Reviewer 1
Munir Ahmad Nayak

Dear Dr. Louise

Thank you for sending the manuscript to me for a review. Below you will see a short summary of the manuscript, followed by my specific (major) comments, and then technical corrections (minor comments) at the end.

Summary

Tracking moisture in the atmosphere over time has many applications in the fields of hydrology and meteorology, such as finding the major moisture sources of particular extreme precipitation event at a given location. Moisture tracking models can be represented with a variety of schemes, which include Eulerian and Lagrangian (in two and three dimensions) frameworks, different integration time steps, different sets of vertical forcings, representations of vertical wind velocities, locations of moisture releases to the atmosphere, etc. The results of moisture tracking, for example evaporation recycling rate, the distance travelled by moisture, etc., will depend on the scheme chosen. The authors experiment a set of evaporation tracking schemes to asses the sensitivity of tracking results to different schemes. In summary, the steps used in the manuscript can be written as: a.) Select seven point-sources of evaporation across the globe, b.) track the evaporation (during first five days of July 2012) from these locations using a tracking scheme, c.) keep track of precipitation locations, i. e., latitude and longitude points where precipitation happened, d.) repeat the above three steps with different model settings, and e) compare the tracking results. Based on the comparisons among different schemes, the authors propose an "optimal" tracking scheme for general hydrological applications: 3D Lagrangian, 500 particles released per mm evaporation, moisture releases at surface, linear interpolation in time and space, adding as many vertical forcings as possible, etc.

The manuscript is very clearly written, and overall, I think it can be of interest to many readers of HESS and other similar journals. There, however, are some places in the manuscript where authors should provide more justifications and clarifications; I added these in the "specific comments" section below. I hope the authors can address these comments, after which the manuscript may be suitable for publication in HESS. I will be looking forward to reading a revised draft.

Thank you for the encouragements and we are happy to see that the manuscript was clear.

Specific comments

1. When an air parcel moves, it gains and losses moisture along its track, the gain and loss can be attributed directly to the location where the change happens if the parcel is within the boundary layer. When the parcel is out of the boundary layer, the change locations are not clearly evident, since they can come from remote sources (Sodemann et al., 2008), which are difficult to evaluate. It is not clear how the "original" evaporation (Lines 95-96) is maintained throughout the parcel's track. More clarity on this will help in interpreting many results presented in section 3, for example evaporation footprint.

In the current model, no distinction is made between moisture within the boundary layer and that above it. We are treating the allocation in the same way regardless of the current vertical position of the parcel. That means that we are allocating the surface fluxes (and the moisture gains and losses) to all moisture in the atmospheric column, and thus to all parcels present at that location. The "original evaporation" in this case reflects the evaporation at the location and moment the parcel was released, which is probably a different location than the parcel along the track. We then allocate moisture along the parcel's path according to the ratio of P/PW at the current location of the parcel. As stated, this is done independent of vertical position of the parcel, meaning we assume perfect vertical mixing throughout the atmospheric column. We changed the lines around the former lines 95-96 to state that it is about the amount of original evaporation transported with the parcel (lines 104-108 in the document with tracked changes): "However, once there is precipitation at the location of the parcel, a fraction of the moisture (precipitation over precipitable water of the entire atmospheric column, $\frac{P}{PW}$) that is still present in the parcel is allocated to rain out in that location. This assumes that all moisture in the atmospheric column has the same probability of raining out. Thus, the amount of original evaporation remaining decreases with downwind moisture transport.".

Regarding whether the moisture can be attributed to remote or local sources, we actually think that moisture above the boundary layer can be attributed more easily. If the only difference between the boundary layer (BL) and free troposphere (FT) is the mixing (strong in the BL, not so strong in the FT), the source regions of parcels in the free troposphere can be more easily determined because the parcels will move with the large-scale winds. In the boundary layer, there is more mixing and therefore more vertical displacement of parcels. Given that this study shows that atmospheric moisture transport can be quite sensitive to vertical displacement assumptions, we could assume that it is more difficult to pinpoint the original moisture source of boundary layer moisture compared to free tropospheric moisture. Nevertheless, we did not look at the difference between boundary layer and non-boundary layer moisture here, but consider it worthwhile for future research to test explicitly what the sensitivities of moisture recycling to boundary layer and non-boundary layer vertical mixing are.

2. Section 2.1.4: During convective up- and down drafts, horizontal winds also show significant changes in magnitude and direction; the particles can then be displaced vertical depending on the changes in the vertical winds, instead of assigning random vertical displacements to them, which seems arbitrary. If feasible, another scheme based on this large horizontal wind gradients may be added in the present framework.

Thank you for this suggestion. We agree that the unconditional mixing scheme currently employed is a simplification of reality. As suggested, large-scale conditions may influence the vertical mixing rate. Furthermore, it is possible to use the convective mass fluxes to determine local vertical mixing rates, as we did in Staal et al. (2018, *Nature Climate Change* 8:539-543). As the goal of the current work is to test the sensitivity of different kinds of assumption on moisture tracking, we have chosen to limit the analyses to the current mixing assumptions of four mixing strengths that all happen regardless of atmospheric conditions. For future work, a sensitivity analysis of more physical vertical displacements would be relevant.

3. The basic structure of the model is not presented anywhere. I suggest adding a stepwise

procedure on how the tracking is performed. Actually, response to this might answer my first comment 1 also.

We have rewritten the model description section (2.1) and presented the moisture tracking procedure in a more stepwise way (lines 98-112). Furthermore, we restructured the Methods section with a more logical order of presentation of model structure and assumptions, and experiments.

4. This baseline model is 3D Lagrangian with 10,000 parcels released per mm; the 3D model in L243, table 1, and other results almost identical to the baseline model. This does not seem a reasonable way to compare models and present results, since baseline itself is not "True Tracking" and cannot be a perfect reference. It might be a good idea to use other models' output as reference, such as HYSPLIT (Draxler and Hess, 1998), LAGRANTO (Wernli, H., and H. C. Davies, 1997).

"True tracking" is, of course, impossible. However, a three-dimensional Lagrangian scheme with an extremely large amount of parcels (10,000 for each mm evaporation) uses the available information in the most elaborate way. That is, no information is lost. For this reason, we considered that the best possible baseline. We chose not to compare our results with those form other models in the literature, which were generally developed for ERA-Interim data. Rather, our aim is to test how the ERA5 data can be used best for moisture tracking purposes. Revisiting existing models would address different questions. We prefer to remain within our original scope and not use other models as reference.

5. In Section 3.2, it is argued that number of parcels released does not affect tracking results greatly. We should note, however, that number of parcels may matter to capture convective/converging and diverging events, as stated by the authors in section 2.2.4. Here, the simulations are run only for one case (July 2012), which may not have large convergence or divergence at any time. We should be careful in generalizing these results to all events, unless simulations results of some specific convective events show similar results.

We agree that we must be careful in generalizing our results. Because of possible biases resulting from using only one case, we did our experiments for seven locations spread around the globe. Indeed, some locations were more sensitive to certain changes in model settings than others. However, in general the results were robust. Apart from these seven cases we published our code including options to change the settings according to our methods. Thus, users of the model can perform their own sensitivity analysis if they want to, or change the settings to suit specific research questions. We added in lines 533-534 that we tracked only for one month the moisture released during five days is an additional reason for caution in generalizing our results.

Technical corrections

Define "footprints" at the beginning, somewhere in the introduction.

Thank you for spotting this. We now define "footprint" as "the distribution of precipitation resulting from evaporation from a point or area" in line 50.

One of the aims of the manuscript was to evaluate model structure; however, it is not clear where model structures have changed. Perhaps, Eulerian and Lagranian can be taken as different model structures, but this needs to be written explicitly.

Thank you for spotting this. We now define model structure as "Eulerian or Lagrangian and the number of spatial dimensions" in lines 76-77.

L25-26: Fig. 1 does not specifically show moisture recycling as indicated here.

We removed the reference to Figure 1 in this sentence.

L46-48: Rather than "assumptions", I feel they are more like user "choices".

We agree and rephrased to "choices" in these lines.

L44: Here, I suggest writing "parcels" instead of "particles".

We replaced "particle" with "parcel" throughout the manuscript.

L51-54: It is not clear how the results will be incorrect; also, clearly explain why the Eulerian model simulations will not be as fast as Lagrangian when moisture is released from small areas.

We moved the explanation of why the results will be incorrect from the methods to the introduction in lines 54-56: "If the time step is chosen too large, real moisture transport may occur faster than the simulation grid and time step allow for (i.e., if the Courant number $C = \frac{v\Delta t}{\Delta x} > 1$). If the time step is taken too small, numerical diffusion will occur, meaning that moisture transport in the model will be faster than in the forcing data."

To explain why Eulerian models will not be as fast as Lagrangian for small areas we added "The reason is that they are insensitive to an increase in scale, as all grid cells are updated with the same speed regardless of the amount of moisture present." in lines 58-59.

L60: Do you mean "which resulted in Courant numbers exceeding one …"?

We removed this part of the sentence, because it was unnecessarily complicating.

L125: I am not sure if I understand why vertical mixing is to be carried out every time interval and how is it performed; more details on this can help readers.

The vertical mixing is carried out every time step in the sense that every time step, a random number [0-1] is determined. If this number is smaller than dt/mixing_rate_hour (dt is the internal time step, mixing_rate_hour is the mixing strength in terms of how many times vertical mixing happens on average, in our case 1 hour, 6 hours, 24 hours and 120 hours), the random vertical displacement occurs. By carrying out the mixing procedure (including random number assessment) every time step, two things are achieved:

1. Mixing happens on average once per mixing_rate_hour hours (so once every 1, 6, 24 or 120 hours);
2. The mixing happens at random moments throughout the trajectory, so there is no bias regarding to mixing at specific moments.

We have added some additional description in section 2.1.8 (lines 255-259): "During every time step, there is a small probability (dt/mix-strength) of running the vertical displacement. We summarize these stochastic vertical displacement versions of the model by the mix-strength (unit: hours), or average time for one repositioning of one parcel, which is once per hour, once per six hours, once per 24 hours and once per 120 hours. This procedure ensures that for each parcel, mixing happens on average once in the time period described by the mixing strength and that the mixing happens at random moments during the trajectory. Thus, no biases occur due to mixing at specific prescribed moments.".

L130-L133: Rephrase for more clarity.

We have added units to the data used and remove the "division by the grid cell length" part, which may have been confusing. Now, the section states that the 2D flow speed is driven by the vertically integrated flow speed. Of course the grid cell length is still relevant for the calculation, but as it is not relevant for the 3D vs 2D discussion, we removed it here.

L150: Do you mean "particle" instead of "parcel"? Try to be consistent.

We now consistently refer to parcels.

L153: No, this does not seem realistic; you might not be able to capture convergence or divergence with this scheme, just because it is random.

We agree that it was too strongly phrased. We removed this sentence.

L178: Here, 10,000 particles are released per mm of evaporation over first 5 days of July 2012? Evaporation from a point source at any instant will be transported during each time step; are we releasing parcels at one instant, say t=0, or over multiple time steps (t =0, t=1, and so on.). Add a few lines to clearly explain how parcels are released, and how evaporation over 5 days will be captured by parcels released.

Yes, 10,000 parcels are released for every mm of evaporation during those five days. We added "All evaporation within this period is accounted for" in lines 164-165.

L189: In table 1: I would also add a simple metric "mean distance travelled".

Thank you for this suggestion. When we ran our simulations, unfortunately, we did not record the distance that the parcels travelled through the atmosphere. We did, however, record the distances between the source locations and those of the sink locations. This is already present in Table 1 (mean latitudinal distance and mean longitudinal distance).

L225-234: The entire section can be as a separate row in Table 1.

Thank you for this suggestion. We added new rows to Table 1 with the results presented in section 3.1, which are averages of the data that were already provided.

L250: Low value of CRR is observed in 2D Lagrangian case, not Eulerian; see Figure 3c also

Thank you for spotting this. We meant the high value of CRR in the 2D Eulerian case and corrected the sentence accordingly.

L254: Fig. S5, not S4.

Thank you for spotting this. We corrected it.

L259-262: Give clear reasons of so much computational time difference between 3D Lagrangian and 3D Eulerian schemes. In other words, why do we think 3-D Eulerian takes so much time?

The reason is that Eulerian models update the moisture content of all grid cells, even when the actual tracked moisture is in just a part of it. This information is added in lines 58-59.

Figures 6, 7 and other similar figures: Since these figures do not shown clear differences, perhaps it is better to show differences directly, i.e., map of baseline footprint minus footprint from the given set up.

We appreciate this suggestion, but showing differences would serve another purpose than ours. Lack of difference among simulations is—in our case—a relevant result. If we chose to display the differences (only) in the cases where they are very small, the collection of figures would highlight the model settings to which the model is insensitive to rather than the ones that it is sensitive to.

Figure S25B: There is an unusual straight color line in this panel; can that be removed?

Thank you for spotting this. This straight line was due to an error in the plotting. The ERA5 data (and our model) runs on longitudes from 0-360, while Matplotlib expects longitudes from -180-180. We made an error shifting grids, which we now corrected.

Section 3.5: Were the models run with interpolation or with nearest neighbor method. Also, the results here can be concisely presented in a tabular form, rather than text.

As explained in section 2.2.4, all Lagrangian runs were forced by interpolated data, unless stated otherwise, in which case the nearest neighbor was used. Thus, mentioned section uses interpolation.

Section 3.6: Will it be feasible to test sensitivity to timestep dt = 3 hours?

Yes, it is. We expanded our sensitivity analysis to time steps, now including dt = 3h and dt = 6h. The respective figures in the main text and the supplement were updated with two new panels. Also the Methods (section 2.2.7) and the Results (section 3.7) were updated accordingly. Note that time steps >1h imply a degraded temporal resolution compared to the forcing data. We added this information in lines 233-234: "… 3 h, and 6 h. Note that the latter two imply a degradation of the temporal resolution. For these cases we averaged hourly data on wind speed and direction." The additional tests did not change the settings of our optimal model.

Section 3.7: I am not sure if I clearly understand the purpose of mixing and its usefulness in practice. Perhaps provide more details.

The purpose of the vertical mixing is to approximate the role of turbulence in atmospheric moisture transport. Turbulence may be a very important driver of mixing, but this is not covered by reanalysis data. Accounting for vertical mixing as we do may compensate for that. We clarified this in lines 388-389: "Turbulence may cause considerable vertical mixing in the atmosphere, but because the rate of this mixing is unknown, …".

References

1. Sodemann, H., C. Schwierz, and H. Wernli, 2008: Interannual variability of Greenland winter precipitation sources: Lagrangian moisture diagnostic and North Atlantic Oscillation influence. *J. Geophys. Res.*, **113**, D03107.
2. R. Draxler, R., and G. Hess, 1998: An overview of the HYSPLIT_4 modelling system for trajectories. *Aust. Meteorol. Mag.*, **47**.
3. Wernli, H., and H. C. Davies, 1997: A lagrangian-based analysis of extratropical cyclones. I: The method and some applications. Quart. J. Roy. Meteor. Soc., 123, 467–