Answer to Obbe Tuinenburg (R2) in the Interactive comment

I have read and assessed the manuscript. Although I like the topic of the study, I am a bit worried about the experimental set-up, because it is unclear to me which experiments are compared. Furthermore, I am concerned about the assumptions about the atmospheric moisture budget taken in the Lagrangian moisture tracking model. I realize that these assumptions have been taken in many previous studies, but I think the they impact the conclusions significantly.

We would like to thank very much the referee for his kind remarks.. Please find below the responses to your comments.

Some of the details of the experiment are unclear to me. I believe this experiment is comparing online Eulerian to off-line Lagrangian methods, which is not entirely fair. I would recommend doing the experiment with all methods in an online mode and all methods in an offline mode, so their differences can be more meaningfully interpreted. Furthermore, a lot of details about the model settings are not included and these may be important, see our work on the assumptions influence moisture tracking models: https://hess.copernicus.org/articles/24/2419/2020/ (Tuinenburg and Staal, 2020)

We do not agree that it is not meaningful to compare online and offline methods. After all, both methodologies have the same objective: the characterization of moisture sources. If both methodologies were close to "reality", both should provide similar results regardless of whether they are on-line or off-line. In fact, other authors have previously made this type of comparison (e.g. Dominguez et al.,2019).

At the reviewer's suggestion, some more details concerning the configuration of the models have been included (see for example the new Table 1). We have also restructured the methodology for a better understanding of the experiment carried out.

Dominguez, F., H. Hu, J.A. Martinez, 2019: Two-Layer Dynamic Recycling Model (2L-DRM): Learning from Moisture Tracking Models of Different Complexity, J. Hydromet. V. 21 I. 1 DOI: 10.1175/JHM-D-19-0101.1

I am worried about the assumptions regarding the atmospheric moisture budget that are used in this study but have been used in a lot of similar studies using FLEXPART. The main idea in this model is that a change in the atmospheric precipitable water along a trajectory is allocated to the total water budget at the surface (E-P), rather than its individual components E and P. As far as I can retrace, this assumption stems from the paper by Stohl (2004) on FLEXPART. It seems to be an assumption that is convenient from the atmospheric moisture budget perspective, but it becomes problematic when you actually want to allocate changes in atmospheric moisture to either E or P. I assume this approximation was warranted in the time when FLEXPART was developed when the surface fluxes (and especially E) where very model dependent and frequently used to reduce the near-surface biases of the model wrt observations. As a result, evaporation estimates were frequently unrealistic or unphysical. I would argue that at present, the surface fluxes are estimated a lot more reliably and therefore I wonder why the fluxes are not used directly, but rather the method still relies on using the total budget. I think this practice creates significant biases in moisture allocation.

We fully agree with the reviewer on this point. Changes in the moisture content of Lagrangian particles is assigned to a process of evaporation (E) or precipitation (P), when this is not always the case. In fact, Ruud van der Ent in his comments proposes a physical mechanism, convergence and divergence, which gives rise to increases or decreases in the moisture content of air parcels that has nothing to do with E or P. This paper aims precisely to quantify the inaccuracies of the Lagrangian method and discuss its possible causes, such as the one just mentioned.

Specifically on L92. For situations where E-P<0, E is assumed to be zero. I had a look at the ERA5 data to check how well this assumption holds for the domain and days considered (36N-48N, 10W-8E, over 19-21 Oct 1982 and 6-8 Nov 1982). As the authors did, I aggregated the data to 3-hourly means (from the hourly ERA5 resolution). The fraction of evaporation that occurs when E-P<0 is about 32% of the total evaporation for the domain (globally this is about 16% for these days).

We would like to thank the reviewer for his work. Taking into account the values provided by the reviewer, we have decided to remove from our article the discussion related to the E field and also the figure in which we showed it (Figure 8). Therefore, we have kept only the E-P field, without separating it.

However, we would like to clarify that we were aware that the separation of the E-P field into its two components is problematic, especially for the analysis of moisture sources in specific precipitation events. This was already stated by Stohl and James 2004. The reason why we included it, being aware of the inaccuracies involved, is because we believe that the qualitative analysis of sources from the E-P field should be complemented with the E-field. Some researchers assign only regions with E-P>0 as moisture sources. But this need not always be the case. Areas with negative E-P could also have contributed to the precipitation accumulated in the event studied. The E-field does not suffer from this problem, which can lead to misinterpretation of the results.

Stohl, A. and James, P.: A Lagrangian analysis of the atmospheric branch of the global water cycle: Part 1: Method description, validation, and demonstration for the August 2002 flooding in central Europe, Journal of Hydrometeorology, https://doi.org/10.1175/1525-7541(2004)005<0656:ALAOTA>2.0.CO;2, 2004.

Regarding the assumption of the precipitation events, on L103, only moisture is allocated when E-P < -2 mm per 3h. Again, I had a look at the ERA5 data for the domain and days. The precipitation events for which the condition is true only represent 75% of the precipitation for the domain (globally this is only 47% for the days considered).

I think that these fractions of the evaporation and precipitation events missed is significant. Depending on how these E and P events are distributed compared to the cases studied here, the results will probably be affected quite a bit. (I realize the present study is done based on WRF simulation, which are different than the ERA5 reanalysis, but I would not expect these fractions to be very different.)

Thresholds similar to E-P < 2mm/3h have been used by other authors in the past (e.g. Stohl et al.,2008). This assumption is only used to select the air parcels that contribute to the heavy precipitation events within the target area (Figure 1c), so we see no inconsistency in this point.

Stohl, Andreas, Caroline Forster, and Harald Sodemann. "Remote sources of water vapor forming precipitation on the Norwegian west coast at 60 N–a tale of hurricanes and an atmospheric river." Journal of Geophysical Research: Atmospheres 113.D5 (2008).

L 59-60: This was not the main conclusion of Van der Ent 2013. The main conclusion of that paper was that both methods have differences in moisture flow representation compared to the on-line tracking. The main problems occur in locations with a lot of vertical variability in the horizontal integrated moisture flow, such as monsoon areas with strong flows at the surface and return flows higher up in the atmosphere. The Eulerian model in that study initially considered the vertical integral of the horizontal moisture flow, leading to underestimation of horizontal moisture flow in situations where the flows are opposite in the lower and upper atmospheric levels (thus cancelling each other). Based on this study (and a lot of other work), the WAM model was adapted to use two layers in the vertical.

Thank you for your comment. The introduction has been modified and the sentence "where the evaporated moisture from Lake Volta (in West Africa) was tracked until it precipitates, concluding that the Lagrangian method leads to inaccuracies in the calculations in the presence of strong wind shear" is no longer included in the text.

L360: How exactly was the ERA-Interim data used in the present study? I did not find any other mention in the manuscript rather than in the acknowledgements.

The ERA-Interim was used as initial and boundary conditions for the Eulerian WRF-WVTs model. Since the results provided by this model had already been published in another study (Insua-Costa et al., 2019), we have decided to remove this database from the acknowledgements.

Insua-Costa, Damián, Gonzalo Miguez-Macho, and María Carmen Llasat. "Local and remote moisture sources for extreme precipitation: a study of the two catastrophic 1982 western Mediterranean episodes." Hydrology and Earth System Sciences 23.9 (2019): 3885-3900.