

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 positive and constructive comments. Below pls find a response to the issues raised.

“First of all, I find the manuscript a bit unbalanced in terms of contents. There is a lot of focus on methods and equations (esp. for irrigation), but relatively a few figures for results. This makes the manuscript very tedious to read with a lot of text and information. At the same time, some information that are critical to assess the results are either missing or in the appendix. For example, forcings and their spatial disaggregation, model formulations of LE and H, etc.”

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We are sorry the m/s was tedious to read. We accept that the technical detail of the modelling and data assimilation can be a bit tedious, which is why we tried to minimise that aspect in the main text by transferring some of the material to the appendix and referring to previous publications where possible. In principle, we could ‘spruce up’ the m/s with additional figures, but there are in fact already 10 figures, several of them multi-panel ones, and hence we are not sure it would make the m/s less tedious to read. We will include a new figure illustrating the workflow, as requested by the other referee. At the same time, however, the referee also asks for additional material to be included that would likely make the m/s even more tedious for readers not overly interested in the modelling details. Given the model theory and formulation is already available elsewhere we hesitate to overburden this paper with it. The energy balance equation is the main model component of relevance here, which is why we included that aspect of the theory. We look for guidance from the editor as to what changes should be made to make the m/s less tedious yet provide any additional information deemed critical to assess the results. Possible options could include putting detail into the supplementary material, for example.

“Definition of the secondary evaporation: There is no description on how groundwater’s contribution to LE/ET is a secondary source. In an idealistic theoretical situation, the capillary flux from groundwater will replenish soil moisture (at some point when the soil moisture is drying up), which would eventually increase LE. It is not clear if the model considers such capillary flux processes explicitly. I am curious about what fraction of ‘other’ sources is actually coming from groundwater-soil-LE pathway, and not groundwater-baseflow-surface water-LE pathway. The first one may have a critical influence on vegetation and carbon cycle processes.”

The model does consider capillary fluxes, but in the offline model those are ultimately constrained by longer-term local rainfall, and therefore do not constitute secondary evaporation (i.e., it is derived from locally recharged, unconfined groundwater rather than lateral groundwater inflows). As our study demonstrates, data assimilation helps

to estimate secondary evaporation from non-local water sources, but does not directly attribute it to a water source – that requires ancillary data. In some cases, the secondary evaporation may be from irrigation with water pumped from confined aquifers (which bypasses the capillary rise pathway). In other cases, it is possible that secondary evaporation is inferred, e.g. because rainfall is underestimated capillary rise or deep root water uptake is more important than predicted by the background model (e.g., because the vegetation is more deeply rooted or groundwater is closer to the surface than assumed). There is obviously much more to be done to understand the global water balance in full detail. Our data provide a means of prioritising regions where there appears to be hydrological behaviour that is not easily explained by the background model, and therefore is worthy of further investigation.

“Assimilation of LST into model: In the assimilation of LST into model, the basic assumption is that the model-simulated partitioning of the energy fluxes (H and E) are correct. The corrections or ‘nudges’ for LST are back-calculated from the modelled H , and these are propagated through spatial patterns of observed LST. But, there is no explanation of how ‘background’ H and LE are calculated in the model. Perhaps, these may be inferred from previous papers/reports on the model (?), but they are so critical for this study and results presented herein, they deserve to be in this manuscript.”

The basic assumption is actually not that the partitioning of H and LE is correct, but rather, that the estimated total available energy ($A=H+LE$) is correct. Data assimilation may change the estimate of H and through that $LE=A-H$. The background H and LE are estimated using the conventional Penman-Monteith approach (Penman, 1966). However, we agree that we did not explain this very well and also did not provide much detail on the way PM theory was implemented and parameterised. Perhaps this is also the important detail the referee referred to in the opening comment. This information is indeed detailed in the model documentation, but we agree that that is an important aspect and will include more details in revising the m/s. It is a bit tedious due to the consideration of several evaporation pathways, and hence we might include it as an

appendix.

“One information that is imperative is whether the parameters of the modelled LE and H were optimized or not. If not, are the used parameter values are reasonable for a global-scale application?”

They were not optimised. The most important parameter overall, surface conductance, was predicted from satellite-observed surface reflectances following Yebra et al. (2013) and tuned using a large database of evaporation measurements (FLUXNET). Another important parameter, vegetation height (affecting aerodynamic conductance) was derived from Lidar remote sensing by Simard et al. (2011).

“Related to the above point, validation for model simulated LE and H is not shown or discussed. There are references to a previous study or an unpublished work but the findings of this study also warrant a section on evaluations at the global scale. I am aware that observed global ET and H data are not available, but a comparison with either FLUXNET observations (for sites) or other satellite-based ET products can provide a valuable benchmark.”

We have performed this analysis and mention the results in the text (l. 507-508). In revising the m/s we could include a figure with those results if the editor feels it adds value. We did not do so as it could mistakenly be interpreted as a validation of the data assimilation procedure, which it is not: the vast majority of flux towers are in environments without secondary evaporation.

“Estimation of irrigation water use: Assumption of rooting depth: The parameter s_{max} is dependent on the assumed rooting depth. The manuscript would benefit from a discussion on how these parameters vary globally, and to what extent do this variation affects the estimation of secondary evaporation from irrigated area.”

This is explained in l. 232-236. Essentially, we follow the published methodology of Siebert and Döll (2010). The assumptions made here, in fact, do not affect the esti-

mation of secondary evaporation at all. What it does affect is the calculated irrigation efficiency and therefore the estimate of irrigation water use. This is a perhaps subtle, but important distinction.

“Evaluation against discharge observations: In my subjective judgment, the improvement in the basins with discharge < 300 mm/y is mostly driven by Paraná because it has discharge with the largest magnitude. In reality, the river basins with large irrigation water withdrawal/use are also equipped with dams and are not of run-of-river type (with no reservoir). The secondary evaporation from these ‘dammed’ rivers also comprise of evaporation from reservoirs. So, in my opinion, it would be helpful to include the information of reservoir volume (e.g., from GranD database) in the analysis or the figure. This is important because the water evaporated from the reservoirs might actually be significant, especially because the irrigation requirement/use from this study is much lower than previous estimates.”

Actually, it is largely also due to the improved water budget for closed basins (dots on the vertical axis) and several other basins (e.g., Indus). Our methodology does use remotely sensed water extent, and that would include reservoir surface area, so evaporation from reservoir surfaces would be included in the estimates.

“Comparison with previous estimates: The manuscript addresses the minimum irrigation water requirement, which I understood as the actual gross irrigation water use (gross because it has both bare soil evaporation in irrigated areas+transpiration by crops). In most previous modeling studies, difference between PET and ET is used to calculate irrigation water requirement (and withdrawal). Current manuscript rightly points that there are several limitation to ET from irrigated areas. Despite that, it would make sense to compare the difference between PET and ET (Priestley Taylor is already used in the current study) with the bias of I0 against withdrawal.”

Unfortunately, we did not fully understand the analysis the referee proposes. We do compare I0 to withdrawal in Fig 5 and I. 435-449, and this does provide some useful

insights, discussed in I. 561-599. We could compare irrigation area ET to PET (as done for example in Fig. 1e) but are not sure how to summarise such a comparison globally or what it would demonstrate.

“Forcing variables: The results of this study are extremely dependent on the biases in the WFD forcing data as well as the spatial patterns of HYDROCLIM data. It is not clear from the current analysis if the biases in secondary evaporation are related to WFD magnitude (over a half degree grid) or the spatial patterns of HYDROCLIM (over 0.05 deg grids).”

The term ‘extremely’ is subjective, but given the Penman-Monteith energy balance approach used, the evaporation estimates will depend on the meteorological forcing data, as does any method to estimate evaporation. We used the relative spatial patterns in HYDROCLIM only to adjust air temperature. Because we only assimilated satellite LST in areas with modest relief, we do not expect that the downscaling will have had much effect on secondary evaporation estimates. We also suspect that biases in air temperature in the WFD forcing data may, in fact, be less important than uncertainties in the radiation balance, wind speed, and perhaps specific humidity.

“Temporal variation of secondary evaporation: I would have really learnt a lot on what is driving the secondary evaporation if there was a discussion on temporal variation of secondary evaporation at the global scale. This would provide insights on whether the secondary evaporation increases in wet season (for e.g., in water bodies such as wetlands and river channels because the surface area becomes larger) or in dry season in which the groundwater access by plant can be expected to be maximum.”

We thank the reviewer for this interesting suggestion and will seek to add some analysis around this. Based on our results so far, we suspect that secondary evaporation will be greatest in the warm season due to the importance of evaporation from slowly changing water bodies. However, we can certainly calculate these patterns and will look into it.

“Evaporation larger than precipitation in southern Africa and Yucatan: The discussion

focuses on the biases in the precipitation. If total E (primary + secondary) were correct, the signal should appear in the water storage changes. In that case, GRACE satellite measurements should show a declining terrestrial water storage. A comparison on loss of storage in the study period and the total $E - P$ would provide a great motivation for future studies on what are driving such changes. Essentially, this would already help in refining the potential causes of the negative water budget.”

Once again we thank the reviewer for the suggestion. Some knowledge of GRACE-based trends was on our mind in interpreting the results, but we did not make this explicit. Essentially, a previous GRACE model-data assimilation study some of the authors were involved in (Van Dijk et al., 2014: doi:10.5194/hess-18-2955-2014) inferred that water storage did decrease slightly over the Yucatan peninsula between 2003 and 2012 (slightly different from the 2001-2014 period in the present m/s), but increased quite strongly in southern Africa. Neither trend was predicted by an ensemble of hydrological models (particularly not the African case), which led us to suspect deficiencies in the rainfall estimates driving those models. We will expand the discussion along these lines.

Editorial Comments: # “Line 1: In my opinion, ‘estimates’ should be replaced by ‘simulations’. Essentially, the results are dependent on hydrological model simulations.”

We disagree; satellite observations were assimilated to make the results less dependent on model simulations.

“Line 232: $i=1,26$ can be replaced by just 26.”

We used this notation to make the meaning of i in A_i in the same sentence clear.

“Line 259: There is no description for what P_g is. I assumed that it is precipitation for the grid cell.”

The referee is right. Apologies, we will revise this.

“Figures 6-9: I recommend using the same color maps and scales in these figures.

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It is a bit confusing because the same color ‘blue’ means a different value in different figures.”

Thank you, we can change that.

“Table 1: Just curious that observed discharge in Nile is 0. Fascinating that no water from such large river basin reaches the ocean.”

Agreed.

“Line 534: can have affected → can affect or could have affected”

Agreed, thank you.

“Line 621: wrong units: km³/yr → m³/yr

Agreed, thank you.

“Line 671-673: → Before reaching the ocean is misleading because a fraction of the open water evaporation is from rivers which do not drain to the ocean (e.g., inland lakes).

We do not think this is misleading. Our phrasing was chosen for pragmatic reasons, although there is also a conceptual argument. The pragmatic reason was that, in identifying closed basins, we found it challenging to separate “truly” closed basins from basins that DEM analysis suggested were closed but which actually did appear to have an overflows according to independent reports. Surprisingly, it appears that there is no reliable global map of closed basins, and it took background research to identify the basins shown in Fig. 3. There were many other basins that the DEM suggested were closed but where we were not able to confirm that, meaning we ultimately did not identify all closed basins and therefore cannot make the distinction between secondary evaporation from (all) closed basins and all ocean reaching rivers. The conceptual reason is that the referee’s argument can, in fact, be turned around: those rivers in ‘closed basins’ do not drain to the ocean because open water evaporation is so high.

The difference between closed and ocean-draining basins is a threshold (lake) level, and some basins currently switch between these states depending on the difference between rainfall and evaporation, many others did in the past. We do accept that there are closed basins that would require a very large increase in rainfall indeed (or decrease in evaporation) to top the overflow threshold and start draining to the ocean, but it does mean that there is no fundamental difference between ‘closed’ and ‘open’ basin. We do believe that a map of all (currently) closed basins would be a valuable information source for water balance studies and are currently looking into producing one using DEM data of higher accuracy and resolution, but early indications are that it requires intensive quality control. If it had existed, we would have made the distinction.

“Line 674-678: Does the groundwater include baseflow-river-ET and groundwater capillary flux-soil moisture-ET? I am not sure if the second process can be categorized as the secondary evaporation.”

We are not entirely sure how to interpret this question. The primary evaporation estimates by the model do include the effect of capillary rise. However, if the primary evaporation estimates are too low data assimilation increases those estimates, and the difference will be (perhaps partly or wholly incorrectly) ascribed to secondary evaporation from lateral inflows. We discuss this in I. 501-504.

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