Answer to comments by Dr Ruud van der Ent – Delft University of Technology

After seeing part of this work presented at the EGU GA in 2020, I am happy to see this work in written form. The comparison of different moisture tracking models is important and timely. In this paper, the authors highlight several errors in moisture tracking arising from a Lagrangian offline method FLEXPART-WRF in comparison to an Eulerian online method WRF-WVT. I have some comments that I think the authors should be able to take into account in a revised version of their manuscript.

We would like to extend a special thanks to Ruud van der Ent for his brilliant comments, which have been of great help in improving the article.

The authors present their results as Eulerian vs. Lagrangian, but I think this is misleading. In fact there are many more differences between the methods as indicated roughly in the table I made below. I think it would be good if the authors expanded/improved this table and used it in their paper. Also, I would actually suggest adjusting the title, because these findings cannot be generalized to all Eulerian or all Lagrangian models. For example, the moisture tracking method I developed myself is Eulerian offline and not very computationally demanding.

Thank you for the time invested in the table. We think it is a very good idea and we have included it. We also agree that we have not been careful enough about the distinction between the different Eulerian and Lagrangian methods. Indeed, the results obtained cannot be extended to all types of Lagrangian and Eulerian methods, so we have modified the text (also the title) to try to improve this weak point of the article.

On line 13-14 the authors conclude "We argue that such an inconsistent contribution is associated with the fact that the Lagrangian method does not consider moisture phase changes." However, I do not think that the phase changes play a major role. In Van der Ent et al., (2013) we found that the effect of phase changes on moisture tracking is really minor and does not significantly effect the patterns of the moisture tracking.

Indeed, our hypothesis that the unrealistic values found in the Sahara were due to phase changes was wrong. Motivated by your comment and also by the first reviewer's comment, we have tried to quantify how phase changes affect the results obtained. To this end, we have taken advantage of the fact that the WRF model provides us with 6 moisture species (vapour, cloud water, rain water, snow, ice and graupel) to include the sum of all these species in the Lagrangian analysis. That is, we have repeated the calculation shown in Eq. 1: $e-p=m*dq/dt$, but in this case q would be the sum of all moisture species within an air parcel instead of just water vapour. Results have shown that the effect of including liquid and solid water in the model is very small. Specifically, the E-P field values change by about 4% on average (absolute) when liquid and solid water are included (see section 3.3 of the article).

Yet, of the differences mentioned in the table above, the key problem with FLEXPART-WRF in my understanding is the combination of the Lagrangian moisture pathways AND evaporation attribution by E-P balance. As Obbe Tuinenburg already pointed out in his review this does not work very well because E and P can be concurrent during the same time step. But there is also another issue, which surprisingly has rarely been mentioned, namely the fact that the E-P balance in a Lagrangian framework is neglecting convergence and divergence in the atmosphere. Suppose you have a grid box in a Eulerian sense and convergence takes place equally from all sides, then the volume in the grid box increases, in a Lagrangian setting a parcel exactly at the center of this grid box stays in the same place, but also its volume increases. Now, in the E-P backtracking method, the result would be an attribution to evaporation, but this is not what happened in reality. In such a way you can obtain moisture gains and lossed along the pathway with E and P both being 0. It was already noted by Stohl & Seibert (1998) that specific humidity fluctuations along a trajectory may be entirely unphysical, and Stohl and James (2004), who evaluated the FLEXPART methodology, found that when FLEXPART is used to evaluate E and P separately, evaporation is highly overestimated. In my view, this is a more logical explanation for the wrong attribution of moisture sources over the Sahara than the issue of phase changes (e.g. also discussed in lines 281-289).

We have to acknowledge that we had not been aware of this problem. We agree that this deficiency is probably to a large extent the cause of the unrealistic values found. In fact, we have included a new figure (Figure 10) outlining this problem.

A last suggestion I would like to make is that the figures now mostly just show the FLEXPART-WRF results, whereas it would in my opinion be more informative when the results of WRF-WVT and FLEXPART-WRF would be presented next to each other (i.e. a spatial version of table 1).

We have not been able to straighten out this point because the approach of the two models is totally different so we think it is not possible to make a comparison of the two methodologies in a "spatial" figure.

In conclusion, the findings of this study are important and should present a clear warning to anybody that uses the E-P method for attributing evaporative sources as the authors show that leads to major errors and unrealistic results. The authors made a fair comparison between a golden standard online method WRF-WVT and thus have all the right to be even more outspoken against the use of attributing evaporative sources based on E-P and I hope they bring across this point more strongly in a revised manuscript. Yet, they should be careful in their semantics as the conclusions may not apply to just any other Lagrangian or Eulerian method.

Thank you again for your positive remarks.

References

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