

Reply to Reviewer 2

(The reviewer's comments are reproduced in italics, followed by our responses. Line numbers refer to the preprint.)

Two 30-year coupled model experiments with ECWMF IFS-FESOM at 9 km resolution for a historic (1990-2019) and future (2020-2049, SSP3-7.0) period were investigated for West Africa and the adjacent Atlantic Ocean. A weakly active convection scheme has been used in the experiments. AEW activity is assessed based on 2-5 day bandpass-filtering of 700-hPa meridional wind. From this, Eddy Kinetic Energy (EKE) and energy conversion terms were derived. To understand the changes in AEW activity, the meridional PV gradient, static stability and relative vorticity was considered. To assess projected changes in African Easterly Wave (AEW) activity and related extreme and moderate rainfall events, both “AEW days” and “non AEW” days were defined. MSWEP and CHIRPS were utilized for observed rainfall and other “observed” meteorological parameters were taken from ERA5. A plausibility check of ECWMF IFS-FESOM is first carried out using these observational data sets. Overall, this is an interesting and well-written study, yet some issues need clarification.

We thank the reviewer for the positive assessment of our study and for the detailed and constructive comments, which will help us to clarify several methodological choices that were under-explained in the preprint.

Major:

1. *What is a “weakly active convection scheme”? The authors say that it has a reduced cloud base mass flux. The approach will likely not be familiar to many readers. What is the purpose of this? What implication does it have for West Africa where highly organized convective systems occur? In the literature, explicit convection experiments have been successfully used at about 9 km resolution for this region. Can the authors state what they expect in terms of organized convective system when using IFS-FESOM with explicit, parameterized and weakly active convection, respectively?*

We agree that the single sentence in Sect. 2.2 (lines 112–114) is insufficient, and we have proposed to expand it.

The IFS Tiedtke–Bechtold convection scheme is retained in full, but the cloud-base mass flux of the deep-convection closure is reduced (Becker et al., 2021; Rackow et al., 2025). Shallow and mid-level convection are treated as in the operational model. The scheme therefore still removes part of the convective instability, but more slowly than in the fully active configuration, so that a larger share of the deep convection is realised by the resolved dynamics. This is the configuration used in all nextGEMS IFS production runs (Segura et al., 2025), chosen by the modelling group after the Cycle 2–3 experiments showed it to give

the best overall balance of tropical mean precipitation, diurnal cycle and MJO/wave variability at 9 km (Rackow et al., 2025; Takasuka et al., 2025).

At 9 km, the grid is too coarse for deep convection to be resolved, but fine enough that a fully active mass-flux scheme, acting on every column, removes instability before organised resolved circulations can develop. This is a well-known cause of the too-early diurnal peak, too-weak propagating systems and weak AEW–rainfall coupling in parameterised simulations of West Africa (e.g. Marsham et al., 2013; Birch et al., 2014). Switching the deep scheme off entirely at 9 km, on the other hand, delays convective onset, produces isolated and overly intense grid-point storms and, in a global coupled model, degrades the tropical mean state. The reduced-mass-flux setting is a compromise: parameterised convection handles the unresolved, unorganised part of the spectrum, while the reduced sink of CAPE lets organised, resolved systems develop and propagate.

With fully parameterised convection, we would expect the known deficiencies: rainfall too closely tied to local instantaneous instability, weak westward propagation, and a diurnal cycle peaking around noon rather than in the late afternoon and night. Studies with fully explicit convection at 9 km show that propagating squall lines and the nocturnal rainfall maximum are recovered, at the cost of too-few and overly intense systems (Marsham et al., 2013; Birch et al., 2014; Pante and Knippertz, 2019). The weakly active configuration is expected to lie between these two: propagating, cold-pool-driven systems are permitted, but the residual scheme smooths the intensity spectrum.

2. *The authors write „To isolate AEW days, the JJAS 700 hPa filtered meridional wind at the same base point (i.e. 11.5°N, 1°W) was used. AEW days were identified as those corresponding to wind anomalies exceeding the 90th percentile or falling below the 10th percentile of the filtered distribution. The rationale is not clear to me. Does this mean that „AEW“ and „non-AEW“ days were defined sub-continent wide relying on the base point and not locally for a grid point? And selecting the upper/lower deciles of the 2-5 day bandpass-filtered meridional wind at the base point: Doesn't this mean that you look at days with large AEW-related northerlies and southerlies respectively, i.e. to a region ahead or behind an AEW trough? Would the passage of the AEW trough axes at the base point not be associated with new zero meridional filtered winds but still be a potential AEW day? And why did the authors only use one base point and not others up- and downstream? Clearly, since these definitions are central to the following, more explanations and justifications are needed.*

Thank you for pressing on this point; the definition is central to Sects. 4.1–4.2 and its description in Sect. 3.1 (lines 125–131) was too compressed. AEW days are defined once for the whole domain from the base point, not grid point by grid point, to classify days by the presence of strong 3–5-day wave activity crossing West Africa following Crétat et al. (2015). The reviewer is right that the top and bottom deciles of filtered v' at the base point select days of strongest northerlies

(ahead of the trough) and strongest southerlies (behind it), and that the trough axis itself passes when v' crosses zero. This is why the ± 2 -day offset is applied (line 129): for a 3–5-day wave, the trough (and ridge) passage lies within about one day of the v' extremum, so extending each selected day by ± 2 days captures the full passage of the wave, trough included, and not only the two wind maxima. We also repeated the classification with a narrower ± 1 -day window, which gave very similar results, so the conclusions are not sensitive to the width of the window; we propose to include a comment on this in Sect. 3.1. The base point (1°W , 11.5°N) is the one used by Kiladis et al. (2006), which keeps our composites directly comparable with their observed AEW structure. It also lies in the region of maximum 3–5-day v' variance over land and of strongest AEW–rainfall coupling in both observations and the model (Figs. 2, 4), and it is the same point used for the composites in Fig. 3. We did not add base points up- or downstream because, for westward-propagating waves, they would mostly select the same waves one or two days earlier or later. With a phase speed of about 8 m s^{-1} (roughly 6° of longitude per day), the ± 2 -day window means that on an AEW day the selected wave lies anywhere from about 12° upstream to 12° downstream of the base point, so the classification already spans roughly 11°E to 13°W , most of the West African Sahel. We will expand Sect. 3.1 along these lines, explaining that AEW days are defined regionally from a single base point, how the ± 2 -day window captures the trough passage (and that a ± 1 -day window gives very similar results), why this base point was chosen, and how far from it the classification remains meaningful.

3. ***Related to the previous comment: As also mentioned by the authors in the Introduction, there is a northerly and southerly AEW track. In previous literature, the southerly track was investigated using bandpass-filtered 700 hPa meridional wind for example, but the mostly dry northerly waves which grow due to baroclinic conversions are better captured by the 850 or even 925 hPa level. Why did the authors only stick to the 700 hPa level, so essentially to the southerly AEW track? While they are more important to rainfall, the northerly AEWs might grow in intensity or shift due to changes in the meridional temperature gradient.***

We agree, and this point was also raised by Reviewer 1. The choice of 700 hPa and of the 3–5-day band reflects the focus of the paper on the rainfall-bearing waves; extending the full validation, energetics and rainfall decomposition to the 6–9-day band and to 850/925 hPa would have significantly increased the size of the paper, and we chose not to do so for brevity. Nevertheless, the 700 hPa 3–5-day analysis also includes northern track waves. Waves of this period exist on both flanks of the jet (Thorncroft and Hoskins, 1994; Pytharoulis and Thorncroft, 1999); the filtered v' variance in Fig. 2a–b peaks near 20°N over the eastern Atlantic; the composites in Fig. 3 extend into the Sahara; and, most directly relevant to the reviewer's point, the energetics in Figs. 5 and 8 cover both flanks. The only term that intensifies north of the jet in the projection is CA in the 16 – 22°N band, i.e. the low-level baroclinic growth driven by the enhanced meridional temperature gradient of the Saharan heat low (Thorncroft and

Blackburn, 1999), whereas the southern-track response is dominated by the convective terms Cpk and GE. This is exactly the mechanism the reviewer anticipates. We propose to add a paragraph to Sect. 4.2 that draws out this contrast and links it to the amplified Saharan warming. A dedicated northern-track analysis, including the 6–9-day band and the lower levels the reviewer mentions, is left for future work.

Fig. R1 shows the projected change in 6–9-day EKE at 700 hPa, computed in the same way as Fig. 7 of the manuscript. The 6–9-day waves, which are more representative of the northern track, respond differently from the 3–5-day waves. The most prominent signal is a dipole over the eastern tropical Atlantic. The 6–9-day EKE decreases significantly off the coasts of Senegal and Mauritania, reaching about $-0.5 \text{ m}^2 \text{ s}^{-2}$ near 20°N , 25°W , with weaker reductions extending over the westernmost Sahel. Farther northwest, it increases significantly, by up to about $0.5 \text{ m}^2 \text{ s}^{-2}$ near 25°N , 35°W . Elsewhere over the Sahel the changes are weak (mostly within $\pm 0.2 \text{ m}^2 \text{ s}^{-2}$) and patchy, with nothing comparable to the 3–5-day increase over West Africa in Fig. 7, while over the Sahara they consist of alternating positive and negative cells of small spatial scale. In our simulations, therefore, the 6–9-day waves do not intensify over land; instead, their activity appears to be displaced northwestward over the eastern Atlantic, in line with the reviewer's suggestion that the northern-track waves might shift.

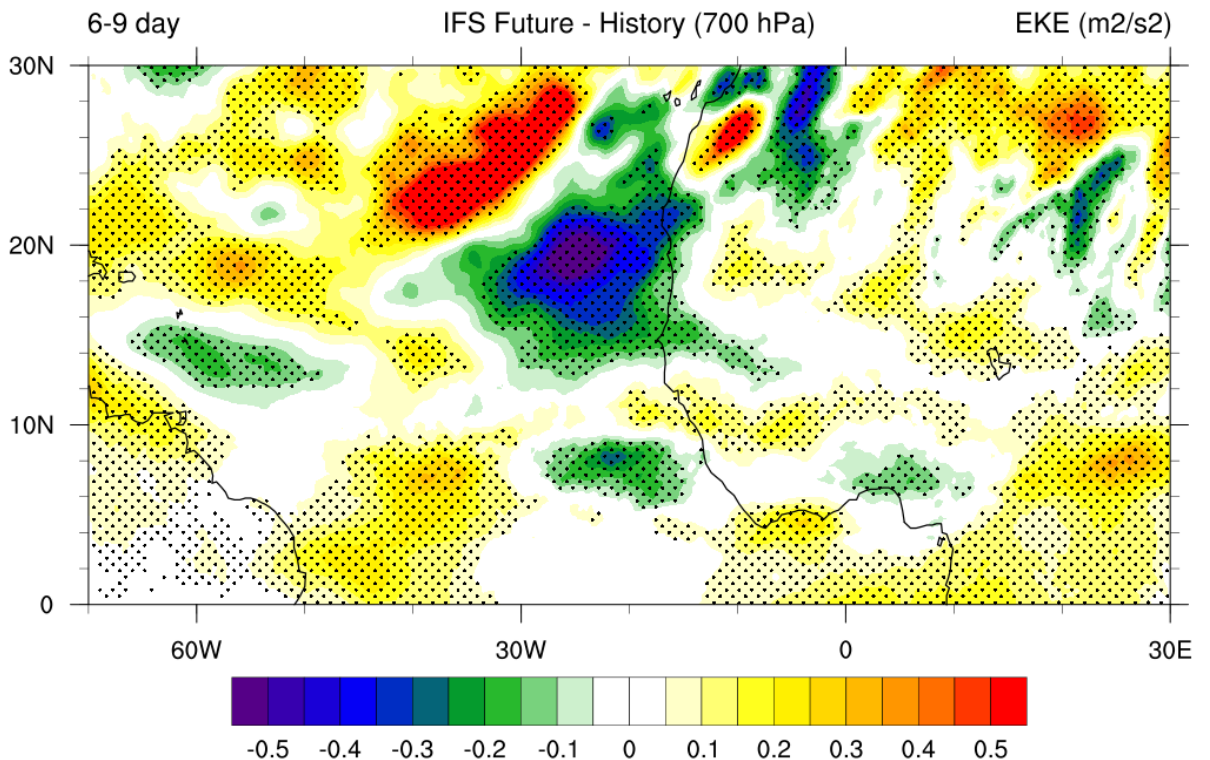


Figure R1. As Fig. 7 of the manuscript, but for 6–9-day band-pass filtered winds: difference between JJAS 6–9-day EKE at 700 hPa in the future (2020–2049) and historical (1990–2019) simulations ($\text{m}^2 \text{ s}^{-2}$). Stippling marks regions where the p-value of the local null hypothesis of equal means is below 0.05.

4. *Again, related to comment 2): While I acknowledge that the bandpass-filtered meridional wind at 700 hPa has been used to create AEW composites, the AEW is an object, and numerous object-tracking algorithms are freely available now. Defining the AEW trough and tracking it would yield a much more obvious AEW-rainfall relationship than used in the present study. Can the authors reflect on the implication of using their statistical approach vs. an object-following approach in the conclusion.*

We agree that trough tracking (e.g. Berry et al., 2007; Brammer and Thorncroft, 2015; Belanger et al., 2014; Lawton et al., 2022) would give a sharper, trough-relative picture of the AEW–rainfall relationship, together with a wave count and intensity distribution that our approach cannot provide. We will mention the important limitations of this study in the Discussion section. These include the following points: the increase in the fraction of AEW days is a measure of amplitude exceedance, not of wave count, and the projected weakening of EKE over the Atlantic could reflect fewer waves surviving the coastal transition, a question that only tracking can settle. An object-based analysis of these IFS-FESOM simulations, using trough tracking, is planned as future work.

Minor:

375: It would be helpful to refer to previous literature and add a sentence or so why the amplified Sahara warming occurs. The reasoning given is not clear.

This point was also raised by Reviewer 1. We agree that this is a missed opportunity, both for placing the projection in the context of observed trends and for tying the thermodynamic change more explicitly to the energetics results. So the following references will be added to the results and discussion: Cook and Vizzy (2015), Vizzy and Cook (2017), Evan et al. (2015), Lavaysse et al. (2016), Zhou (2016).

When IFS is compared to ERA5 like in Figs. 1 and 2, it would really help to add a third column showing the difference. It would really make it easier to comprehend the many statements made by the authors on the differences.

We agree that showing the difference will make it easier for the reader to understand. We have replotted the figures and will make the necessary changes to the text in the results.

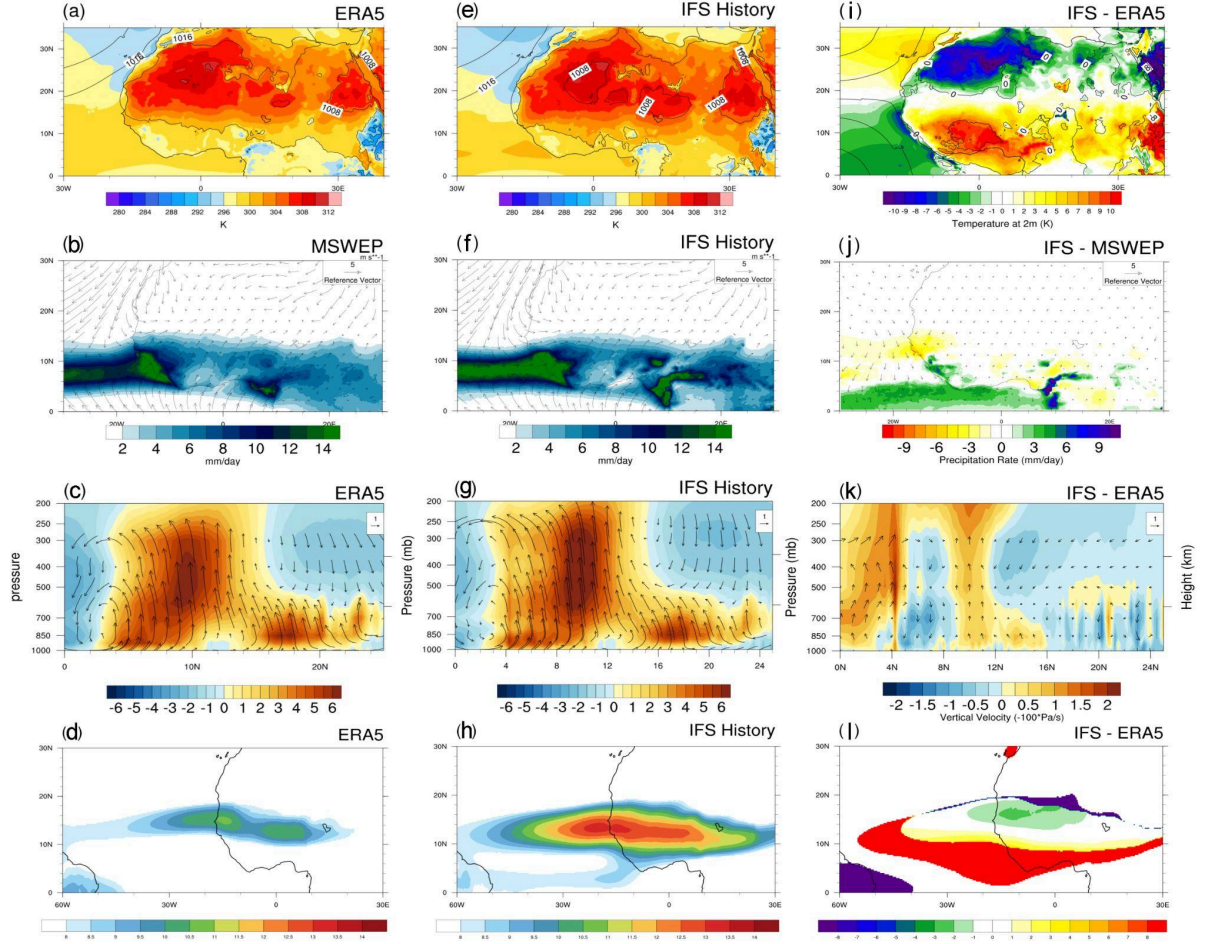


Fig.1: Mean JJAS fields in the observations (left column), in the IFS model (middle column), and their difference (left column): surface temperature (K, shaded) and sea level pressure (hPa, contours) (a,e,i); total precipitation (mm/day, shaded) and surface winds (m/s, vectors) (b,f,j); vertical-cross section zonally averaged between 20°W-30°E of omega (in 10⁻² Pa/s with a change of sign where positive values represent upward motion, shaded) and omega-meridional wind vectors (m/s for meridional wind while omega is scaled to match the magnitude of the meridional wind, vectors) (c,g,k); wind speed at 600 hPa (d,h,l). In the left column, all variables are derived from ERA5 except for precipitation, for which MSWEP dataset was used.

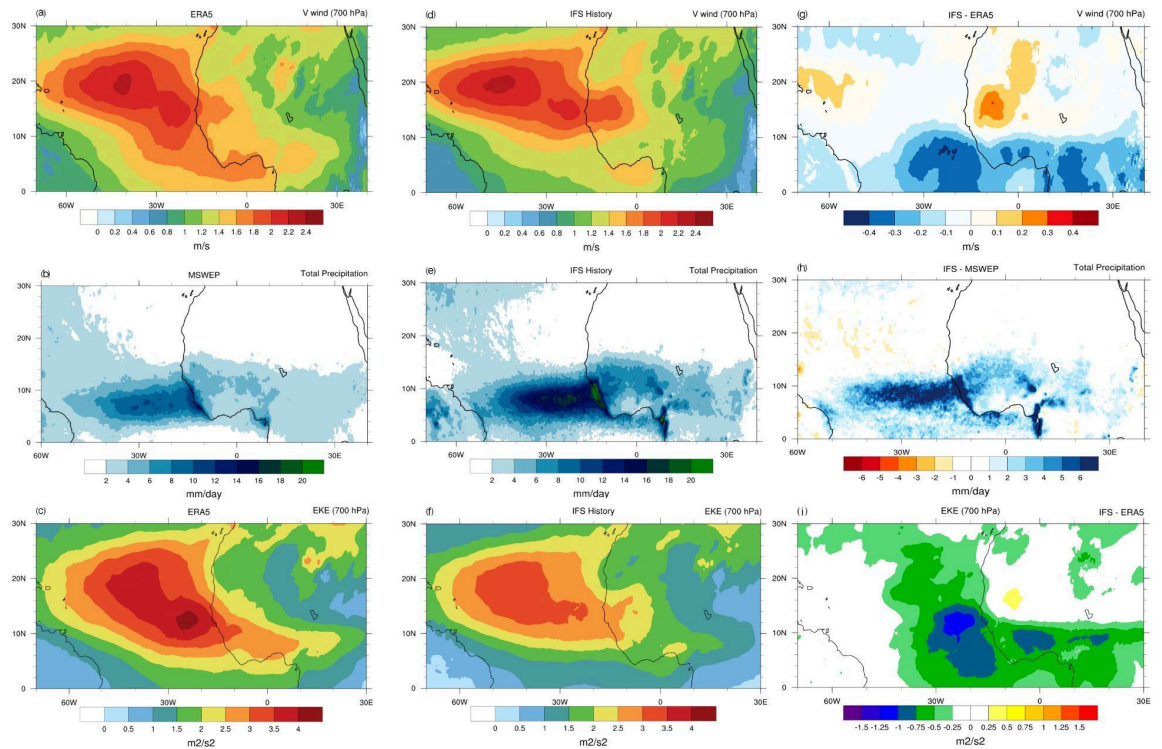


Figure 2. Variability associated with AEW in ERA5 reanalysis (left column), IFS model (middle column), and their difference (left column). Standard deviation of band-pass filtered fields between 3 to 5 days during JJAS season of: meridional wind at 700 hPa (m/s) (a,d,g); precipitation (mm/day) (b,e,h); eddy kinetic energy at 700 hPa (m²/s²) (c,f,i).

Caption of Fig. 3: “11.5°N”, the “N” is missing.

Thank you for pointing out the typo. It will be corrected in the revised manuscript.

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