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Comment on hess-2020-651

Obbe Tuinenburg (Referee)

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-RC2, 2021

Dear Daniel Viviroli,

I have read and assessed the manuscript. Although I like the topic of the study, I am a bit worried about the experimental set-up, because it is unclear to me which experiments are compared. Furthermore, I am concerned about the assumptions about the atmospheric moisture budget taken in the Lagrangian moisture tracking model. I realize that these assumptions have been taken in many previous studies, but I think the they impact the conclusions significantly.

Kind regards,
Obbe Tuinenburg

- Some of the details of the experiment are unclear to me. I believe this experiment is comparing online Eulerian to off-line Lagrangian methods, which is not entirely fair. I would recommend doing the experiment with all methods in an online mode and all methods in an offline mode, so their differences can be more m
eaningfully interpreted. Furthermore, a lot of details about the model settings are not included and these may be important, see our work on the assumptions influence moisture tracking models: https://hess.copernicus.org/articles/24/2419/2020/ (Tuinenburg and Staal, 2020)

- I am worried about the assumptions regarding the atmospheric moisture budget that are used in this study, but have been used in a lot of similar studies using FLEXPART. The main idea in this model is that a change in the atmospheric precipitable water along a trajectory is allocated to the total water budget at the surface (E-P), rather than its individual components E and P. As far as I can retrace, this assumption stems from the paper by Stohl (2004) on FLEXPART. It seems to be an assumption that is convenient from the atmospheric moisture budget perspective, but it becomes problematic when you actually want to allocate chan
ges in atmospheric moisture to either E or P. I assume this approximation was warranted in the time when FLEXPART was developed when the surface fluxes (and especially E) where very model dependent and frequently used to reduce the near-surface biases of the

model wrt observations. As a result, evaporation estimates
were frequently unrealistic or unphysical. I would argue that at present, the surface fluxes are estimated a lot more reliably and therefore I wonder why the fluxes are not used directly, but rather the method still relies on using the total budget. I think this practice creates significant biases in moisture allocat
ion.

Specifically on L92. For situations where E-P<0, E is assumed to be zero. I had a look at the ERA5 data to check how well this assumption holds for the domain and days considered (36N-48N, 10W-8E, over 19-21 Oct 1982 and 6-8 Nov 1982). As the authors did, I aggregated the data to 3-hourly means (from the hourly ERA5 resolution). The fraction of evaporation that occurs when E-P<0 is about 32% of the total evaporation for the domain (globally this is about 16% for these days).

Regarding the assumption of the precipitation events, on L103, only moisture is allocated when E-P < -2 mm per 3h. Again, I had a look at the ERA5 data for the domain and days. The precipitation events for which the condition is true only represent 75% of the precipitation for the domain (globally this is only 47% for the days considered).

I think that these fractions of the evaporation and precipitation events missed is significant. Depending on how these E and P events are distributed compared to the cases studied here, the results will probably be affected quite a bit. (I realize the present study is done based on WRF simulation, which are different than the ERA5 reanalysis, but I would not expect these fractions to be very different.)

L 59-60: This was not the main conclusion of Van der Ent 2013. The main conclusion of that paper was that both methods have differences in moisture flow representation compared to the on-line tracking. The main problems occur in locations with a lot of vertical variability in the horizontal integrated moisture flow, such as monsoon areas with strong flows at the surface and return flows higher up in the atmosphere. The Eulerian model in that study initially considered the vertical integral of the horizontal moisture flow, leading to underestimation of horizontal moisture flow in situations where the flows are opposite in the lower and upper atmospheric levels (thus cancelling each other). Based on this study (and a lot of other work), the WAM model was adapted to use two layers in the vertical.

L360: How exactly was the ERA-Interim data used in the present study? I did not find any other mention in the manuscript rather than in the acknowledgements.