

## **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 1.**

### **General**

**The authors study the influence of Atlantic Multidecadal Variability (AMV) on the West African Monsoon (WAM) in a suit of simulations. The main findings are a an intensification of WAM in response to the positive phase of AMV, driven by a northward shift of the African ITCZ associated with energetic constraints on the position of the ITCZ, and a weakening and northward shift of the shallow meridional circulation over the Sahara, which supports the intensification of WAM by reduced dry air intrusions into the deep-convective region. The paper is well organized and well written, and the results are overall well supported. I do have a couple of concerns regarding the energetic constraints and regrading the attribution of the surface heat anomalies associated with AMV with the WAM response. I would therefore recommend accepting the paper after addressing these concerns. Additional minor comments are listed below by line number.**

We thank the reviewer for the careful reading and constructive comments on our manuscript. In the following, we provide a point-by-point response to the points raised in the comments and the modifications we propose to address them. In particular, to address the main concerns raised, we discuss the limitations of the energetic framework and of our study, and we further explore the link between anomalies of NEI in different regions (tropical Atlantic, extratropical Atlantic, equatorial West Africa) and anomalies of Sahel rainfall and analyse DCP-C AMV tropical and extratropical experiments, as proposed by the reviewer.

### **Comments**

**1. The energetic constraints are no more than a balance condition, and I was not convinced that the heat anomalies in the north Atlantic are a direct driver of the WAM response — especially considering that the regional energy balance is not closed (i.e., due to zonal energy fluxes). For example, moisture transport from the tropical Atlantic into the Sahel would induce a similar result, in which case, the northward shift of the ITCZ would yield a similar change to the energy transport fields, rather than be a a response to the imposed energetic imbalance. Given that the authors explain the northward shift of the WAM ITCZ as a direct response to the energetic imbalance, I would expect better support (e.g., see the comment below) for this claim and better description of the limitations of this hypothesis.**

We thank the reviewer for this insightful comment. We agree that the energetic framework adopted here primarily describes a balance constraint and does not, by itself, establish a unique causal pathway. We also acknowledge that processes not explicitly quantified in our analysis, such as zonal energy fluxes and atmospheric energy storage, may contribute to the regional energy budget. In addition, we recognize that, as proposed by the reviewer, a northward shift of Sahel precipitation could arise from changes in moisture transport from the tropical Atlantic.

While the analyses we can perform are constrained by the available model output (see below), we would like to emphasize the following points:

1. The experimental setup imposes SST anomalies over the North Atlantic (including both tropical and extratropical regions), which induce a regional energy perturbation. Part of this excess energy is locally radiated to space, but a substantial fraction is redistributed through changes in the atmospheric circulation. In this context, negative NEI anomalies developing in the South Atlantic act to enhance the interhemispheric energy contrast, as noted by the reviewer, but are best interpreted as part of the atmospheric adjustment to the imposed North Atlantic forcing, rather than as an independent driver of the response. The resulting anomalous moist static energy flux exhibits a southward component at Sahel longitudes (Fig. 4a), which is consistent with a northward displacement of the ascending branch of the monsoonal circulation and the associated precipitation. We interpret this adjustment as being physically consistent with and, in fact, a necessary response to the imposed energy perturbation. At the same time, we acknowledge that it involves coupled dynamical and thermodynamical processes and does not uniquely isolate a single causal mechanism.
2. We further acknowledge that the vertically integrated moist static energy fluxes shown in Fig. 4a do not allow us to disentangle the relative roles of different processes contributing to the anomalous energy transport. In particular, they do not distinguish between contributions from the mean circulation and transient eddies, nor between latent and sensible components, or between different vertical levels of the atmosphere. As a result, our analysis cannot rule out that enhanced moisture transport from the tropical Atlantic may play an important role in linking the imposed North Atlantic energy perturbation to the Sahel precipitation response. The presence of a westerly component in the anomalous energy flux over the Sahel is consistent with this interpretation (Fig.4a). A more detailed process-level attribution would require high-frequency, vertically resolved model output, which is not available for the simulations analyzed here.
3. While we do not have the necessary output to verify that in the analyzed simulations, on seasonal time scales, atmospheric storage tendency is about one order of magnitude smaller than NEI in ERA5 data (Fig. R1). 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.

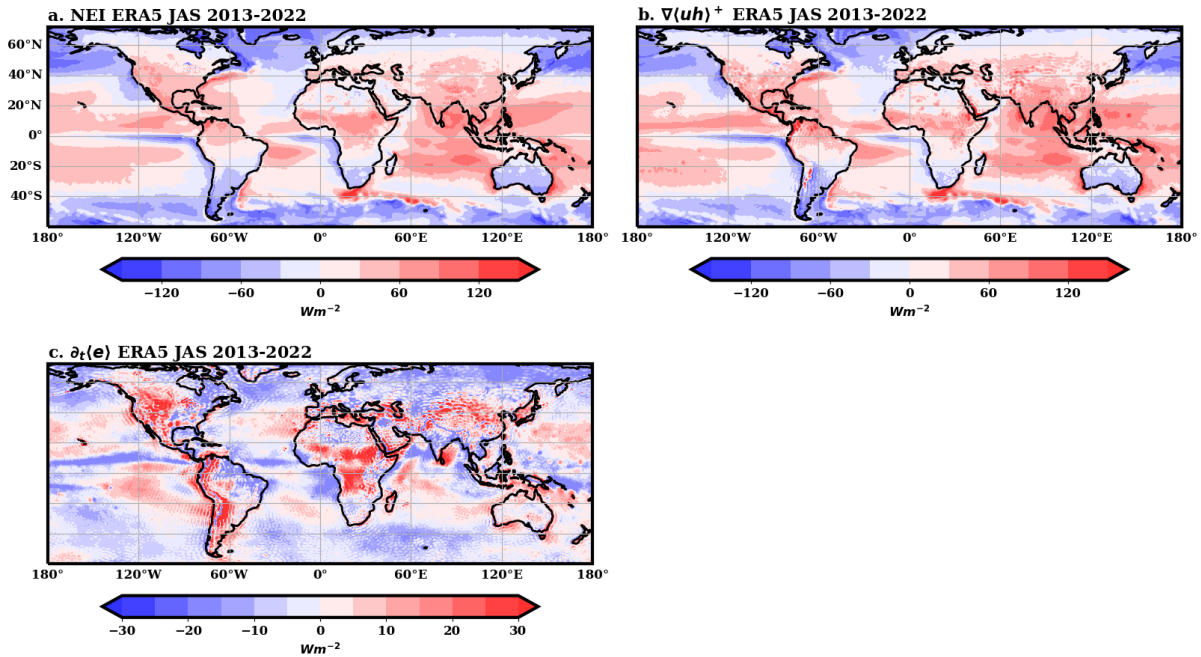


Fig. R1: a) Net energy input (NEI) calculated from ERA5 (Hersbach et al., 2020) energy imbalances at TOA and the surface averaged for the July to September season for the 10-year period of 2013-2022 (shaded,  $\text{W m}^{-2}$ ); b) Mass-consistent divergence of the vertical integral of total energy 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 directly address these concerns, we propose the following changes in our manuscript:

- a. Remove the word “direct” on Line 231, to acknowledge that the response may arise through an intermediate chain of processes. Also include a reference to the South Atlantic negative anomalies, along the following lines:

*These adjustments also include changes in NEI in regions remote from the primary forcing, such as the Atlantic south of the equator, where negative NEI anomalies develop (Fig. 4a) and further strengthen the inter-hemispheric energy gradient.*

- b. Include a more thorough discussion of the limitations of the energetic framework. The discussion 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). Second, the vertically integrated MSE fluxes (Fig. 4a) do not uniquely identify the specific chain of intermediate physical links driving the WAM adjustment to the AVM forcing. In particular, these integrated fluxes do not allow us to disentangle the relative contributions of the mean circulation and transient eddies, nor the specific roles of latent and sensible energy transport. Consequently, our results do not preclude, and are indeed consistent with, the hypothesis that enhanced moisture transport from the tropical Atlantic acts as a primary causal link between North Atlantic warming and the northward Sahelian ITCZ shift. The presence of a westerly component in the anomalous MSE flux over the Sahel (Fig. 4a) supports this interpretation. High-frequency and vertically resolved model output (not currently available for this ensemble) would be necessary to further attribute these changes to specific process-level dynamics.*

- c. This discussion would be complemented with the addition of Fig. R1 to the Supplementary material as Fig. S13.

**2. DCPD includes experiments where the tropical and extratropical components of AMV are separated (Boer et al. 2016). Examining the sensitivity of the response to this decomposition can be very helpful in attributing the WAM response to specific processes (e.g., tropical moisture transport vs. inter-hemispheric asymmetric heating). I would like for the authors to analyze these experiments or at the very least explain why were these not examined.**

We thank the reviewer for this suggestion. To address this, we analyzed the three models in our ensemble (IPSL-CM6A-LR, CNRM-CM6-1, and HadGEM3-GC31-MM) that performed these specific DCPD-C experiments (tropical vs extratropical AMV forcing). Our analysis led us to exclude these results from the final manuscript for the following reasons:

1. The Sahel rainfall response is highly inconsistent across these three models. For example, in CNRM-CM6-1, the response is driven by the tropical component, whereas in HadGEM3-GC31-MM, it is significant only in the extratropical experiment (see Figs. R2 and R3 in this reply).
2. Even when SST restoration is restricted to the extratropics (north of 30°N), positive SST anomalies are communicated to the tropical Atlantic via coupled air-sea feedbacks (e.g., Zhang et al., 2019). This precludes the clean attribution of the WAM response to specific latitudinal bands that the reviewer was suggesting.
3. Only 3 of our 11 models performed these experiments. Including results from such a small subset would undermine the robust multi-model consensus that is the focus of our study.

In addition, we note that in the full AMV experiments analysed in the manuscript, both contributions, tropical and extratropical NEI, are relevant, and their combination explains more inter-model variance of Sahel rainfall than any of them separately (Fig. R4).

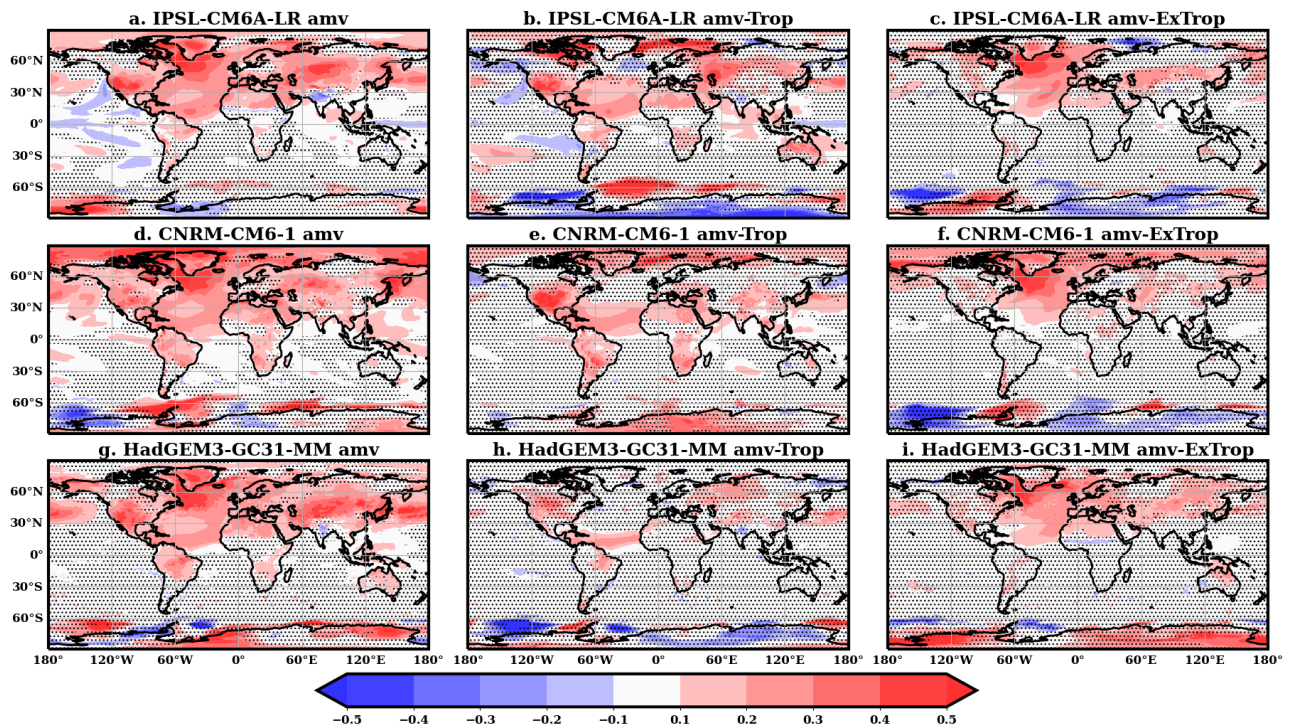


Fig R2: Multimodel mean AMV+ minus AMV- change in JAS surface temperature ( $^{\circ}$  C) for the simulations where the SSTs were restored in the whole North Atlantic (left column, plots a, d and g), only in its tropical region (central column, plots b, e and h) and only in its extratropical region (right column, plots c, f and i) for the IPSL-CM6A-LR (first row, plots a to c), CNRM-CM6-1 (second row, plots d to f) and HadGEM3-GCM31-MM (third row, plots g to i) models. Dots mark regions where anomalies are not statistically significant ( $p < 0.05$ ) according to the parametric t-test for differences in means, assuming a Gaussian distribution for the samples and independence of the different ensemble members.

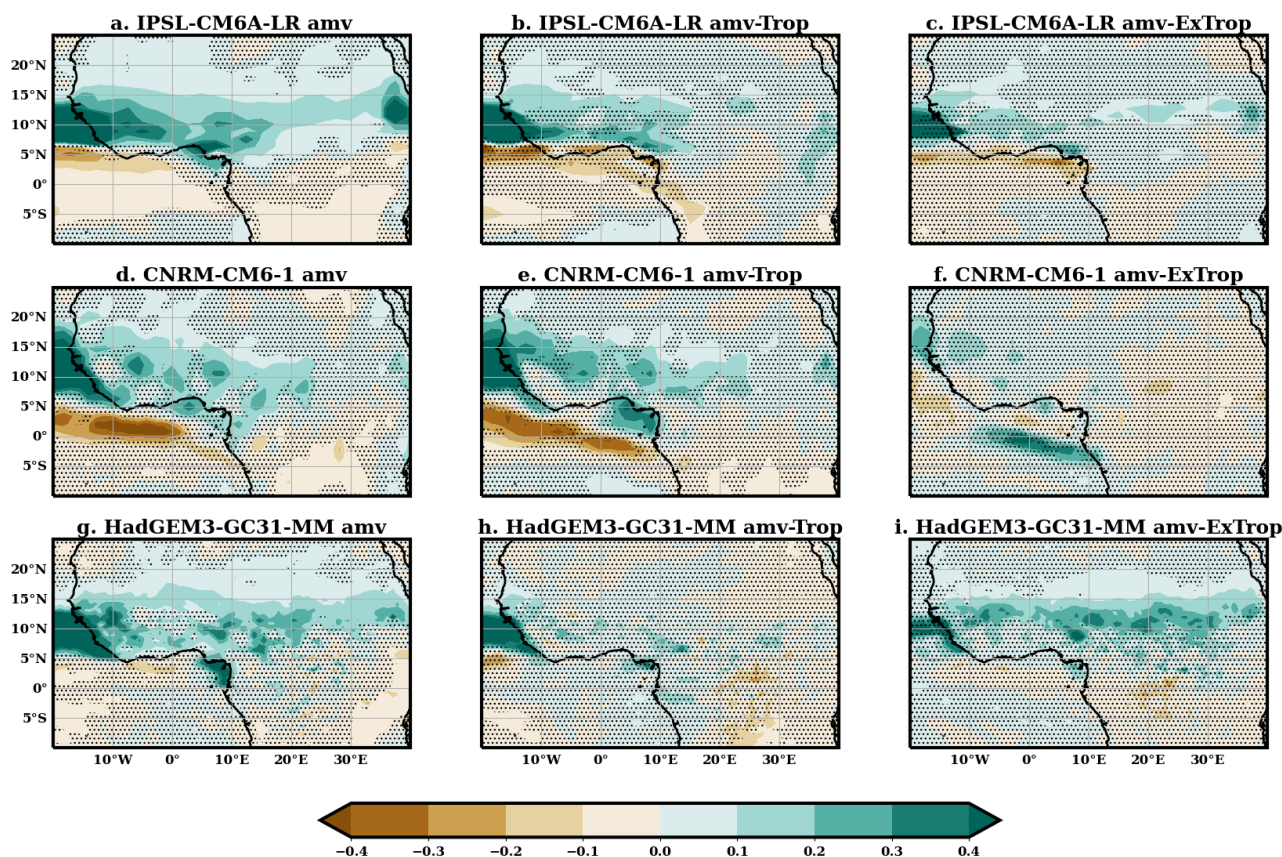


Fig. R3: as in Fig. R2 but for changes in JAS precipitation ( $\text{mm}\cdot\text{day}^{-1}$ ).

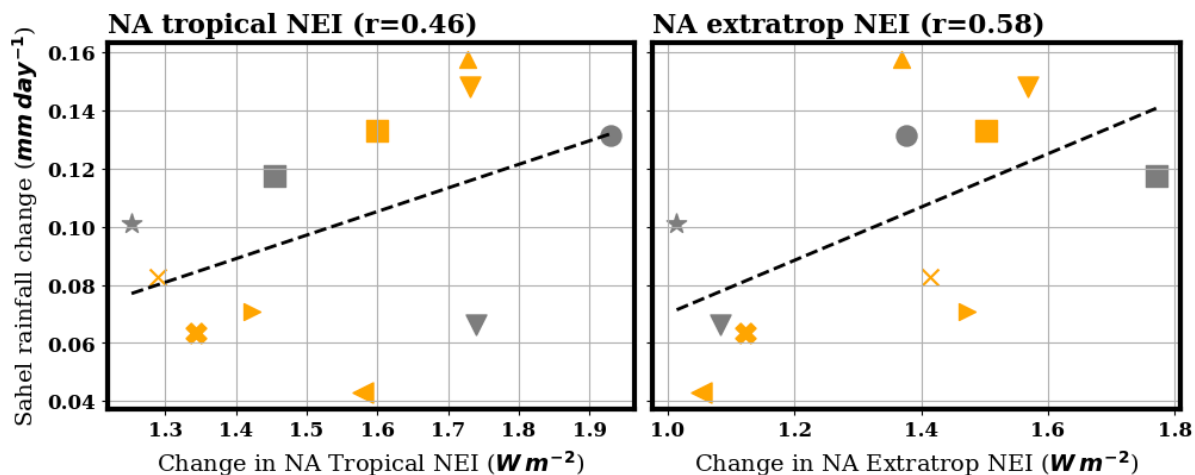


Fig. R4: Scatter plots of averaged Sahel rainfall change ( $\text{mm}\cdot\text{day}^{-1}$ ) and: a) NEI averaged over the tropical North Atlantic ( $\text{W}\cdot\text{m}^{-2}$ , using the orange box in plot 4c from the equator to  $30^\circ\text{N}$ ) and b) NEI averaged over the extratropical North Atlantic ( $\text{W}\cdot\text{m}^{-2}$ , using the orange box in plot 4c only north of  $30^\circ\text{N}$ ).

To address the reviewer's concern, we propose to 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 at the end of line 241. We would also include figure R4 in the supplementary material as Fig. S6.

## Line-by-line comments:

### Comments by line number

**74—76 It would be helpful to show the AMV pattern to which are you relaxing the model, and not have to refer to Boer et al. (2016).**

We agree that explicitly showing the restoration region is helpful. Figure R5 shows the AMV positive pattern to which SSTs were restored in the AMV+ simulations. We will include this figure in the supplementary material as Fig. S1 and update the text (Line 74) to point directly to this figure and clarify the region where the restoration is performed.

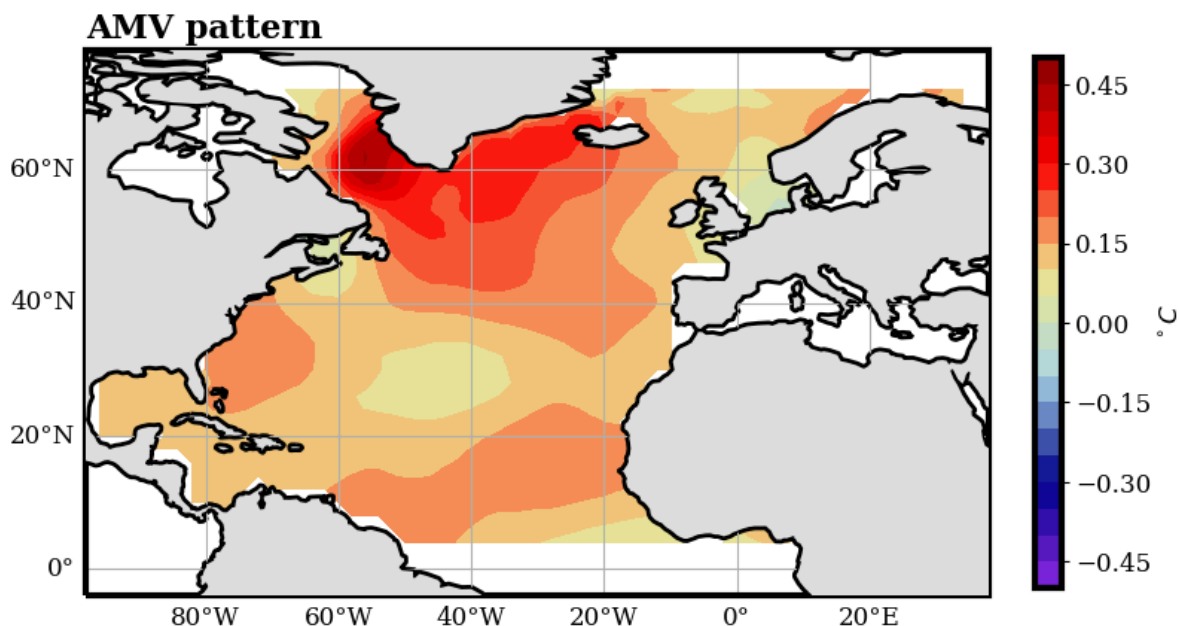


Fig. R5: Full AMV+ pattern (°C). SSTs are restored to follow the pattern between 10°N and 65°N. The AMV anomalies to which SSTs are restored are gradually reduced in an additional buffer zone 8° to the north and south of this box.

**84—85 The context of this sentence is not clear. Do you mean that for interpretation of the results (which show the positive phase), we assume that the negative phase response is equal and opposite?**

Yes, that is correct. To clarify this for the reader, 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.*"

**102-103 Not clear what you mean by that. Since you use monthly data, the calculation neglects transient eddies. Or are you using monthly means of covariant fields?**

We appreciate the opportunity to clarify this. While we use monthly mean fields for NEI, our methodology infers the divergent component of the MSE flux by assuming the energy budget is closed and storage is negligible. Under these assumptions, the inferred term implicitly includes the effects of the entire atmospheric circulation, including the time-mean circulation and transient eddies. We propose to revise Section 2.2 to remove potentially confusing language regarding the "small contribution" of eddies and add a clear explanation of how the inferred term accounts for the "total" energy transport.

**231—232 This is speculative. The energetic balance mandates that northward shifts of the ITCZ are associated with southward cross-equatorial energy fluxes. Specifically, it seems like the dominant NEI contribution is from the tropical Atlantic, west of the Sahel. I am therefore not convinced that excess heating of the north Atlantic as a whole is a well supported interpretation of the results. Moreover, negative NEI just south of the equator in the WAM sector might prove to be as important to the energetic contrast driving the WAM ITCZ shift. Similarly, changes in equatorial atmospheric NEI (at the orange rectangle in panel 4b), which determine the extent of ITCZ migrations in response to hemispherically asymmetric heating, may also contribute to the shifts of the WAM ITCZ (cf. Adam et al. 2019). As an alternative hypothesis, could it be that heating of the tropical Atlantic supports increased moisture transport to the WAM region leading to anomalous precipitation?**

We refer the reviewer to our detailed reply to their Major Comment 1. Regarding the specific role of equatorial NEI, which could act as a sensitivity factor for ITCZ shifts in response to a hemispherically asymmetric heating, we find no evidence of a consistent relationship between changes in equatorial NEI at Sahel latitudes (in the box in plot 4b) and Sahel precipitation (see Fig. R6).

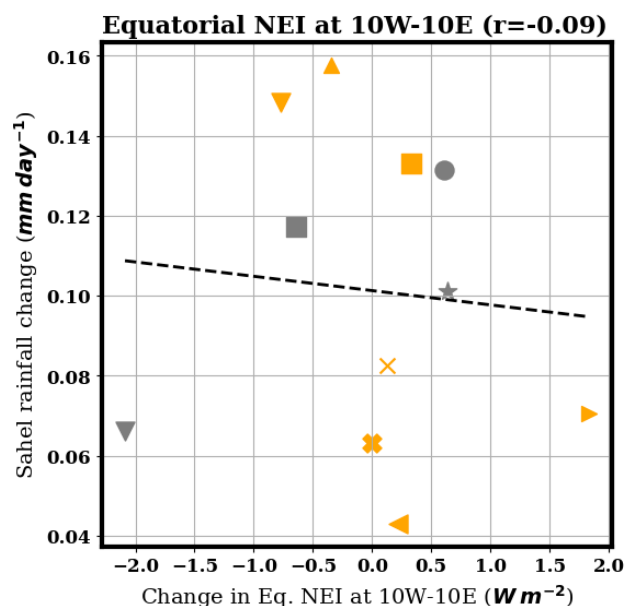


Fig R6: Scatter plots of averaged Sahel rainfall change ( $\text{mm day}^{-1}$ ) and NEI changes averaged over the equator at Sahel longitudes ( $\text{W m}^{-2}$ , see orange box in plot 4b).

**244 This formulation neglects the temperature difference between the atmosphere and surface, which can lead to some error (cf. Siler et al. 2019)**

We thank the reviewer for this remark. We acknowledge that our formulation involves approximations that are not clearly stated in the text. In particular, (i) near-surface wind speed is estimated from monthly mean zonal and meridional components at 1000 hPa, and (ii) near-surface relative humidity is approximated as the ratio between near-surface specific humidity and saturation specific humidity at the surface. The latter approximation makes our formulation consistent with Siler et al. 2019 (their eq. 2).

These approximations introduce some errors. In particular, neglecting the temperature difference between the surface and near-surface air can lead to errors in our estimation of the relative humidity changes. However, scaling arguments following Siler et al. (2019) indicate that, for typical temperature differences and their simulated changes, the resulting error in fractional relative humidity remains small (on the order of a few percent), and does not affect the qualitative interpretation of Fig. 5.

To clarify this point, we plan to revise the text to explicitly state these approximations. Specifically, we propose to modify lines 250–251 along the following lines:

*“In Fig. 5, we present these terms for the surface, except for the wind speed, which is estimated from monthly mean zonal and meridional components at 1000 hPa, and RH, which is calculated as the ratio between near-surface specific humidity and saturation specific humidity at the surface. These represent approximations to near-surface wind speed and relative humidity.”*

**247 (and elsewhere where relevant) relative → fractional**

We thank the reviewer for this correction. We propose to replace all instances in the manuscript following the reviewer’s suggestion.

**261—263 Also, consider the negative and positive feedbacks discussed in Karnauskas (2022) and Ganguly et al. (2024)**

We thank the reviewer for highlighting these feedbacks. Due to the experimental setup, the atmosphere and ocean are partly decoupled in the Atlantic north of  $2^{\circ}\text{N}$ . This prevents the full realization of Wind-Evaporation-SST (WES) feedbacks in the North Atlantic. This comment has, however, brought to our attention that this effective decoupling was not clearly explained in the previous version of our manuscript. We believe that the inclusion of figure R5 to the supplementary material and the clarification of where the SST restoration is performed will help clarify this. In addition, we propose the following updated text (Line 263): *“This yields a dipole in surface wind speed anomalies (negative north of  $10^{\circ}\text{N}$ , positive to the south; Fig. 5c), which in turn suppresses latent heat flux to the north and increases it to the south, consistent with the pattern in Fig. 5a. Note that in this region, the experimental setup prevents full atmosphere-ocean coupling, such that positive and negative Wind-Evaporation-SST feedbacks (e.g. Karnauskas 2022; Ganguly et al. 2024) are hindered.”*

**272—274 This is hardly the rising branch of the Hadley circulation, not is it “the” ITCZ, which are both zonal mean constructs. Perhaps a better way to describe these regional dynamic changes would be by treating them as changes in the the monsoonal Hadley-like circulation in the African sector.**

We thank the reviewer for this suggestion. We plan to adopt the reviewer’s suggested terminology, referring to the monsoonal Hadley-like circulation in the African sector and the main rainfall band rather than the global ITCZ.

**300—303 If we explain the easterly jet based on the thermal wind, isn’t this response a simple and obvious result of the northward shifted heated region over WAM?**

We agree that the AEJ response is a consequence of the shifted thermal gradient. We plan to revise this paragraph to clarify that increased soil moisture from enhanced rainfall promotes evaporative cooling, which alters the latitudinal temperature gradient and, via thermal wind balance, shifts the AEJ northward.

**Figure 5 caption ECMWF surface specific humidity can be readily calculated with surface pressure and 2m dew point temperature. No reason to exclude these models (e.g., <https://forum.ecmwf.int/t/how-to-calculate-hus-at-2m-huss/1254> ).**

We thank the reviewer for this tip. However, we have not been able to find near-surface dew point temperature in the data catalogue at ESGF for these models and experiments. Near-surface specific humidity and near-surface relative humidity were also missing for ECMWF-IFS-HR and ECMWF-IFS-LR models for PRIMAVERA amv-pos and amv-neg experiments in the ESGF catalogue. For this reason, they remain excluded from Fig. 5.

#### References:

- 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>
- 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>
- Zhang, R., Sutton, R., Danabasoglu, G., Kwon, Y.-O., Marsh, R., Yeager, S. G., Amrhein, D. E., and Little, C. M. (2019). A Review of the Role of the Atlantic Meridional Overturning Circulation in Atlantic Multidecadal Variability and Associated Climate Impacts, *Reviews of Geophysics*, 57, 316–375, <https://doi.org/10.1029/2019RG000644>