**Answer to Anonymous Referee (R1) in the interactive comment.**

I found the study interesting, with a clear experimental setup and straight-forward analysis. The writing needs to be improved, I have some suggestions, but I recommend further proof-reading. I have some concerns regarding the explanation of the results and the overall conclusions of the manuscript so I recommend Major Revisions.

Thank you very much for your review and your time. We are confident that the modifications you suggest will improve the manuscript. Please find below the detailed responses to your comments.

Main concerns

My main concern is that the authors limit themselves to highlighting the deficiencies in the Lagrangian methods with little effort to propose improvements. In the abstract they state that the deficiencies are related to phase change, but the results do not provide enough evidence to support this statement. How did you come to the conclusion that phase change is the main problem? Can you quantify this? How would you propose to improve the Lagrangian methods to incorporate phase change?

In the introduction they state “the present work is intended to contribute to improving the Lagrangian analysis” but currently the authors mainly highlight deficiencies.

As such, the last paragraph in the manuscript falls short of conveying a way forward to improve the science.

We fully agree with the reviewer that our assumption that a large part of the errors found came from phase changes was based on weak arguments. The problem of omitting phase changes is well known and was argued already in Stohl and James 2004. In the cases studied, especially in the October case, we thought it made sense that the unrealistic values found in the Sahara were due to this fact, since we understood that, with cloud cover over this region in the days leading up to the event, it was very likely that some of the liquid water had evaporated on contact with the very dry Saharan boundary layer.

Motivated by the reviewer's comment, and also by Ruud van der Ent'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. Van der Ent et al. (2013) came to this same conclusion using a completely different method.

Therefore, our hypothesis was wrong, so we have decided to make a major revision of the article. In this new version, moisture phase changes are only discussed in section 3.3, where we show that their contribution to the errors found is small (see new Figure 9). Sentences such as "We argue that such an inconsistent contribution is associated with the fact that the Lagrangian method does not consider moisture phase changes" have therefore been removed. Instead, other possible errors have been discussed, such as those related to the convergence and divergence of humidity (see Figure 10).

Finally, we agree that our study does not offer any explicit improvement of the Lagrangian technique used, so the title of section 3.3 has been changed to "Limitations of Lagrangian analysis and possible causes". However, we have decided to keep the sentence “the present work is intended to contribute to improving the Lagrangian analysis”, since we believe that our study, although it does not offer an improvement of the model code, can improve the interpretation of the results provided by the model.

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.

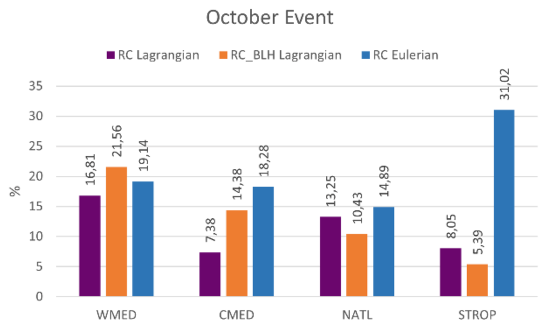
van der Ent, R. J., Tuinenburg, O. A., Knoche, H. R., Kunstmann, H. and Savenije, H. H. G.: Should we use a simple or complex model for moisture recycling and atmospheric moisture tracking?, Hydrol. Earth Syst. Sci., 17(12), 4869–4884, doi:10.5194/hess-17-4869-2013, 2013.

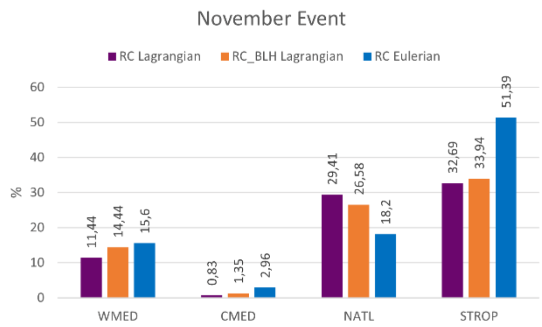
The main results of the Insua-Costa et al. 2018 study should be much clearer. The simulation setup, length of simulation, boundary conditions, horizontal resolution and main results should appear in a paragraph on their own before showing the results of the Lagrangian analysis.

Some of this information was already contained in the second paragraph of section 2.2 (Eulerian approach). However, following the reviewer's suggestion to include more detailed information, we have also added the boundary conditions and parameterizations used by Insua-Costa et al. 2018. The information related to the parameterizations has been included in a new table, in which we summarize the main features of the WRF-WVT and FLEXPART-WRF models.

Table 1 is arguably the most important result however, it seems insufficient to make the argument. It would be good to include a graphical display of results. Also, can you represent the results as a time series? Would this give additional insight?

Thank you for the suggestion. We have now replaced Table 1 with the following bar chart:




However, we believe that it is not possible to compare the results provided by the Eulerian and Lagrangian models using a time series, since the basis of each methodology is different. Specifically, the Eulerian method is not useful for creating plots similar to those in Figures 4 and 7; that is, the Eulerian method tells us where the moisture came from but tells us nothing about how many days earlier that moisture evaporated.

I was confused about the results from RC Lagrangian and RC_BLH Lagrangian. Do the latter (RC_BLH Lagrangian) use the method of Sodemann et al. 2008? If so, please clarify when you are discussing the results. Also, when you discuss the results, this is left to a last paragraph. However, it seems best to discuss the three methods together. RC and RC_BLH are very similar techniques, so it doesn't make sense to discuss them separately.

Both ratios are estimated considering the method of Sodemann et al. 2008. At the reviewer's suggestion, the definition of RC_BLH has been added to the methodology section together with the definition of RC. However, we believe it is better to discuss them separately in the text. This is because RC_BLH is calculated as an attempt to reduce the shortcomings of the Lagrangian method, so it makes more sense for it to appear in the "Limitations of the Lagrangian analysis and possible causes" section.

I think there needs to be more detailed explanation in some cases. In figure 8, what methods did you use? I am guessing these are RC and RC_BLH, but I am not sure. The same with Figure 9, what methods are you using? what is the exact domain of analysis? It is unclear to me which lines correspond to which axes.

Both figures have been eliminated in this new version. Figure 8 was eliminated at the suggestion of the second reviewer. Figure 9 was eliminated because we considered that it was no longer meaningful, since we showed that the contribution of moisture phase changes to the unrealistic values found was very small. Two new figures have been included to replace them.

Abstract Line 6: You state that these methods are "complex". Compared to what? Please read and include the following paper that will help you justify classification of the models by complexity and show another example of using WRF with water vapor tracers as the "truth" to improve other models.

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

We have removed the word "complex" in the abstract, as suggested by the reviewer. In addition, we have added the reference to Domínguez et al. (2020) in the Introduction.

Please reference Figure 1 and explain clearly in the text what it depicts.

A clearer explanation of figures 1 (a) and (b) has been included in section 2.2 (Eulerian approach). Figure 1 (c) had already been explained in section 2.1 (Lagrangian approach).

Line 277: “Positive E values in these areas…” Doesn't this contradict the main finding that the Lagrangian technique is particularly bad for remote sources?

We do not understand why the reviewer relates this sentence to the conclusion that the Lagrangian technique is especially bad for remote sources. Here we only want to show that positive E-field values near the rain-affected area are more likely to show a moisture source region than if the positive E values were much farther away. This is because, as discussed in the paper, a moisture gain (E>0) in a very distant region is likely to be lost along the way before the air parcel gaining this moisture reaches the Mediterranean. In any case, the E-field maps have been removed from the paper at the suggestion of the second reviewer, so this discussion is no longer included in the article.

Line 325: “The two most used techniques…” This is not really true. There are MANY studies using analytical methods. Please see the Dominguez et al. 2020 reference.

Following the reviewer's suggestion, the sentence “The two most used techniques for the study of the moisture origin are the Lagrangian and Eulerian models” has been replaced by “Two of the most used techniques for the study of the moisture origin are the Lagrangian and Eulerian models”. The reference to Dominguez et al. 2020 has been included in the introduction.

Minor Issues

All minor corrections have been introduced in the text as suggested by the reviewer (see version of the manuscript with changes marked). The only exception is the comment concerning line 9 of the abstract, which has not been included since we believe it is better to keep the enumeration.