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Discussions

Interactive comment on "Integrating multiple satellite observations into a coherent dataset to monitor the full water cycle – Application to the Mediterranean region" *by* Victor Pellet et al.

Anonymous Referee #2

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Review of "Integrating Multiple Satellite Observations into a Coherent Dataset to Monitor the Full Water Cycle – Application to the Mediterranean Region"

Overall, I found this to be a strong and well-written paper. It makes two (worthwhile) contributions. First, a methodological contribution regarding the appropriate approach for simultaneously enforcing water closure within land, atmosphere and ocean domains. Second, it advances the state-of-the-art in terms of water balance estimates for the Mediterranean Basin. These contributions are significant and worthy of publication in HESS. Nevertheless, there are three major points that the authors should address before final publication. I suspect that some of my concerns arise from misun-

derstandings on my part (rather than actual flaws) and can be addressed via re-writing for improved clarity.

MAJOR

1) The paper needs to do a better job of describing the INTegration (INT) methodology and its impact on subsequent stock and flux predictions. My understanding is that the INT approach is applied with two aims: 1) to downscale sub-basin scale results down to the pixel scale and 2) to extrapolate (balance-constrained) results OUTSIDE of the Mediterranean. This raises two questions:

a) If INT down-scales and extrapolates outside of the Mediterranean Basin – why does it have any impact on sea-level results in Figure 3 (which presumably reflect spatially averaged conditions within the Basin...neither of which are impacted by INT)? To me, it seems like SW+PF and INT should yield the same results for a sea-level metric. However, these results are used specifically to motivate the added value of INT (line 15 of page 17). Is the improvement in INT versus SW attributable to INT? Or would it also occur for SW+PF?

b) What exactly is the rational for the extrapolation portion of INT? Why would you ever want to extrapolate? Why not just apply terrestrial closure (at a minimum) to Northern Europe SW results? Also, how does this extrapolation contribute to the (bottom-line) analysis in Figure 6? I presume it facilitates the application of a larger atmospheric water balance analysis, but – given that this is a Mediterranean Basin analysis - it seems strange to extrapolate BEYOND the Mediterranean Basin. It would improve the manuscript if this extrapolation step was better motivated.

2) I feel like the paper could do a better job describing its approach to error estimation (and the effect of these estimates on its merging results).

a) I was confused by the treatment of EO uncertainty throughout the manuscript. First, in Line 8 of page 8, the manuscript says states "...we considered the same uncertainty for all data sets of given parameter following de-biasing..." However, later in Section 3.3, it seems as if a different uncertainty is assigned to various precipitation estimates when applying Equation (5). Can these descriptions be made more consistent?

b) in Section 3.4, the authors invoke a filter-based closure constraint that varies as a function of a Σ matrix but do not describe how this matrix was derived. If would be helpful if this was clarified.

c) Equation (5) appears to use the temporal standard deviation of individual products (precipitation products in the example given) after seasonal bias correction as a proxy for the magnitude of their random error. This seems like a dangerous assumption. Assume, for example, that you had a precipitation product that simply mimicked the TMPA seasonal climatology (used here as the de-biasing reference). Given large inter-annual variability in rainfall, this product would be a poor rainfall product to use in a water balance context. However, it would have a low temporal standard deviation, and (therefore) be heavily weighted by Equation (5).

Also - on a related point - after you de-bias the precipitation products, does it really matter (for a long-term water balance study) how - or even IF - you merge the products? After de-biasing, they all have the same long-terms means and will thus produce the same long-term water balance analysis.

3) Page 16, Line 3. The PF approach is designed explicitly to reduce closure residuals. So, it is questionable to use the reduction of closure residuals as evidence that that PF approach is "working" or that a closure constraint is necessary. Another possibility is that not all flux and stock terms are being accounted for him. That is, the flux/stock estimates utilized here are actually accurate but nevertheless should not close. Some discussion of this possibility is needed. The same issue comes up in Section 6, first paragraph. By design, the author's approach reduces residuals (that is a given). However, can this really be taken as objective evidence that flux or stock predictions have actually been improved?

Other Issues:

1) Why are there so few rain gauge stations applied to the precipitation analysis in Figure 5? It's difficult to believe that only 7 rain gauges are available in the Mediterranean Basin. Also, why is it that the best results (for INT) in Figure 5 occur OUTSIDE of the basin (where INT results are based on an approximate extrapolation)? This seems odd. If INT is performing an accurate downscaling, it seems like it would be more effective WITHIN the basin.

2) In Section 3.2 (on the "optimal selection" (OS) approach) necessary? The methodology section is already quite long and the OS results do not seem to make a significant contribution to the manuscript's results.

3) What is meant by "quasi-triangular balance" in Section 5.1? This terminology will likely be unfamiliar for some HESS readers (it was to me).

4) Figures 4 and 6. A better use of color would be to differentiate between the INT and CAL cases (which are very difficult to distinguish). Also, the INT+/- and CAL+/- notation should be explained in the figure caption.

5) Some discussion of the statistical significant of differences in Figure 3 would be useful.

6) Overall the paper is quite well-written but it does suffer from an excess of minor English usage errors. Superficial proof-reading in this regard would help.

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