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Discussions

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.

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We thank the reviewer for their comments and are glad that they enjoyed the m/s. Below we address the issues raised.

"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 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."

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It is certainly true that our approach requires a model to assimilate the satellite observations into, and it is also true that additional assumptions are needed to translate water use estimates to irrigation water demand. We meant to emphasise that our method differs from existing methods in that it does not require mapping of the area irrigated or the extent of wetlands to then explicitly simulate secondary evaporation. Instead it is inferred from the observations. In revising, we will look for some textual changes to avoid the wrong impression.

"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"

The overall approach is different from existing models in that secondary evaporation is constrained by the satellite observations, rather than the result of simulation. If the reviewer refers to the W3RA model used in assimilation, then we believe a similar approach could be applied to other models, provided they have a coupled water and energy balance model and provided they are extended for data assimilation in the way described.

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

We do not think that all forcing data can reasonably be considered of really low quality in any objective sense, but some of the inputs create greater uncertainty than others. The model takes several different forcing data as input, the evaporation estimates are not equally sensitive to all of them, and the quality of forcing data also varies spatially. Hence it is impossible to give a quantitative answer to the question. With regard to evaporation specifically, one observed issue is that of heterogeneous biases in air temperature in regions with strong relief. Fortunately, secondary losses occur mostly in areas with low relief (see l. 162-167).

"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 cut-off 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."

Does the reviewer mean the model is biased in the absence of secondary evaporation (i.e. in drylands)? We do not have evidence for that. We have evaluated the ("offline" or background) model against evaporation rates reported by the global Fluxnet network for non-irrigated environments and did not find a bias (l. 505-510). The reviewer suggests showing global biases, but there are no global ET observations (other than Fluxnet) to calculate those from. However, we do compare with estimates from other models and find that ours are well within that range (l. 619-629). We did not use EnKF but nudging based on energy balance model inversion. The reviewer is correct that we did cut off the E' updates. It was necessary to maintain internal physical consistency, but it is true that it may have introduced bias, particularly if real E was consistently higher than the available energy, for example, due to biases in meteorological forcing data. We can mention this in revising the m/s.

"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."

Thank you for the suggestion. Unfortunately, data assimilation procedures can be difficult to explain and tedious and confusing to read. We will add a flow diagram to attempt to illustrate the procedure better.

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

This is discussed in l. 562-587. The assimilation of satellite vegetation observations goes some way to address errors in vegetation parameterization. However, the (necessary) assumption that the assimilation increment is due to irrigation has uncertainty associated with if - but only if - a large part of the grid cell is occupied by non-irrigated land. Hence also the recommendation that our approach should work better at higher resolution, if good irrigation area mapping was available. We hope to pursue this line of enquiry (see l. 600-609). Regards validation: see previous comment.

"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."

Domestic and industrial water use are not considered because these are typically non-consumptive uses (i.e., the water is returned to the environment after use). Possibly the main exception to this would be irrigation in urban landscapes, which the irrigation mapping does not capture well or at all. If those uses lead to surface cooling then the LST data assimilation will still have increased E estimates and so they are implicitly accounted for. In practice, consumptive urban or industrial water uses are unlikely to have a meaningful impact on the water balance of large basins.

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

Neither, they vary spatially as a function of climate aridity and land cover using predictive relationships derived from model calibration to evaporation, soil moisture and streamflow from a very large number of sites and small and unregulated catchments, respectively.

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

No, only grid-based routing (l. 135)

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

Poor at predicting highly dynamic surface water extent, you could say, yes. Like all global models, we believe.

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

0.05° using HYDROCLIM to downscale of the WFDEI data (l. 701-706). We agree that correct Tair is important.

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

Correct, but note that this does not imply that LST is assumed homogenous across the area, this calculation is to remove the mean bias between daytime LST and time-of-overpass LST.

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

At least in theory, yes. Agreed.

“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”

Disagree. The MIRCA2000 mapping suggests the grid cell is 100% equipped for irrigation. To our knowledge, the published models assume that the entire equipped area is irrigated so the statement holds. Of course, that assumption could be changed in principle, but that would simply move the problem.

“Line 619-623 I feel the units are incorrect, I guess the first estimates should be 75.5*10ËĘ12 Km3 y-1 (as well as for the other estimates from this study, which are now 1000 times lower than other studies)

We believe the units are correct. We could have written 75.5*10ˆ12 m3 y-1 as 75,500 km3 y-1 but felt using base units (m) was more appropriate.

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