

***Interactive comment on “Global 5-km resolution estimates of secondary evaporation including irrigation through satellite data assimilation” by Albert I. J. M. van Dijk et al.***

**Anonymous Referee #1**

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I really enjoyed reading this paper and find that it has valuable contributions towards a global irrigation demand estimate. It provides an interesting new perspective on obtaining global irrigation estimates. However, while I was reading the manuscript I did have several questions and concerns. Please find below the major and minor comments I have after reading the manuscript.

Major comments: I find that the introduction is somewhat incorrect and building up to something that is not really happening in the objective of the paper. The authors mention how all kind of modelling efforts will not produce independent and accurate estimates of irrigation water demand, but after the reading the objective I can only

C1

conclude that they will themselves do modelling as well. I think it would be good to refocus the introduction and state that models can have a valuable contribution but have their limitations and highlight how the authors would like to resolve these limitations.

Line 123-139 In the introduction it is stated that most models do not include lateral water flow, but then again, the others already give some model example where lateral flow is included. Arguably the lateral flow in the routing is not included, but that is also the case for the model used in this study. I do not fully understand why this model is different from the other global water balance models out there. They authors should do a better job to highlight this, to emphasize why this model is better suited for this excursive than others

Line 125 -126 The quality of the forcing data is really low, how do the authors think this will impact the simulations and consequently the evaporation estimates?

Line 184-185 and Figure 1, I have the strong feeling that the model is biased in its estimates of  $E$ . Therefore, this would violate the basic assumption of a normal distribution with a mean of 0 around the observations. In addition, the authors cutoff the  $E'$  updates, which is in my opinion another violation of the EnKF. I feel the others should make sure that the model is bias free before implementing a DA technique like the EnKF. Otherwise, they can show the global biases to convince the reader that this is only the case for Figure 1, but I have to strong suspicion that it is also a problem for other regions (as for most models). I think the authors should address this large limitation in their discussion or somewhere else in the manuscript.

The manuscript could significantly benefit from a flowchart describing the full updating, calibration, nudging and assimilation procedure. Which variable are subject to what and where and how? The manuscript is difficult to follow without.

Line 253-254 Why is the increase in the estimation evaporation not from missing model processes? Incorrect vegetation parameterization or something else. This assumption is vital for the manuscript and is not really supported by argument on the model's quality

C2

to estimate evaporation in general. Has the model been validated against independent evaporation estimates?

In addition, to the previous comment, the authors have not mention other forms of water use. I see no inclusion of domestic or industrial water use in the model nor in the estimates? Maybe these abstractions cause the errors in water basin closures. Can the authors exclude this and/or provide estimates of these quantities for the river basins? This would inform the reader if irrigation evaporation has a real significant contribution or if it is simply correcting for the fact that other sources of water use are missing in the model

Minor comments: Line 129-134 are the calibrated parameter spatially consistent or are they really tuned to the individual basins?

Line 134-135 Does the model have any lateral flow simulations of groundwater or surface water?

Line 150 a nudging factor of 0.99 is rather high, does this mean that the model is almost always wrong?

Line 156-159 what is the spatial resolution of the Tair forcing, since it is very important for the LST simulations

Line 177 15degree, does this mean that the LST is spatially average over a 1500 by 1500km area???

Line 508-510 the true error can also be larger. . . It is not said that it will be smaller due to the representativeness error.

Line 581-583 As far as I understand most other models use sub-grid parameterization, which would allow for a partial coverage of the grid cell by irrigation areas. This statement is therefore potentially incorrect and should be removed to avoid misinforming the reader

C3

Line 619-623 I feel the units are incorrect, I guess the first estimates should be  $75.5 \cdot 10^{12} \text{ Km}^3 \text{ y}^{-1}$  (as well as for the other estimates from this study, which are now 1000 times lower than other studies)

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C4