Reviewer #2

The paper "Meteorological ingredients of heavy precipitation and subsequent lake filling episodes in the northwestern Sahara" by Rieder et al. uses satellite observations and ERA5 output to to identify lake filling events in the northern Sahara and characterize the large-scale meteorological conditions associated with them. The manuscript is well written and addresses the important topic of heavy precipitation events in a complex part of the world and has interesting implications for paleoclimate. However, clarification of some key points are needed, so I recommend this paper be accepted with revisions.

We would like to thank Reviewer #2 for the time taken to read our manuscript and for the helpful comments. We are also glad to read the reviewer find the paleoclimatic implications interesting.
Please see below our detailed response to all the comments raised by the reviewer.

You've shown very clearly that the high precipitation events (HPEs) you've identified are associated with large-scale circulation patterns and moisture convergence, which makes sense. Are there conditions where the pattern of extratropical cyclones and and upper level PV anomalies *don't* result in these events? How frequently do these patterns coincide? It would be interesting to see how what composites of these large-scale patterns that do not produce HPEs look like: if they don't result in moisture convergence this would be a very strong support for your claim. Or if you showed cases of the low-level cyclone without the PV anomaly, that might show moisture transport but not the ascent. This is probably beyond the scope of your paper, but should be considered.
We agree with the reviewer that the co-occurrence/non-co-occurrence of upper-level PV features and surface cyclones is indeed an interesting question. To emphasise the importance of the upper level PV features, we show below (Fig. R1) the 315 K PV climatology for DJF for the years 2000-2021 for (a) all days, (b) days with LFE-generating HPEs,(c) strong HPEs, and (d) medium HPEs. This figure, as well as Fig. 12, Fig. A6-A7, highlight how anomalous upper-level and surface conditions are during HPEs. In the paper, we address the frequency of both surface cyclones (~5-10% of time; Wernli and Schwierz, 2006) as well as upper-level PV cutoffs (~5-10% of time; Portmann et al., 2021) in the vicinity of the catchment (L520-527). Of course, the fact that both systems have a similar frequency in the area may be a coincidence. However, there are many reasons to think that at least when precipitation peaks, this co-occurrence is not happening by chance. To demonstrate it, we took the liberty to use a figure from an earlier version of a preprint now accepted to Communications Earth and Environment by de Vries et al., showing not only the association of extreme precipitation with Rossby wave breaking (Fig. R2a; defined there as the occurrence of a PV streamer or a cutoff low) but also the precipitation surplus due to Rossby wave breaking. The surplus represent estimates of how much precipitation is enhanced or reduced due to Rossby wave breaking, and therefore, where positive (as in the studied catchment) precipitation is enhanced when cutoffs occur and is reduced when they are absent. That being said, in order to complete the answer of the non-co-occurrence of upper-level PV features and surface cyclones on HPEs in the region, further studies are needed. Therefore, we have added the following statement to our discussion: “Additionally, the presence of upper-level PV features and low-level cyclones does not warrant the formation of a HPE, and further studies are needed to determine the frequency of these features without HPE formation.” (L527-528)

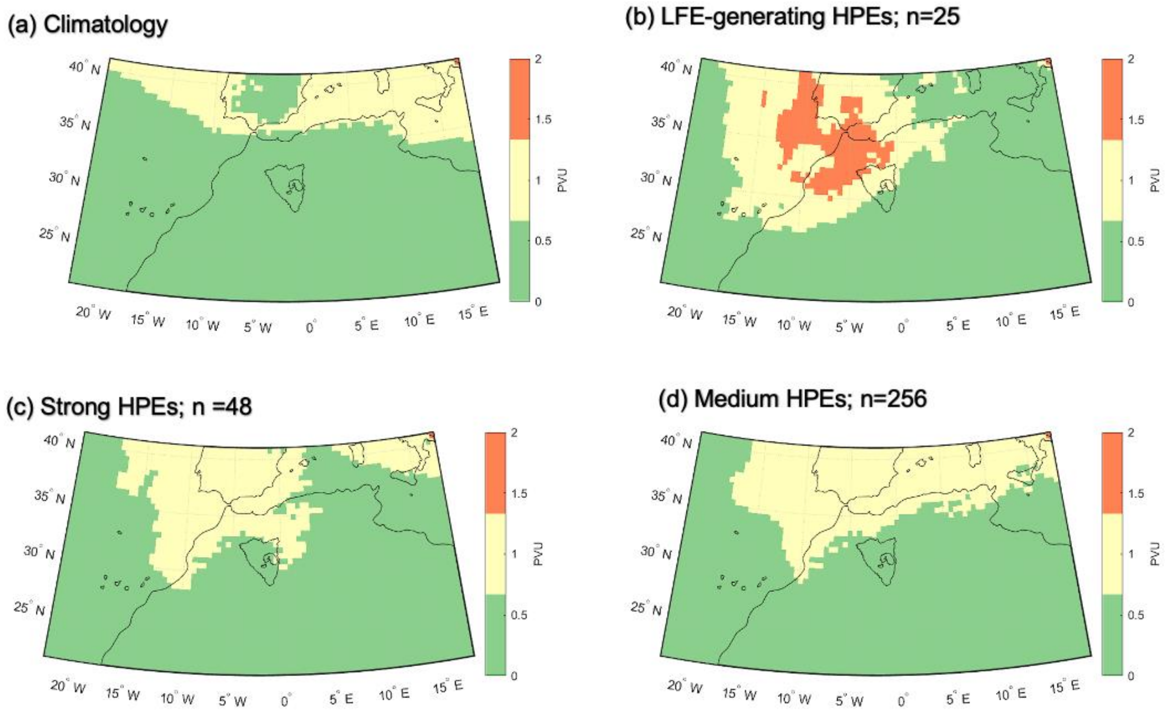

Figure R1. PV climatology at 315K for DJF months between 2000 and 2021 (a), during LFE-generating HPE days (b), strong HPEs (largest 20% of HPEs; c), and smaller HPEs (d).

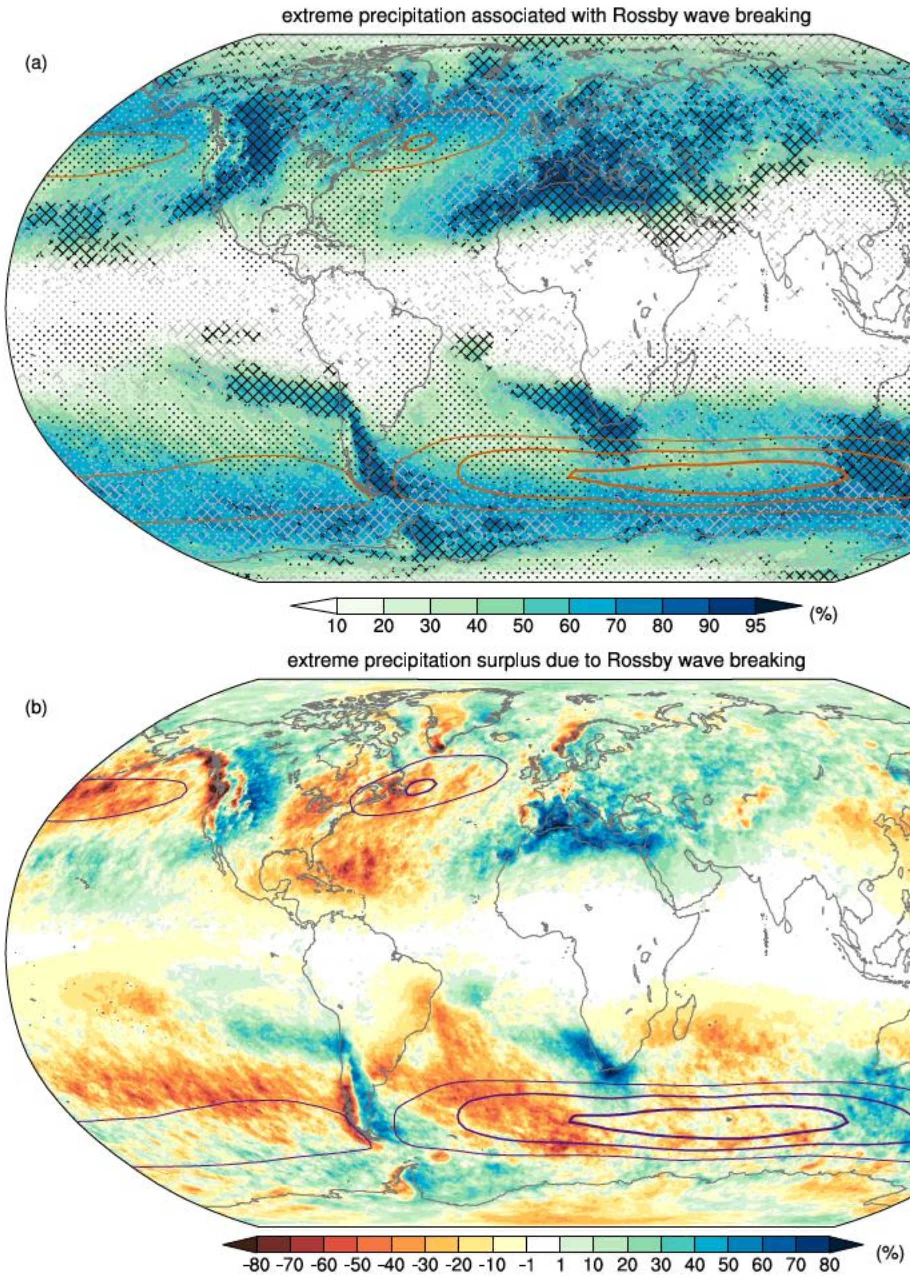

Figure R2. Association of extreme precipitation and Rossby wave breaking (RWB; a), and the precipitation surplus due to RWB (b). The figure was adapted from de Vries et al., (accepted).

I don't fully understand why this moisture recycling "domino-effect" is required for these HPEs. I think this is an interesting hypothesis which should be explored further, but there isn't fully evidence for it. I would suggest moving this to a discussion instead of continually emphasizing it as something that is definitely happening. Could this possibly be due to choice of reanalysis? I always worry when I see a study that relies only on a single reanalysis, especially in regions that have sparse observations. If you used MERRA2, which might represent below cloud evaporation differently than ERA5, would you see the same regions of moisture supply?

We thank the reviewer for the opportunity to elaborate on the domino effect, as it seems the way we presented it in the manuscript made it unclear. To make the description of the domino effect clearer, we have now rearranged all the text that concerns it, and concentrated it in the last part of the results as a "bridge" to the discussion, where it is discussed further. The effect is described

in the manuscript in order to explain how it is possible for the Sahara and the dryland regions surrounding it to behave as moisture sources. Fig. 9a and c, and Fig. 13 show that these dry areas were marked by the moisture source diagnostic (MSD) as the origin of the moisture for precipitation during HPEs in the catchment. However, clearly, these areas cannot be a major primary source of moisture because they are dry at the surface. To explain this discrepancy, we analysed vertical profiles along the trajectories (Fig. 10) as well as the evolution of the vertical profile of the atmosphere over the catchment (Fig. 8). Both these figures hint towards evaporating precipitation that falls before (and upwind) the occurrence of the main HPE. Therefore, in the manuscript, we suggest that at least part of the moistening of the atmospheric column upwind of the catchment is caused by this type of precipitation evaporation, which explains why the MSD mark these areas as moisture sources.
As the reviewer suggests, different reanalysis products may yield different results in terms of the moisture diagnostic. However, since the synoptic scale flow patterns are rather similar between different reanalyses (e.g., Davies and Sprenger, 2024) and humidity in North Africa (as well as other parameters) is similar between MERRA2 and ERA5 (Baba et al., 2021; Johnston et al., 2021), we think that while the exact amount / location of this domino process can slightly differ between different data sources, the effect itself is not a consequence of the choice of reanalysis product. Clearly, redoing the whole analysis with another reanalysis dataset is beyond the scope of this manuscript, but it would indeed be interesting to assess the representation of this mechanism in different numerical models because the evaporation of precipitation below the cloud is a process that is not well constrained. As shown in a sensitivity study in Aemisegger et al. (2015) for a cold front passage case study over Europe using the regional numerical weather prediction model COSMO (Steppeler et al. 2003) neglecting below cloud evaporation locally can lead to an increase in rainfall intensity of 74%. In this study, we do not have the observational means to quantify the importance of this process, however, based on the detailed trajectory analysis shown in Fig. 10 we can descriptively pinpoint the mechanisms that lead to the handover of moisture from an upper-level ice cloud layer to a lower tropospheric air stream that is originally relatively dry and comes from the Sahel. What we present is indeed a phenomenological description: we diagnose a progressive increase in specific humidity (Fig. 10a) in the dry airstream from the Sahel as it travels just beneath the ice cloud from which the falling snow (Fig. 10b violet contour) gets fully sublimated as it falls in the airstream (trajectories' location is shown in Fig. 10a by the orange line).
Finally, we would like to emphasise that rather than the type of reanalysis used, we think that the assumptions and thresholds chosen in the setup of the moisture source diagnostics play a much more important role. From a current intercomparison effort of existing methods (comparing 20 ensembles from 7 different methods, Benedict et al., 2024) we see that the footprint areas are the same, while the weights of different contributions, in particular, the contribution of local vs. remote sources can differ between methods as well as between ensemble members of the same methods using different threshold settings. This is in agreement with earlier comparisons between Lagrangian and Eulerian moisture source analyses (Winschall et al. 2014).

One part of your methodology that confused me was the use of both LANDSAT and MODIS. Why don't you just use your algorithm on MODIS, instead of just using it for visual confirmation of these events? Some clarification on how you are using both of these and why would be greatly appreciated.
While our methodology is applicable to MODIS imagery, we chose to apply it to Landsat images because of the higher spatial resolution it gives. Landsat pixels are ~70 times smaller compared to MODIS's and therefore, provide much better estimates of the area covered with water over such a small lake, where high slopes characterise the edges of the lake. However, given the coarser temporal resolution of Landsat imagery, we used MODIS imagery to (a) verify the results obtained by our Landsat-based algorithm, and (b) to make sure we have successfully detected

all LFEs. For (b) we visually inspected the 10 largest HPEs, where we thought there might have been LFEs the algorithm missed (although eventually there were no such events).

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