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Interactive comment on "Integrated validation of assimilating satellite derived observations over France using a hydrological model" *by* D. Fairbairn et al.

Anonymous Referee #2

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This study describes the implementation of a simple Extended Kalman Filter (SEKF) to assimilate LAI and soil moisture observations into a hydrological model over France, and its validation against streamflow measurements. The topic is appropriate for the HESS journal, but the paper is not very well written. The technical approach appears sound at places and has some interesting aspects but there are many issues with the results, or at least their explanation which is not clear at all.

The NITm and NITbc simulations use a different minimum LAI (1.2 m2/m2) and a bias-corrected radiative forcing (+5%) respectively, but nothing is said about how these numbers were chosen. Was the new minimum LAI chosen based on the observations? If so, there really is no point in comparing the LAI from the new simulations with the same

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data.

Additionally, the Nash scores of the NITm and NITbc simulations are shown only for the stations where at least one of the simulations had a positive score (p. 9, l. 21-22). Essentially, the average NSEs reported in Table 4 and Figure 6 are artificially better than what they ought to be since “most of the stations in northern and southeast France are excluded from this calculation”. No explanation is given at to why this was done, making the discussion of the results rather dubious.

Furthermore, the assimilation doesn’t appear to have much of an impact on the streamflow simulations and actually decreases the skill (even when excluding the stations that had the negative NSE). I wonder what the rationale was of not using a more sophisticated data assimilation algorithm that could overcome some of the limitations in the SEKF. There are many limitations with this approach that I don’t see any worthwhile scientific contribution added by this study, although there are some interesting aspects in this work.

Given these flaws, I unfortunately will have to recommend that the manuscript be rejected. Some minor comments are outlined below, in case the editor decides on requesting major revisions.

p. 2, l. 10: I would replace the term “network”, which usually refers to in-situ measurements. p. 2, l. 10 “a short forecast from the past”: it doesn’t have to be from the past, it can be a prediction of the current time (i.e. observation time). p. 4, l. 22: can the authors add a sentence on what the “delayed cut-off” version of SAFRAN is? p. 5, l. 15: why were only ASCAT observations used and not SMOS for example? Is it because of the study period? p. 5, l. 21: why do the soil water index data need to be interpolated to the model resolution? Can’t the SEKF handle different spatial resolutions between the model and the observations? p. 5, l. 25: has the WG1 soil moisture climatology been validated? p. 5, l. 32: were additional LAI products considered (e.g. MODIS)? p. 7, l. 25-27: this is confusing, how are the 1.2 m2/m2 and +5% values obtained? p.

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8, l. 5: how are the LAI and WG1 estimates validated against satellite observations? Weren't these satellite observations assimilated into the model? p. 9, l. 28-29: I don't understand how the good performance of the NITbc is explained by the relationship between the bias in the discharge ratio and the NSE. Doesn't the NITbc just have a bias-corrected radiative forcing? Where's the causality between the simulation configuration and the performance? Wouldn't it make sense that the model with the smaller bias would have better performance in terms of NSE? p. 12, l. 14-15: but nothing is said on how the higher LAI parameter was chosen.

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