

Interactive comment on “Unsaturated zone model complexity for the assimilation of evapotranspiration rates in groundwater modeling” by Simone Gelsinari et al.

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This paper aims at assimilating evapotranspiration data from MODIS over a 5 x 1 kmsq area. Water table dynamics are linked with ET because of vegetation access to the groundwater, thus, the authors test if the assimilation of ET data (available via satellite data) can help improving water table dynamics (not observable directly via satellite data).

I think the paper is well written and of interest to the HESD readership. My main concern is related to the robustness of the main conclusion related to the assimilation part. I am a bit doubtful about the significance of the authors results. Yes, the results show

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evidence that the assimilation of ET improves WT dynamics, but the authors should test for the significance of these results. In fact, the improvements reported in table 2 and 3 seem very marginal and small. I would like to see confidence intervals added to the calculated RMSE and r so that the authors can conclude whether their approach lead to significant improvements or not. Please reduce the strengths of statements like those in lines 373-376 or lines 402-405

Also, if I understand correctly, the RMSE and r statistics for ET are calculated against the same data that are assimilated, correct? If so, I would have expected the verification statistics of the assimilation to improve much more, but the improvements are marginal. Can the author comment on this?

Line by line comments: Line 94: “The area was originally planted”. What was planted? The area or trees? Please reword. What is the influence of the sea level to the ground-water level? (Figure 1 indicates that the test domain is located near the coast) Line 138: Add reference to the section where you explain the coupling. Line 180-181 and 268-269. What do you mean by “ET WT link”? Please explain. Further, how do we see that the “link is reproduced” in figure 4? I have a hard time to see a clear relationship between ET and WT in figure 4. Table 1 (and in text) is perturbation fraction referred to the coefficient of variation? If so, I'd replace it with coefficient of variation which is more commonly used term in statistics? Section 2.4.1. Table 1 reports some perturbation numbers, but the reader is referred to Gelsinari et al., 2020 for the ensemble generation. I recommend adding a list/table of all perturbed parameters/meteorological inputs/prognostic states to this article too. Line 200: I think there are other algorithms that work better in highly non linear system (e.g. particle filters) so I'd remove this as a reason for choosing the EnKF. Line 230. Please add the update equation to this article so that the reader does not have to go back to Gelsinari et al., 2020 to see it. Line 231: What do you mean “limited”. Please reword and clarify in the article. Line 249: What ensemble verification skills do you use? I think it is important to expand this part, especially since you refer to it later in the article (line 305) Line 257: ... assimilation

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results and to the respective Line 271: config. 1 temporal dynamics is not always lower. What happen in 2005? Line 278: indicate the blurred area in the figure too so that the reader knows what you are referring to. Line 288-289: replace seasons with months. Figure 6, 7. Please darken the ensemble replicates. I can barely see them on my screen. Line 318: add figure reference: e.g.: “(see panel b in Figure 7)” or “(see Figure 7b)”. Line 327-328: Can you be more explicit in explaining why the reduction in ET errors suggests improved state variables? From your table 3, some of these states degrade even if ET improves. Figure 8 and Figure 9: what is the cloud of points in the openloop and assimilation? I assume these are all the ensemble member at the given time step. If so, why do they have a x-axis dimension on the bottom plot?

Best of luck! -Manuela Giroto

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