

Comment on hess-2020-651

Anonymous Referee #1

Referee comment on "Extreme precipitation events in the Mediterranean area: contrasting two different models for moisture source identification" by Sara Cloux et al., Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2020-651-RC1>, 2021

In this work the authors compare the results of two moisture tracking methods to evaluate the sources of two extreme precipitation events that affected the Mediterranean area. A previous study analyzed these events using a Eulerian method while this study compares the results of the previous study (which they take as the truth) with those of a Lagrangian method. The authors find significant differences in quantification of remote moisture sources and terrestrial moisture sources from northern Africa. They recommend using the Lagrangian method to qualitatively estimate moisture sources.

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.

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.

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.

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?

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.

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.

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

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

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?

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.

Minor Issues

Abstract Line 7: Throughout the text please avoid words such as "infamous"

Abstract Line 9: perhaps change to "of moisture evaporated from the Western Mediterranean, Central Mediterranean, North Atlantic ocean and tropical"...no need to enumerate.

Abstract Line 10: change to "Africa. In this work we use..."

Abstract Line 16: " six times lower than the Eulerian..."

Line 26: "receiving large rain accumulations..."

Line 28: "Several characteristics make this part of..."

Line 29: "Mediterranean Sea is a large..."

Line 30: remove "are some examples"

Line 34" While HPEs are a regional..."

Line 37: please also add the following reference by Hu and Dominguez:

Hu, H., F. Dominguez, 2019: Understanding the Role of Tropical Moisture in Atmospheric Rivers, J. Geophys. Res.

- Atmos.,124 13,826-13,842. <https://doi.org/10.1029/2019JD030867>

Line 42: "Lagrangian models being the most widely used..."

Line 46: "However, they are considered to be..."

Line 49:"Eulerian approaches have only been"

Line 53: "obtained using this tool have already been.."

Line 63: "short-distance sources as well as long-distance..."

Line 124: "11-day period can experience different gains..."

Line 143: "This allows the detailed estimation of the relative

Line 170: "Only those particles that experience a significant..."

Line 228: "The tracers in the Eulerian..."

Line 259: "Lagrangian methodology is limited for"...

Line 291: "Latitude 23N northwards). During the first days considered, that is, between 5 to 11 days prior...."

Line 329: "limitations of these methods.."

Line 336: I don't really agree with this statement, you have to be more specific.

