

Review of

# An energetic perspective on the impact of the Atlantic Multidecadal Variability on the West African Monsoon

Mohino et al.

## 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.

## 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.
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.

## 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).
- 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 in equal and opposite?
- 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?

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?

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

247 (and elsewhere where relevant) relative → fractional

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

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.

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?

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> ).

## References

- Adam, O., Schneider, T., Enzel, Y., & Quade, J., 2019: Both differential and equatorial heating contributed to African monsoon variations during the mid-Holocene. *Earth and Planetary Science Letters*, 522, 20-29
- Siler, N., Roe, G. H., Armour, K. C., & Feldl, N., 2019: Revisiting the surface-energy-flux perspective on the sensitivity of global precipitation to climate change. *Climate Dynamics*, 52(7), 3983-3995
- Ganguly, I., Gonzalez, A. O., & Karnauskas, K. B., 2024: On the role of wind–evaporation–SST feedbacks in the subseasonal variability of the East Pacific ITCZ. *Journal of Climate*, 37(1), 129-143
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