

Answer to Obbe Tuinenburg in the Interactive comment on “Extreme precipitation events in the Mediterranean area: contrasting two Lagrangian and Eulerian models for moisture source identification” by Sara Cloux et al.

I have read and assessed the revised manuscript. While the manuscript has improved, I still have some criticism on the current manuscript that should be addressed before publication.

Thank you very much for your review. We believe that the modifications you suggest will improve the manuscript. Please, find below the responses to your comments.

1. I am still unconvinced by the experimental setup, table 1 makes this clearer; there are many differences between the two models tested that could all lead to the differences found. Nevertheless, the authors all attribute this to the difference in model structure (Lagrangian vs Eulerian). My suggestion would be to do an experiment in which only one aspect of the model setup is changed to test the effects of individual aspects.

We understand the reviewer's concern on this point. However, to our knowledge, the proposed experiment is totally inaccessible. As the reviewer says, there are many differences between the two methods used and not all of them are related to the structure of the model. For example, table 1 specifies that one of the models takes phase changes into account and the other does not, which has nothing to do with the model structure. However, it is impossible to isolate some individual aspects of the model to assess their effect. In other words, it is not possible to match the two columns of table 1, except for the first row. For example, the Lagrangian methodology used inherently works in a backward direction, and it is impossible to match it to the Eulerian methodology. Even if such an experiment were possible, we are not sure it would be necessary; here we are comparing a recently coupled Eulerian tagging tool to the WRF model against a widely used Lagrangian approach, and we are interested in the overall differences (with all their aspects) between the two models. A similar comparison has been made by other authors in the past (van der Ent et al., 2013).

We consider that there is not really a problem with the experimental set-up, but a problem of language. Perhaps we have overused the terms Lagrangian and Eulerian to refer to the methods used. As the reviewer points out in the following comment, there are many different Lagrangian methods, and we should have used a more concrete vocabulary. We have tried to correct this point (see next reply), and in this way we hope to make it clear in the text that the differences found need not be exclusively due to the difference in model structure.

Van der Ent, R. J., et al. "Should we use a simple or complex model for moisture recycling and atmospheric moisture tracking?." *Hydrology and Earth System Sciences* 17.12 (2013): 4869-4884.

2. In many parts of the text, the results are still generalized to Lagrangian models or methodologies. This is problematic, as it gives the impression that the problems you diagnose are valid for all Lagrangian methods. However, many Lagrangian approaches have completely different assumptions. This was also indicated in the previous review, and by other reviewers. I suggest to change all mentions of Lagrangian model(s) to FLEXPART-WRF.

In the previous revision, we had already tried to solve this problem by adding the following clarification at the beginning of the Methods section: "so from now on, when we refer to the Lagrangian or Eulerian approach, we will be referring to the specific methodology explained here". However, we agree that in some parts of the text this generalisation may still confuse

the reader. Therefore, following the referee's suggestion, we have used the term FLEXPART-WRF instead of Lagrangian model when we refer to the tool used. In addition, we have differentiated between FLEXPART-WRF, E-P balance and the Sodemann et al. (2008) methodology (see second paragraph of Section 2.1); FLEXPART-WRF deals with the dynamical part (particle trajectories) while the E-P and Sodemann et al. (2008) methods deal with the hydrological part (moisture sources) of our analysis. Nonetheless, the use of the term "Lagrangian" has been retained in some parts of the text where it is considered necessary and not misleading.

3. The Figure 10 that is now added to the text is nice, and the simulation of convergence and divergence is problematic if you consider only a limited number of parcels. In our paper on the sensitivity assumptions of moisture tracking models

(<https://hess.copernicus.org/articles/24/2419/2020/> (Tuinenburg and Staal, 2020)), we discuss the effect of the number of parcels on the simulation quality. You could have had a look at that and used it in your discussion around Figure 10.

Thank you for your recommendation. We have included this reference as part of the discussion of Figure 10.

4. The problem with the E-P balance is not addressed properly to my liking. The authors have added to the results section that the positive E-P values based analysis is not entirely certain (L220-230 of the track-changes manuscript). Given the large fractions of moisture not taken into account by taking the assumptions (see values in my earlier review), I would have expected some clear language.

To clarify this point we have added the following sentence in the middle of paragraph L220-230: "a preliminary analysis shows that a significant fraction of the total evaporation occurs when $E-P < 0$ (see <https://doi.org/10.5194/hess-2020-651-RC2>)". We have taken the liberty of referring directly to the previous reviewer's comment because we think it is the easiest way for the reader to understand the concept we are trying to convey here.

Answer to Ruud van der Ent in the Interactive comment on “Extreme precipitation events in the Mediterranean area: contrasting two Lagrangian and Eulerian models for moisture source identification” by Sara Cloux et al.

I apologize for submitting this review only on the last day of the deadline. I think the authors did an outstanding job in improving this paper.

We thank the reviewer again for his positive comments.

I thank the authors for giving reference to my ideas about convergence/divergence as possible sources of error for the Lagrangian tracking in combination with moisture source identification based on E-P. However, instead of "personal communication" it would be more traceable if they cited the comment on the original manuscript instead: <https://doi.org/10.5194/hess-2020-651-CC1>

We have added the reference to the previous comment as suggested by the reviewer.