the timing and relative sizes of fluctuations (information already available in the correlation coefficient), but not about the magnitudes of the original variables (which in the case of BWR vs. precipitation are not comparable in magnitude). In this sense, talking about "bias correction" when comparing standired anomalies is not so informative.
(2) Author's response: We completely agree with your statistical reasoning. Applying RMSE to standardized anomalies of variables with entirely different physical dimensions essentially transforms the RMSE into a proxy for correlation, stripping it of its primary utility—measuring absolute magnitude errors. Consequently, framing this as "bias correction" is indeed conceptually inaccurate and adds no new physical insight beyond what the correlation and tail dependence analyses already provide.
(3) Author's changes in manuscript: We have removed the RMSE analysis on standardized anomalies (Figure A2 has been removed) and deleted all references to "bias correction" in this context. The manuscript now relies strictly on correlation coefficients to evaluate the relationship between the indices and precipitation, which inherently and correctly handle differing units and magnitudes.

Point 14

(1) Comment from referee: (L190): This line seems incomplete.
(2) Author's response: We thank you for catching this typographical error.
(3) Author's changes in manuscript: We have corrected the latex sentence in the revised manuscript.

Point 15

(1) Comment from referee: (L195-198): BWR, CAPE and ERA5 precipitation are derived from the ERA5 reanalysis. This explains why they are consistent among them. It would be better to compare to other precipitation estimates, like from the GPM-IMERG dataset, in order to better support the comments about the PISCO data set.

(2) Author's response: We thank you for this critical observation. We completely agree that the internal consistency between BWR, CAPE, and ERA5 precipitation fundamentally arises from their shared derivation within the IFS model architecture. Therefore, we have removed the comment about the quality of PISCO after 2017. Instead, following your recommendation not to rely solely on the internal consistency of ERA5, we consulted the official climatological reports from the National Meteorological Service of Peru (SENAMHI) and the Ministry of Agriculture and Livestock (MAG) and National Committee for the Regional Study of the El Niño (ERFEN) of Ecuador. Specifically, during April 2023, these two official institutions reported significant positive rainfall anomalies on the northern coast of Peru and on the southern coast of Ecuador. While during April 2024, these two official institutions reported significant negative rainfall anomalies on that region.

(3) Author's changes in manuscript: We have removed the comment about the quality of PISCO in the last paragraph of the Section 4.1, and we have expanded it to explicitly include the independent observational report from SENAMHI and INAMHI, which supports the positive and negative anomalies diagnosed by BWR in April 2023 and April 2024, respectively. New references have been added to the bibliography.

Point 16

(1) Comment from referee: (L200-232): In order to have a more direct comparison, it would be convenient to have a version of Fig. 6a with "physical link assumptions", so that a more direct comparison with Fig. 6b could be attempted. In addition, some variables could be modified before constructing Fig. 6, to facilitate the interpretation of the MCI values, e.g. having evaporation positive when directed towards the atmosphere (line 209), and vertical velocity positive when upwards.

(2) Author's response: We thank you for this excellent methodological suggestion. We followed your suggestion to implement 'physical link assumptions' in the Figure 6a to force the orientation of the contemporaneous $\omega \rightarrow$precipitation link. This provides a direct comparison between a causal network constrained by physical assumptions and the data-driven causal discovery (Figure 6b), where the BWR index absorbs the variances of omega and the buoyancy. This assumption ($\omega \rightarrow$precipitation) is physically justified by the climatological background state of the coastal NWSA. Unlike regions with free tropical convection, the NWSA has a dynamically limited regime, strictly capped by large-scale and regional persistent subsidence. Under these boundary conditions, local thermodynamic responses cannot autonomously sustain deep convection. Therefore, at the monthly timescale, regional omega in middle levels (such as we showed in Fig. 11) acts as the mesoscale boundary condition that dictates the environment, while continental precipitation acts as the strictly subordinated thermodynamic response.

(3) Author's changes in manuscript: We have updated the time series fed into the PCMCI+ algorithm by inverting the sign of vertical velocity ($-\omega$, so positive means ascent) and evaporation (so positive means upward flux). We have updated Figure 6a with "physical link assumptions" and we also updated Figure 6b replacing omega and buoyancy by the BWR index to avoid deterministic collinearity in Section 4.2.

Point 17

(1) Comment from referee: (L244). The author says that positive precipitation anomalies stimulated deep convection. But physically, it is the other way around. Please clarify.
(2) Author's response: We thank you for pointing out this conceptual typo. Deep convection is the physical driver that produces precipitation, not the reverse.
(3) Author's changes in manuscript: We have removed the Section 4.3.1, following your recommendation in Point 18. Therefore, that sentence no longer exists.

Point 18

(1) Comment from referee: (L242-269): Section 4.3.1 provides a context, mostly supported on previous studies. This section is not needed in the main text. Instead, the author can make a very brief reference (e.g. 2 or 3 lines) to the cited studies in order to pinpoint specific years, or move this subsection to the supplementary material.
(2) Author's response: We agree that this section disrupts the flow of the main results by acting as a literature review. Keeping the main text focused on our novel diagnostic findings is a much better structural approach.
(3) Author's changes in manuscript: We have replaced Section 4.3.1 with a concise, 3-line summary (within the Section 4.3), that simply pinpoints the extreme specific events to set up the subsequent analysis.

Point 19

(1) Comment from referee: (L270-283): Section 4.3.2. This section is not very deep, and in its current form does not provide additional insight. It would be useful to diagnose horizontal mid-level convergence, as apparent for years 1998 and 2017, for example.
(2) Author's response: We appreciate the suggestion to deepen the physical analysis. Integrating horizontal mid-level convergence aligns with the dominance of large-scale dynamics and provides the physical context for the extreme vertical velocities observed during these years.
(3) Author's changes in manuscript: We have renumbered Section 4.3.2 to Section 4.3.1, and we have significantly expanded it. We incorporated a brief diagnostic analysis of the mid-level horizontal convergence during the extreme events of 1998 and 2017, explicitly linking this convergence to the intense ascending motion ($-\omega$).

Point 20

(1) Comment from referee: (L285). Figure 11. The legends in the figures should include the name of the variable. I guess that the units were included in order to use a single horizontal numerical axis: this should be explained in the caption. In addition, the horizontal lines for the LCL, LFC and EL advertised in the caption are not included in the figures.
(2) Author's response: We thank you for identifying these visual omissions and formatting issues, which are essential for the proper interpretation of the figure.
(3) Author's changes in manuscript: We have updated Figure 11. The legends now explicitly state the variable names alongside their units. We have added a sentence to the caption explaining the use of a shared horizontal numerical axis. Further, we have removed the LCL, LFC, and EL advertised in the caption. Finally, we have moved this Figure 11 and its respective analysis to the Section 4.3.1 to improve reading flow.

Point 21

(1) Comment from referee: (L283-299): Section 4.3.3. In this section the author can make a more detailed analysis of the vertical levels on which each variable (omega, delta-T, omegadelta-T) exhibit more interesting features. For example: what are the typical vertical levels at which omegadelta-T is the largest?
(2) Author's response: We strongly agree with this suggestion. Because the BWR is a vertical integral, analyzing the vertical profile of the integrand ($-\omega\Delta T$) provides excellent physical insight into where the actual convective "work" is being done within the atmospheric column.
(3) Author's changes in manuscript: We have moved the content of Section 4.3.3 within the Section 4.3.1 to improve reading flow, and we have expanded it to include a detailed vertical analysis. We now describe the specific pressure levels where the product $-\omega\Delta T$ reaches its maximum and contrast this with the levels where pure thermodynamic instability (ΔT) peaks and other features.

Point 22

(1) Comment from referee: (L300-319): Section 4.3.4. This section shows the diagnostic nature of the BWR index (line 313). The BWR already contains the atmospheric response to regional circulation drivers (e.g. regional anomalies in the Walker circulation), via omega. The the atmospheric response could also be assessed in terms of precipitation anomalies directly. Please expand on how the BWR provides information that is additional to the precipitation anomalies, e.g. for the 2016 event.

(2) Author's response: We sincerely thank you for this critical observation and for specifically pointing to the 2016 event, which illustrates the added value of the BWR index over direct precipitation anomalies. We agree that precipitation is the ultimate observable output; however, relying solely on precipitation limits our diagnosis to an effect, completely blind to the underlying mechanism. The 2015-2016 El Niño was globally one of the strongest on record. Based on thermodynamic indices alone, extreme hydroclimatic anomalies over the coastal NWSA was warned. However, the extreme hydroclimatic anomalies failed to materialize. If we assess the atmospheric response in 2016 using only precipitation anomalies, the map simply shows a non-event (neutral/ close to the climatological conditions). It tells us what happened, but not why. In contrast, the BWR index explicitly diagnoses the physical mechanism behind this false alarm. The integrated BWR anomaly for 2016 remained low because it mathematically captures the lack of coupling between the local dynamic trigger (mid-level omega) and the required thermodynamic instability (Delta T) over the coastal region during that specific period. The BWR tells us that the regional "convective engine" was physically suppressed, preventing the conversion of available potential energy into kinetic energy (deep convection), despite the massive background global forcing.

(3) Author's changes in manuscript: We have renumbered Section 4.3.4 to Section 4.3.2, and we have expanded it to explicitly include this comparative analysis, using the 2016 "failed extreme" as a case study to demonstrate how the BWR physically explains hydroclimatic responses rather than merely quantifying their final output.

Point 23

(1) Comment from referee: (L354). The author talks about "predictive memory", but this sounds contradictory. Maybe a better term would be "predictive capability".

(2) Author's response: We sincerely thank you for pointing this out. We completely agree that "predictive memory" was a confusing and somewhat contradictory term. Following a general recommendation from the editor to streamline and shorten the manuscript, the entire section containing that phrase (Section 4.4) has been drastically condensed. However, we found your suggested term to be highly accurate and physically sound. Therefore, in the newly condensed version of this section, we have permanently removed "predictive memory" and explicitly incorporated your term, "predictive capability", to properly define the operational advantage of the BWR index.

(3) Author's changes in manuscript: The confusing term "predictive memory" has been entirely removed. Following the condensation of Section 4.4, we have integrated your suggestion into the concluding sentence of the section: "Thus, the BWR represents a physical trade-off: it sacrifices a marginal fraction of diagnostic precision at the extreme tail (compared to pure ω) to gain substantial signal stability and predictive capability, rendering it a highly practical tool for sub-seasonal to seasonal operative monitoring".