

Reply to the review of the manuscript “An energetic perspective on the impact of the Atlantic Multidecadal Variability on the West African Monsoon” by reviewer 2

General comment:

The paper is logically organized and well written. The description is like a textbook, and readers who encounter the West African Monsoon for the first time may learn the related dynamics very efficiently through this paper. However, I have multiple minor concerns, especially regarding the details related to the energetic constraint on the ITCZ over the Sahel. I'm happy to review it again after these concerns are addressed.

We thank the reviewer for the careful reading of the manuscript and the constructive comments and suggestions to improve it. In the following, we provide a point-by-point response to the concerns raised in their comments and the modifications we propose to address them.

Major concern: Energetic variable in Fig. 4g is too arbitrary

L231–241: Fig. 4f–h implies that NEI over the North Atlantic (over the box in Fig. 4c; 70°W–0°) causes the southward cross-equatorial energy transport anomaly over 10°W–10°E, leading to the northward ITCZ shift and hence the Sahel precipitation enhancement over the same longitude range. However, the selection of the North Atlantic box for averaging NEI looks too arbitrary. In particular, the longitude range used for averaging NEI and that used for calculating the ITCZ shift are quite different. What is the rationale for your selection of the NEI averaging box for Fig. 4g?

We thank the reviewer for this comment. The NEI averaging domain in Fig. 4c over the North Atlantic was chosen as a compromise between covering the region with strong NEI anomalies and maintaining a simple geometric definition (Eq-70°N, 70°W-0) that minimizes the inclusion of continental land masses. To ensure robustness of our results and to address the reviewer's concerns, we have tested several alternative definitions:

- We recalculated NEI within the original domain (Eq-70°N, 70°W-0) after masking all land points. This does not strongly modify our results (Fig R1a).
- We expanded the domain eastward (Eq-70°N, 70°W-10°E), while maintaining the ocean-only mask. This broader domain yields a very similar correlation (Fig R1b).

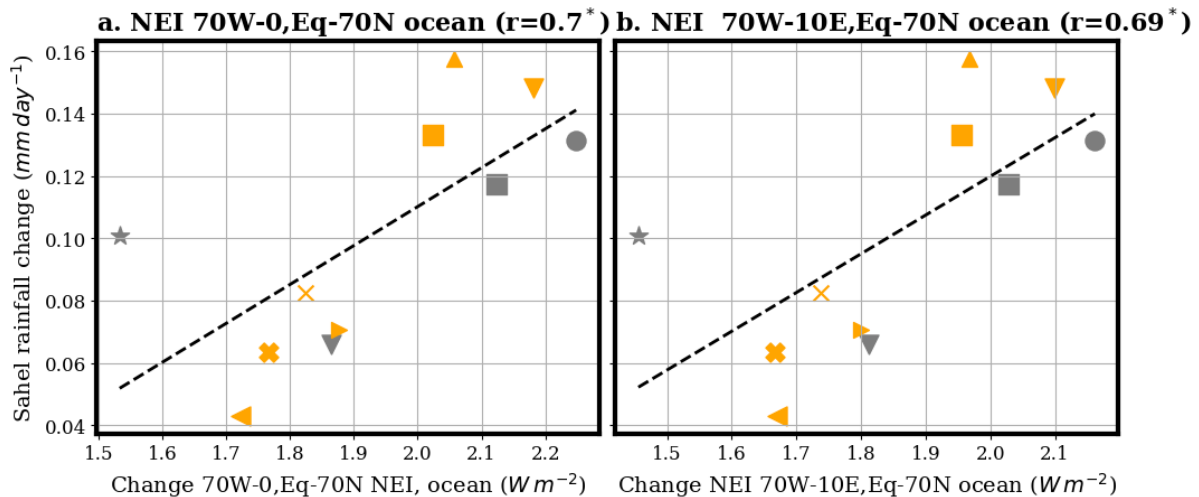


Figure R1: Scatter plots of averaged Sahel rainfall change (mm day^{-1}) and NEI changes (W m^{-2}) averaged over taking only ocean points in the longitude-latitude boxes of: a) $70^{\circ}\text{W}-0$, $\text{Eq}-70^{\circ}\text{N}$ (see orange box in plot 4c in the original manuscript); and b) $70^{\circ}\text{W}-10^{\circ}\text{E}$, $\text{Eq}-70^{\circ}\text{N}$.

To address the reviewer's concern, we propose to add a clarification at line 232 regarding the selection of the NEI domain.

In addition, it seems that the NEI maximum is over the tropics, not in the extratropics where the AMV forcing exists. There are also negative NEI anomalies over the tropical southeastern Atlantic. The reason for this tropical NEI gradient—favorable for the southward energy transport over Sahel longitudes—is not clearly explained. The authors partly attempt to address this by analyzing latent heat flux in Fig. 5, but to me, the wind speed increase in Fig. 5c is too narrow to explain the broad positive NEI over the northern tropical Atlantic.

Perhaps this tropical NEI pattern could be the emergence (or feedback) of the Atlantic ITCZ shift itself, as author speculated with Fig. 5c. Then, we may say that the ITCZ shift over the Sahel can be shaped by energetic imbalance caused by the oceanic ITCZ shift. But, all of these concerns can seem to be too specific. The author can improve the description for the flawless science, albeit current analysis still looks reasonable for the usual publication.

We agree with the reviewer that NEI anomalies in Fig. 4a are locally stronger over the tropical Atlantic than over the extratropics. However, both tropical and extratropical NEI are important and, notably combined, they explain more of the inter-model variance of Sahel rainfall ($r=0.70$ when considering the North Atlantic Ocean, Fig. R1) than either contribution does individually (Fig. R2; $r=0.48$ when considering only the tropical North Atlantic Ocean and $r=0.59$ when considering only the extratropical Atlantic Ocean).

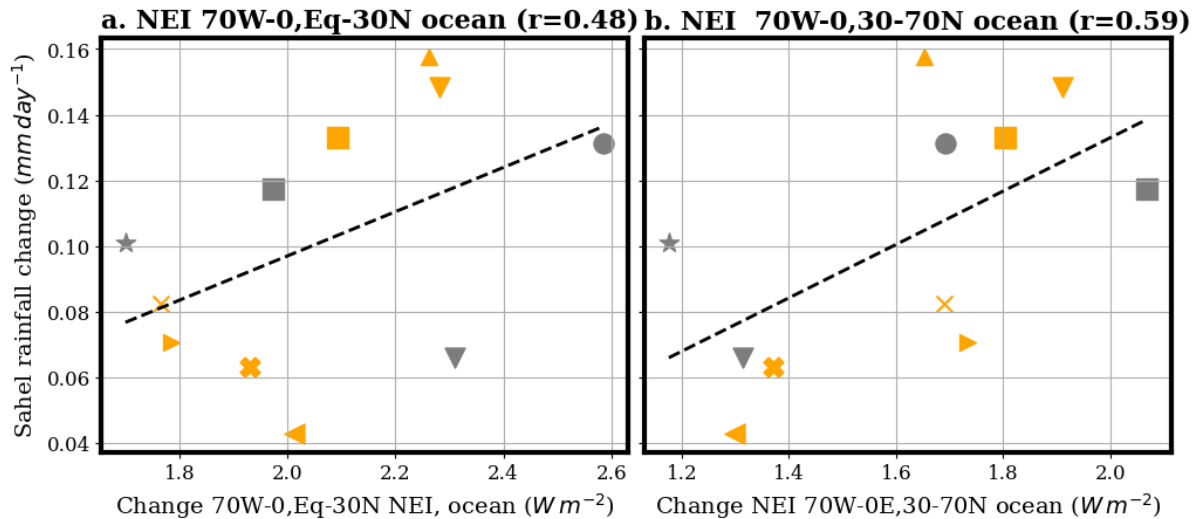


Fig. R2: Scatter plots of averaged Sahel rainfall change (mm day^{-1}) and: a) NEI averaged over the tropical North Atlantic (W m^{-2} , using the orange box in plot 4c from the equator to 30°N) and b) NEI averaged over the extratropical North Atlantic (W m^{-2} , using the orange box in plot 4c only north of 30°N). Only points over the ocean were used in the average calculation. This figure can be compared to the one provided in the reply to reviewer 1 (fig. R4 in that document, for which all points, including land ones, were used).

As for the NEI negative anomalies in the South Tropical Atlantic, approximately between the Equator and 10°S , these appear to be small compared with those in the North Atlantic and do not seem to be relevant to the intermodel spread of Sahel rainfall anomalies (Fig. R3).

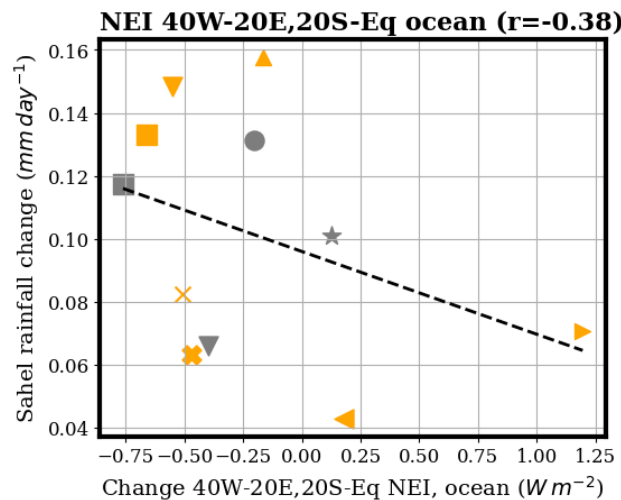


Fig. R3: Scatter plots of averaged Sahel rainfall change (mm day^{-1}) and NEI averaged over the South Tropical Atlantic (W m^{-2} , 40°W - 20°E from 20°S to Eq).

We also agree with the reviewer that the localized wind speed increases (Fig. 5c), which are largely restricted to the south of 10°N , do not account for the broad NEI anomalies in the tropical Atlantic. While latent heat anomalies account for most of the positive NEI across the North Atlantic (85%), they cannot explain the surface

energy imbalance where surface winds weaken in response to a positive AMV phase (e.g., 60°W-20°W and 10°N-20°N, see Fig. 5c in the manuscript). In this specific region, the positive surface energy anomaly is primarily due to cloud-radiative feedbacks linked to the northward shift of the oceanic ITCZ (Fig. R4). The increased cloud cover leads to a reduction in surface downwelling shortwave radiation (Fig. R4f), and a corresponding increase in reflected shortwave radiation at the TOA (Fig. R4k) over the approximate box 50°W-15°W and 5°N-15°N. This term, in turn, is mostly balanced by reduced OLR (Fig. R4l).

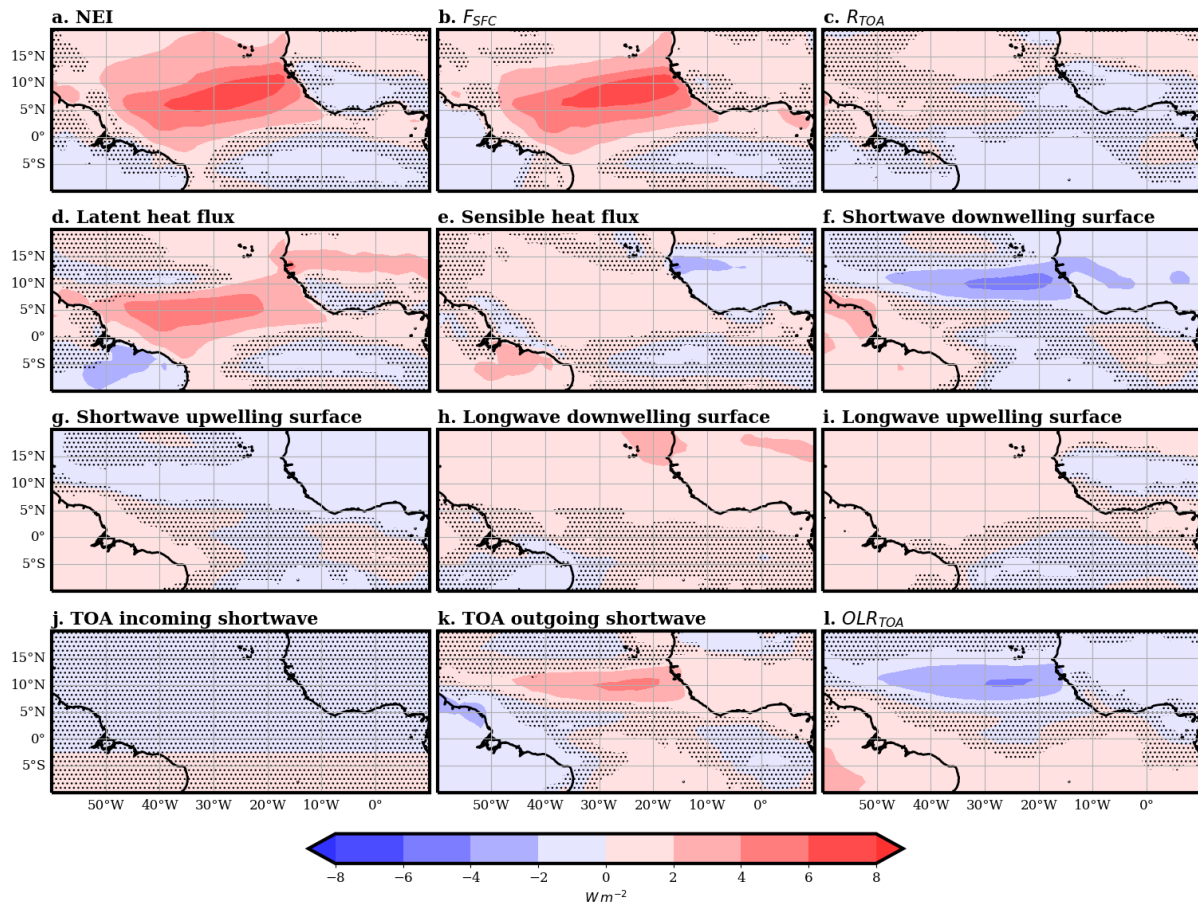


Fig. R4: Multimodel average changes in AMV+ minus AMV- in JAS for: a) net energy input (NEI) into the atmospheric column; b) energy imbalance at the surface (F_{SFC}); c) energy imbalance at TOA (R_{TOA}); d) latent heat flux; e) sensible heat flux; f) surface downwelling shortwave radiation; g) surface upwelling shortwave radiation; h) surface longwave downwelling radiation; i) surface longwave upwelling radiation; j) TOA incident shortwave radiation; k) TOA outgoing shortwave radiation; l) TOA outgoing longwave radiation. All units are Wm^{-2} . Positive values in panels f, h, k and l imply energy loss for the atmospheric column. Positive values in all other panels imply energy gain for the atmosphere. Dots mark regions where fewer than 80% of the models agree on the sign of changes.

To address the reviewer's concern, we propose to add the following:

- At the end of line 236 of the current manuscript, add a short explanation of the NEI changes in the approximate box 50°W-15°W and 5°N-15°N, emphasizing how they are not entirely explained by changes in latent heat fluxes, including figure R4 in the supplementary material as Fig. S5.
- At the end of line 241, add a comment on the relevance of both tropical and extratropical NEI contributions in the North Atlantic, and how their combination explains more of the inter-model variance of Sahel rainfall than either contribution separately. We would also include in the supplementary material a figure similar to Fig. R2 but taking into account NEI over land (Fig. R4 in the document with the response to reviewer 1), which would be Fig. S6.

Minor comment:

L56–57: Kang et al. (2008) is one of the original papers on the energetic constraints on the ITCZ. Please add it as a reference.

Kang, S. M., Held, I. M., Frierson, D. M. W., & Zhao, M. (2008). The Response of the ITCZ to Extratropical Thermal Forcing: Idealized Slab-Ocean Experiments with a GCM. *Journal of Climate*, 21(14), 3521–3532. <https://doi.org/10.1175/2007JCLI2146.1>

We thank the reviewer for this suggestion. We will add the reference in the suggested sentence.

L84–85: "Since this estimation assumes linearity, the changes associated with a negative AMV phase can be obtained by reversing the sign of the anomalies."

→Is this description referring to the forcing itself? The current sentence is difficult to understand.

We apologise for the confusing phrasing. The sentence was referring to the estimation of the changes associated with a positive phase of AMV: we estimate this impact by subtracting the negative experiment from the positive one ($AMV^+ - AMV^-$). There could be non-linearities in the impact of AMV, which we cannot account for without a control simulation. To clarify this, we will move this explanation up in the text, to line 82, so that the new paragraph will read: *"Changes associated with a positive AMV phase are estimated by subtracting the negative experiment from the positive one ($AMV^+ - AMV^-$). Since this estimation assumes linearity, the changes associated with a negative AMV phase can be obtained by reversing the sign of the anomalies."*

L111-112:"Assuming that energy storage is negligible over seasonal and long-term averages (denoted by overbars)"

→In Fig.4 of Donohoe et al. (2013), at the monthly timescale, there are non-negligible contributions from the storage term for the hemispheric

asymmetry of the atmospheric energy budget. Therefore, I do not think you can say "seasonal" here. Strictly speaking, you would need to demonstrate that the storage term does not matter for your analysis by calculating it and comparing it with the other terms. However, the strong correlation between the NEI proxy and rainfall in Fig. 4g may be sufficient for a first-order application.

Donohoe, A., Marshall, J., Ferreira, D., & Mcgee, D. (2013). The Relationship between ITCZ Location and Cross-Equatorial Atmospheric Heat Transport: From the Seasonal Cycle to the Last Glacial Maximum. *Journal of Climate*, 26(11), 3597–3618. <https://doi.org/10.1175/JCLI-D-12-00467.1>

We thank the reviewer for this remark and the reference. While we do not have the necessary output to verify that the energy storage is negligible over the JAS season in the analysed simulations, we highlight that the atmospheric storage tendency is about one order of magnitude smaller than NEI in ERA5 data (Fig. R5). In addition, there is no strong inter-hemispheric gradient in the storage tendency. This confirms a good correspondence between NEI and MSE flux divergence, hence justifying our neglect of the storage term.

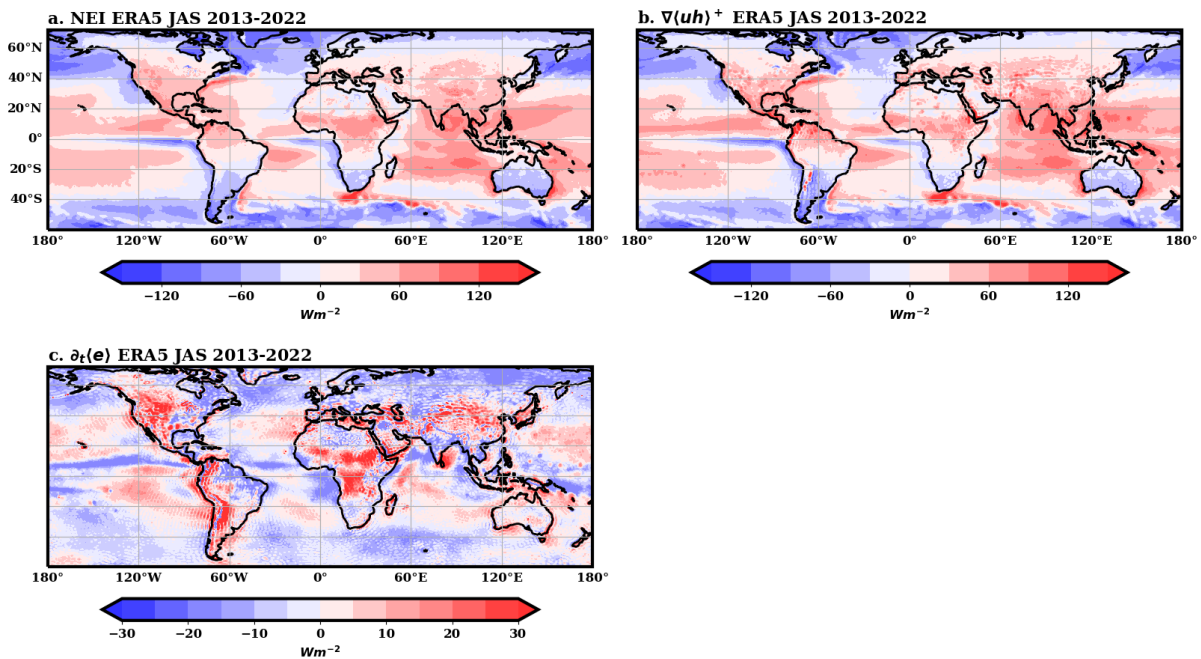


Fig. R5: a) Net energy input (NEI) calculated from ERA5 (Hersbach et al., 2020) energy imbalances at TOA and the surface averaged for the July to September season for the 10-year period of 2013-2022 (shaded, $W m^{-2}$); b) Mass-consistent divergence of the vertical integral of total energy flux (Mayer et al. 2021) averaged for the same season and period; c) time tendency of the mass-weighted vertical integral of moist enthalpy calculated as the residual between NEI and the mass-consistent divergence of the vertical integral of total energy flux averaged over the same season and period. Note the different colorscale.

To address the concern raised by the reviewer, we plan to add Fig. R5 as supplementary Fig. S13 and include a more thorough discussion of the limitations of

the energetic framework. In particular, the discussion regarding the assumption of negligible energy storage would be inserted after line 392 and would read along the following lines:

Some methodological limitations also need to be emphasized. First, our energetic framework neglects atmospheric energy storage, assuming that the divergent component of the vertically integrated energy flux is balanced by the mean NEI (Eq. 1), which might not be the case for seasonal means (Donohoe et al. 2013). While the limited available model output does not allow us to verify the validity of this assumption in our simulations, a comparison of these terms using ERA5 reanalysis (Hersbach et al., 2020; Mayer et al. 2021) suggests this is a robust approximation for a 10-year summer mean (see Fig. S13 in the supplementary material).

L222–224: "Such enhanced southward energy transport is typically realised through a northward displacement of the ascending branch of the Hadley circulation (Donohoe et al., 2014)."

→This is generally true when the gross moist stability (GMS; the efficiency of energy transport by the circulation) remains the same. How can you ensure that this assumption holds in this case as well? The strong correlation in Fig. 4g may indicate that variations in GMS do not play a major role here. However, I think it would be better to explicitly state this underlying assumption—namely, fixed GMS—somewhere in the text.

We thank the reviewer for this suggestion, which we will follow by modifying the sentence to explicitly state this assumption to: "*Provided gross moist stability does not change (Raymond et al. 2009), such enhanced southward energy transport is typically realised through a northward displacement of the ascending branch of the Hadley circulation (Donohoe et al., 2014).*"

L291-292: "These negative anomalies are linked to the surface cooling induced by increased soil moisture following increased precipitation."

→Would it be clearer to say "evaporative cooling" instead of "surface cooling"? The underlying physical process is somewhat obscured in the current wording.

We thank the reviewer for the suggestion. We plan to modify the text as suggested.

L315: In the energy budget equation, the MSE flux term in Eq. (1) is decomposed into a horizontal advection term and a vertical advection term. How is the vertical advection term derived? The continuity equation may

provide the explanation, but the reader would benefit from a more explicit description.

We thank the reviewer for this suggestion. Indeed, we need the continuity equation to reach the equation shown in line 314. To make this more explicit, we plan to expand this explanation in the following way:

“Taking time averages, again assuming negligible atmospheric energy storage ($\partial t \langle e \rangle \approx 0$) and decomposing the total transport in mean and eddy components, the energy budget can be written as:

$$\overline{NEI} - \langle \nabla_h(\bar{\mathbf{u}} \cdot \bar{h}) \rangle - \langle \nabla_h(\mathbf{u}'h') \rangle = 0 \quad (2)$$

where overbars denote time averages, and primes denote deviations from the time averages. Together with the continuity equation ($\partial_p \bar{\omega} + \nabla_h \bar{\mathbf{u}} = 0$, where ω is the vertical velocity in pressure coordinates), the second term in the left-hand side of equation (2) can be written as:

$$\langle \nabla_h(\bar{\mathbf{u}} \cdot \bar{h}) \rangle = \langle \bar{\mathbf{u}} \nabla_h \bar{h} \rangle - \langle \bar{h} \partial_p \bar{\omega} \rangle = \langle \bar{\mathbf{u}} \nabla_h \bar{h} \rangle + \langle \bar{\omega} \partial_p \bar{h} \rangle$$

This provides the following expression for the column-integrated energy balance:

$$\overline{NEI} - \langle \bar{\mathbf{u}} \cdot \nabla_h \bar{h} \rangle - \langle \bar{\omega} \partial_p \bar{h} \rangle - \langle \nabla_h(\mathbf{u}'h') \rangle = 0$$

L329–332: “Consequently, the vertical advection term ($-\langle \omega \partial_p h \rangle$) becomes more negative. Given the tropical MSE vertical structure, this implies a change from a shallower convection regime to a deeper convection regime with a first-baroclinic mode structure (Hill et al., 2017). This interpretation agrees with the horizontal divergence anomalies in Fig. 6c, which show reduced divergence at mid levels and stronger divergence aloft.”

→This is a beautiful explanation, but I think an explicit visualization related to the vertical advection term would be helpful. How about adding changes and climatology of ω , as well as changes and climatology of MSE (h), as supplementary vertical profiles similar to Fig. 6a–c? If shallower convection transitions to deeper convection, this should be identifiable through stronger upward motion in the mid-to-upper troposphere. The MSE profile may also help explain the details of the ω changes under the constraints imposed by NEI and the horizontal advection term. This point may be somewhat secondary, but it would strengthen the mechanistic explanation in this study.

We thank the reviewer for this suggestion. Fig. R6 shows the plot recommended by the Reviewer. As expected, the positive phase of AMV induces reduced ascent in the lower levels and enhanced ascent in the mid-to-upper troposphere, above approximately 675 hPa (Fig. R6a) in our reference box (10°W–10°E, 10–20°N). In

turn, moist static energy is enhanced throughout the troposphere, driven by changes in moisture in the lower-to-mid troposphere and by changes in temperature in the upper troposphere (Fig. R6b). The enhancement is stronger in the lower troposphere, which, together with the climatological ascent, provides a thermodynamic contribution towards an increased vertical import of moist static energy throughout the atmosphere (see term $\omega\delta(\partial_p h)$ in Fig. R6c). This import is counterbalanced by the dynamical contribution: the change from a shallower convection regime to a deeper convection regime acting on the climatological profile of moist static energy provides enhanced vertical export of moist static energy throughout the troposphere (see term $\delta(\omega)\partial_p h$ in Fig. R6c). These changes promote enhanced vertical export of moist static energy (a more negative vertical advection term, $-\delta(\omega)\partial_p h$) in the mid-to-upper troposphere, and a more modest vertical import of moist static energy in the lower levels.

We will include this figure in the supplementary material as Fig. S12 and update the text accordingly (line 331).

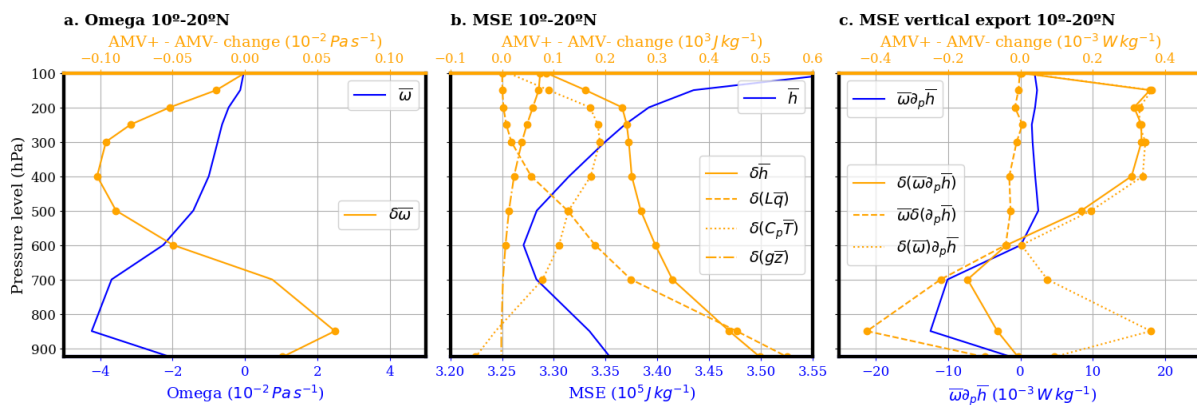


Fig. R6: Profiles of AMV⁺ minus AMV⁻ differences (orange, top axis) and climatological values (blue, bottom axis) in the Sahel box defined as 10°W-10°E, 10°N-20°N (see purple box in figure 2b of the manuscript) of: a) omega (ω , Pa s⁻¹); b) moist static energy (MSE, J kg⁻¹) and c) vertical export of moist static energy ($\omega\partial_p h$, W kg⁻¹). Changes in moist static energy in panel b are further partitioned into the potential energy (gz , dashed dot line), sensible ($c_p T$, dotted line) and latent enthalpy (Lq , dashed line). The vertical export of moist static energy in panel c is decomposed into its thermodynamic ($\omega\delta(\partial_p h)$, dashed line) and dynamic ($\delta(\omega)\partial_p h$, dotted line) components. Models EC-Earth3P-HR and EC-Earth3P in the PRIMAVERA protocol were not used to calculate the multi-model mean omega, as we could not find this variable for these models. In addition, model EC-Earth3 in the DCP-C protocol was also excluded from the vertical profile analysis because too few vertical levels were available in its outputs. Dots in the orange lines mark changes for which at least 80% of the models agree on the sign.

Figure 3 caption: "c and d)" -> "c) and d)"

We thank the reviewer for the correction. We will implement it as suggested in the new version of the manuscript.

References:

- Mayer, J., Mayer, M., Haimberger, L., (2021): Mass-consistent atmospheric energy and moisture budget monthly data from 1979 to present derived from ERA5 reanalysis. Copernicus Climate Change Service (C3S) Climate Data Store (CDS). DOI: [10.24381/cds.c2451f6b](https://doi.org/10.24381/cds.c2451f6b) (accessed 3-03-2026).
- Hersbach, H., Bell, B., Berrisford, P., Hirahara, S., Horányi, A., Muñoz-Sabater, J., et al. (2020). The ERA5 global reanalysis. *Quarterly journal of the royal meteorological society*, 146(730), 1999-2049. <https://doi.org/10.1002/qj.3803>
- Raymond, David J., et al. The mechanics of gross moist stability. *Journal of Advances in Modeling Earth Systems* 1.3 (2009). <https://doi.org/10.3894/JAMES.2009.1.9>