Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2017-757-RC2, 2018

Hydrology and Earth System Sciences
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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.

Anonymous Referee #2

Received and published: 11 April 2018

This study, "Global 5-km resolution estimates of secondary evaporation including irrigation through satellite data assimilation, " presents model-based estimate of the ET from secondary sources. The data produced (provided) is potentially useful, and the modeling framework is commendable for using multiple constraints and using satellite remote sensing in model-data fusion. The analyses presented in the manuscript are scientifically sound, but I have some comments or suggestions that would, hopefully, be useful for the authors to improve the analysis manuscript.

General: 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

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Discussion paper

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.

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.

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. 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?

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.

Estimation of irrigation water use: Assumption of rooting depth: The parameter smax 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.

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.

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.

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).

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.

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.

Editorial Comments:

Line 1: In my opinion, 'estimates' should be replaced by 'simulations'. Essentially, the results are dependent on hydrological model simulations.

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

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

Figures 6-9: I recommend using the same color maps and scales in these figures. It is a bit confusing because the same color 'blue' means a different value in different figures.

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

Line 534: can have affected –> can affect or could have affected

Line 621: wrong units: km3/yr –> m3/yr

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 ocean (e.g., inland lakes).

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.

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