Answer to Anonymous Referee (R1) in the Interactive comment on "Simple physics-based adjustments reconcile the results of Eulerian and Lagrangian techniques for moisture tracking" by Alfredo Crespo-Otero, Damián Insua-Costa, Emilio Hernández-García, Cristóbal López and Gonzalo Míguez-Macho

General comments

This study investigates the uncertainty in precipitation source regions estimated by three different modeling approaches. Precipitation sources estimated by the online Eulerian-based WRF-WVT method are taken as the reference, against which estimates from two offline Lagrangian-based methods are compared: the WaterSip and UTrack methods. Both methods are found to exhibit biases in the estimated precipitation sources compared to the reference data set, in particular showing sources to be geographically closer to the precipitation than the more remote sources estimated by the reference. The study then tests a structural modification to each of the WaterSip and UTrack methods and finds bias is reduced and precipitation sources are made geographically closer to those of the WRF-WVT reference. A key conclusion of the study is that the Lagrangian methods can serve as viable alternatives to the more computationally-expensive WRF-WVT method. The study is well-defined, well-written and the conclusions logically follow the results. In particular, the authors are to be commended for detailing the structural differences between the models. The main area of improvement needed is the clarification of the proposed modifications to the Lagrangian models, and their resulting evaluation against the reference dataset.

Thank you very much for your comments, which we think have substantially improved the article. Please, find below the responses to your comments.

Specifically, the modification of the UTrack model appears to contain two changes: (1) only parcels released from above 2km may be used for tracking, and (2) of those parcels, only those with relative humidity above 90% are subsequently tracked. It is unclear which modification dominates the reported changes to precipitation sources relative to the WRF-WVT sources. Of more minor importance, it is unclear why a higher relative humidity threshold is applied to the UTrack model compared to the WaterSip model; this choice of model modification needs to be clarified.

It is true that the proposed modification of UTrack contains two changes, releasing parcels from above 2 km and retaining only those with relative humidity higher than 80 %, and we agree that we did not test each one separately. Because of this, in the revised version of the manuscript we will make it clear that there are two changes, and we will test them in two steps. First, we will apply only the relative humidity filter and evaluate the improvement. After that, we will repeat our experiment by changing the threshold for the release height. This will be reflected in a modified version of Fig. 7, where we add another red dot resulting from applying only the relative humidity filter. Regarding the choice of the relative humidity threshold, there was a typo in the manuscript, as they should all be 80 %.

The modification of the WaterSip model, requiring parcels to have a minimum relative humidity of 80% immediately before a decrease in specific humidity, needs to be explained more clearly. It needs to be made clearer what the exact problem is with the way WaterSip reduces parcel specific humidity en route, and how applying an 80% threshold of relative humidity helps.

The explanation of the WaterSip model in the methodology will be rewritten. Specifically, we will omit some information that can be found in the literature, particularly the explanation of the basic configuration of WaterSip, well documented in Sodemann et al., (2008). We will focus more on different modifications of this diagnostic tool that have been used in previous studies (Fremme and Sodemann, 2019; Dütsch et al., 2018), as they relate to the problem of specific humidity fluctuations that we investigate later. Moreover, in Sect. 3.2.1 we will clarify why these fluctuations may penalize remote contributions (see our response to your specific comment for a more detailed explanation). Under these assumptions, the application of an 80 % threshold of relative humidity helps to address this problem, as it is a simple approach to filter out non-physical specific humidity decreases (not associated with precipitation), and this will also be included in the second paragraph of that section.

Specific comments:

L47: Which problem is being referred to here?

We refer here to the problem of the origin of moisture in ARs. In the revised manuscript we will make it explicit for clarity, by replacing this sentence by “However, the problem of the origin of moisture in ARs is not yet completely closed, as reflected in the definition of AR given in the Glossary of Meteorology, where it is indicated that the sources of moisture can be tropical and/or extratropical (Ralph et al., 2018)”.

L55/60: Here it is asserted that Eulerian approaches are more accurate than Lagrangian approaches. I do not think it is true that, in general, Eulerian tracing approaches are considered to be more reliable than Lagrangian approaches in accurately estimating precipitation sources. Perhaps you mean *online* Eulerian water vapor tracers are considered more accurate? If this is the case, I suggest rephrasing to clarify. Furthermore, if Lagrangian approaches are asserted to contain “more uncertainty”, than these uncertainties need to be outlined. Relatedly, I think it is important to be careful about asserting that WRF-WVTs can be “considered as synthetic observations”. There needs to be some evidence that WRF-WVTs can in fact accurately represent real observations, for example through comparison with satellite observations of atmospheric moisture. If this or a similar type of evaluation has been done, please refer to it here. Otherwise, I would tone down the language by changing the words “considered as synthetic observations” in L63 (also in L436) to “used as a reference”.

We agree with the reviewer that Eulerian approaches are not more accurate than Lagrangian approaches in general. Because of this, we will rewrite this sentence and clarify that it is the online water vapor tracers that we consider to be more accurate. Regarding the uncertainties in Lagrangian models, we agree that we did not go deep enough. In the revised version, we will emphasize that the uncertainty comes from a number of hypotheses and parameters, which are precisely those explored in this study. Finally, we also agree that it is appropriate to soften the language and speak of “reference” instead of synthetic observations when referring to the results of WRF-WVTs, and this will be reflected in the revised manuscript by changing “to be considered as synthetic observations” to “to be considered as reference”.

L145: Is the specific humidity assimilated from ERA5 like the evaporation field? Does the WRF model close the water balance if ERA5 evaporation is assimilated?

No, the specific humidity is not assimilated from ERA5 like the evaporation field. The WRF model closes the water balance in this case, as we are only changing the surface moisture flux simulated by WRF by the surface moisture flux in ERA5, and the other moisture fluxes are updated accordingly by the model itself.

L155: While the manuscript makes it clear that parcel trajectories are calculated using WRF data in the first case, and ERA5 data in the second case, it is a little unclear which dataset was used to calculate the moisture contribution for each Lagrangian model. From reading section 2.3, I interpret that in the first case, "FLEXPART-WRF", WaterSip reads the specific humidity field from WRF, and UTrack reads the precipitable water field from WRF but the evaporation field is ERA5 data assimilated into WRF. In the second case, "FLEXPART-ERA5", I interpret that both WaterSip and UTrack read all fields from ERA5. If this is not the correct interpretation, please clarify.

This is exactly the correct interpretation. To make this clearer, we will explain it better in the last sentences of the first paragraph of Sect. 3: "Finally, in Sect. 3.3 we test the introduced modifications when the trajectories are generated by FLEXPART, with input data from the ERA5 reanalysis. In this case the other fields required by the diagnostic tools are also obtained from the same reanalysis, not from WRF simulations."

L172 & L210: The Dirmeyer and Brubaker approach is also used by other studies, whose moisture tracking method is very similar to UTrack, e.g. Holgate, C. M., J. P. Evans, A. I. J. M. van Dijk, A. J. Pitman, and G. D. Virgilio, 2020: Australian Precipitation Recycling and Evaporative Source Regions. Journal of Climate, 33, 8721–8735, https://doi.org/10.1175/JCLI-D-19-0926.1. Similarly, the WaterSip approach is also used by other studies, e.g. Cheng, T. F., and M. Lu, 2023: Global Lagrangian Tracking of Continental Precipitation Recycling, Footprints, and Cascades. Journal of Climate, https://doi.org/10.1175/JCLI-D-22-0185.1. Though these specific methods are not formerly evaluated here, it would be pertinent to acknowledge them.

We were aware of the problem with the nomenclature for the Dirmeyer and Brubaker, (1999) methodology, but did not know how to solve it. In the revised manuscript, we will acknowledge these other studies and refer to the diagnostic tool as "the Dirmeyer and Brubaker, (1999) methodology" instead of "UTrack".

Figures 3 and 4: it would be helpful to the reader if these figures could be placed side by side for easier comparison. Is it possible to combine the two figures into one?

As both figures refer to different subsections, we do not think it is possible to combine them into one. However, we will ask for them to be placed one after the other.

L230: To make it easier for the reader to interpret the error scores, it would be helpful to add a sentence linking each score with a physical meaning, e.g. a higher value of MAESS refers to a more accurate comparison with the reference dataset.

We agree with the reviewer that we did not make clear the interpretation of the Mean Absolute Error Skill Score (MAESS). In the revised version of the manuscript, we will explicitly mention that a closer value of MAESS to 1 means a more accurate aligning with the reference dataset, by adding "as usual with a skill score, the closer to 1 means that the results of the Lagrangian model are closer to those of WRF-WVTs" once the MAESS is introduced.

L303: To make it clearer to the reader, it would be helpful for the accumulation over time to be shown with a simple example. As the manuscript currently reads, it is unclear what the problem with the WaterSip method is.

To better explain how the error accumulates over time, we will include another iteration of our simple example (a couple of non-physical increases and decreases): "If another non-physical decrease occurs, this value is updated to 1.95(1-0.05/2.0)=1,90 g kg^{-1}". Moreover, we will make explicit that early contributions are more penalized, as they are affected by many more potential non-physical fluctuations, by adding "as the error caused by a single fluctuation affects all previous contributions, so the early moisture uptakes will be affected by many more non-physical changes".

L378: The original configuration of UTrack appears to release parcels from a random, humidity-weighted vertical level, indicating the starting parcel levels will be in the lower part of the troposphere. Yet here, and in Figure 7, it is indicated that the starting parcel level is 0km. Was the starting parcel height set at 0km in this study, or was a random, humidity-weighted vertical level used as in the original model? Further, did this study use a random, humidity-weighted vertical release height and simply ignore those parcels starting below 2km, or was the release height set at a constant 2km level in the modified case?

We agree with the reviewer that it is not clear how parcels are released vertically, and this should be clarified, as it is a key point of the modification we are proposing. In our case, parcels are vertically released following the density profile, using the domain-filling option of FLEXPART. In the case of the Dirmeyer and Brubaker, (1999) methodology, parcels are released following the humidity profile of the atmosphere. Thus, to match our approach with the original one, we need to weight the moisture origins for each parcel using their humidity. This additional (and important) information will be included in the methodology in the revised version of the manuscript. Specifically, we will add a sentence at the end of the first paragraph of Sect. 2.3 to explain how parcels are vertically released in FLEXPART: "In both cases parcels are released using the domain filling option over the black boxes in Fig. 1, such that they are vertically distributed following the density profile". When explaining the Dirmeyer and Brubaker, (1999) methodology, we will also add another sentence for clarification: "since in our simulations parcels are vertically released following the density profile, we weight the contribution of each parcel by its specific humidity to match the Dirmeyer and Brubaker, (1999) methodology".

Regarding the modification we propose, we simply ignore parcels starting below 2 km, being the rest released as usual. This will also be clearer in Sect. 3.2.2 of the revised manuscript, as we will add "particles are released as usual, but those below z_b are excluded from the analysis" when introducing the modification.

L416: Can you provide some reasoning as to why WaterSip is superior to UTrack when using ERA5 data?

This is probably related to the different number of vertical levels (38 for WRF versus 70 for ERA5) and, in particular, to the extent to which the different methods are sensitive to having more or fewer levels, but this is something we do not know for sure and would require further analysis.

L475: The statement that the Lagrangian methods can serve as viable alternatives for WRF-WVTs is a key conclusion of the study. I would suggest including this conclusion in the abstract.

We thank the reviewer for this suggestion. In the revised version of the manuscript, this will be one of the main points of the abstract. Specifically, the last sentence of the revised abstract will be "Although these modifications may need to be adjusted for

other type of precipitation events, our results demonstrate that Lagrangian techniques are a viable and compatible alternative to Eulerian water vapor tracers, and that the main discrepancies between the different methodologies can be derived from the obviation of certain physical considerations."

Technical corrections

Figure 1: it would be helpful if the subplots each had a title describing their geographic location, e.g. "South Africa". These location labels can then be added to Table 1 to make it easier for the reader to associate the numerical description with a real-world location.

This figure will include the corresponding geographic labels in the revised version.

Figure 2: "Tropical Indic" should perhaps be "Tropical Indian" (same issue applies to later figures). Also some parts of the world are classed as "Tropical land" when they are in fact desert regions (e.g. northern and southern Africa, central Australia, Arabian peninsula). To avoid re-running the model with different regions, I suggest touching on the implications of this classification in your results.

We have corrected all figures changing "Indic" by "Indian" where it corresponds. Regarding the classification of desert regions as "Tropical land", we understand that it could lead to confusion if we were to conclude that a certain amount of precipitation comes from these areas, but this is not the case, as we are referring to the source as a whole. In any case, we do not consider it incorrect, since deserts in the tropics are still "tropical lands", so we decided to keep it as it was. Also, note that removing deserts from the sources classified as "tropical lands" would not affect our comparison at all.

L165: Should "Except for the position and the…" be "Except for the position of the parcel and the …"?

The typo has been corrected.

References

Dirmeyer, P. A. and Brubaker, K. L.: Contrasting evaporative moisture sources during the drought of 1988 and the flood of 1993, J. Geophys. Res. Atmospheres, 104, 19383–19397, https://doi.org/10.1029/1999JD900222, 1999.

Dütsch, M., Pfahl, S., Meyer, M., and Wernli, H.: Lagrangian process attribution of isotopic variations in near-surface water vapour in a 30-year regional climate simulation over Europe, Atmospheric Chem. Phys., 18, 1653–1669, https://doi.org/10.5194/acp-18-1653-2018, 2018.

Fremme, A. and Sodemann, H.: The role of land and ocean evaporation on the variability of precipitation in the Yangtze River valley, Hydrol. Earth Syst. Sci., 23, 2525–2540, https://doi.org/10.5194/hess-23-2525-2019, 2019.

Sodemann, H., Schwierz, C., and Wernli, H.: Interannual variability of Greenland winter precipitation sources: Lagrangian moisture diagnostic and North Atlantic Oscillation influence, J. Geophys. Res. Atmospheres, 113, D03107, https://doi.org/10.1029/2007JD008503, 2008