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Hydrology and Earth System Sciences Discussions

Interactive comment on "A new data assimilation approach for improving hydrologic prediction using remotely-sensed soil moisture retrievals" *by* W. T. Crow and D. Ryu

W. T. Crow and D. Ryu

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Reviewer #1 (Rolf Reichle)

Major point. The reviewer questions the use of the API model in the rainfall correction portion of the methodology and why analysis increments derived directly from the SAC model were not used instead. They ask that we either modify our methodology (such that the rainfall-correction approach is based on analysis increments calculated from the SAC model) or explain why such a modification cannot be made, and argue that such a modification is desirable because it would: 1) unify the analysis as a pure synthetic twin experiment and 2) allow us to drop Case 4 as obviously sub-optimal.

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Response: As background - the general methodology of the paper is based on a synthetic twin analysis in which synthetic soil moisture observations are generated by the SAC model and subsequently 1) used to correct SAC rainfall amounts using our API-based analysis increment approach, 2) and re-assimilated back in to the SAC model to correct SAC model states. In this way the paper attempts to simultaneously correct for the impact of both rainfall forcing errors and incorrect antecedent state representation on hydrologic model runoff predictions. As the reviewer points out, an alternative approach would be to neglect the API model and simply use SAC model analysis increments to correct antecedent rainfall.

This is a very good point that was obviously not treated adequately in the original manuscript. In response, we have chosen the second option presented by the reviewer and added a discussion (see the last paragraph of Section 5) which clarifies the technical barriers involved in eliminating the API model and making our rainfall correction approach compatible with the nonlinear, multi-state SAC model. To date, all real data validation of our rainfall correction system [in Crow et al. (2009)] has been limited to the use of the linear, single-state API model. Preliminary efforts aimed at modifying the rainfall correction procedure to run with a nonlinear, multi-state model (like e.g. the SAC model) have run into complicating factors related to variations in the effective "memory" of various soil moisture states (e.g. for short-term rainfall correction - how to you weigh soil moisture analysis increments applied to a thin surface layer versus a more substantial water table representation?). In addition, the nonlinear nature of these models hampers the innovation-based tuning procedure required to calibrate the Kalman filter (Crow and Van Loon, 2006). This calibration is a critical component of the procedure since it minimizes the likelihood that noise in soil moisture retrievals will corrupt subsequent rainfall estimates (Crow et al., 2009). Consequently, the prospects for successfully adapting the API-based approach to the nonlinear and multi-state SAC model are unclear. In contrast, the API-based rainfall correction approach has already been validated using real remote sensing data products (Crow et al., 2009). In order to maximize the realism of our synthetic results (by keeping it grounded in a verified

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methodology) our strong preference is to keep the synthetic analysis based on an API procedure that has been clearly verified using real data.

We also disagree slightly with the reviewer's characterization that their suggested modification would strengthen the paper. Our fundamental concern for this paper is that the presented synthetic twin experiments provide a realistic representation of a real data assimilation system - particularly with respect to real data challenges encountered when applying our rainfall correction procedure (see Section 7 of the manuscript). The change suggested by the reviewers would generally improve the performance of our rainfall correction procedure by applying it within a less challenging identical twin experiment (where the model used to generate synthetic observations is the same as assimilation model) as opposed to a more challenging (and less realistic) fraternal twin experiment (where synthetic observations are generated using a different model). However, it opens up the real possibility that observed benefits are simply artifacts of an overly artificial synthetic framework. In the paper we attempt to be conservative about assessing the marginal impact of our new rainfall correction scheme by applying the (baseline) state-correction EnKF technique as an identical twin experiment and applying our new rainfall correction scheme using a (more challenging) fraternal twin methodology. In this way we provide a conservative description of its potential added value. Therefore, while the modification presented by the reviewer would streamline the methodology (by making it a pure identical twin experiment) we feel our mixed fraternal/identical twin experimental methodology is actually a positive attribute in that it makes the central conclusions of the manuscript more credible. This point was not adequately articulated in the original manuscript but is now clarified through new text added to Section 7 (3rd paragraph) Section 8 (last paragraph).

Finally, while Case 4 is clearly sub-optimal, the fact that it is sub-optimal is based on a correlation between observing and modeling errors that does not depend on whether our whether our rainfall correction approach is applied using the SAC model or an API approach. In particular, even of we were to adopt the reviewer’s suggested

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modification, we would still feel it necessary to demonstrate the sub-optimal properties of Case 4. That is to say, we do not feel the replacement would make the sub-optimal nature of Case 4 results any more obvious.

In summary, we feel that the negatives associated with the modification (principally decreased realism) outweigh the possible advantages. However, we acknowledge that an adequate discussion of this important point was missing in the original manuscript and have made a strong effort to address this likely reader concerns through modifications discussed above.

Crow, W.T., Huffman, G.H., Bindlish, R. and Jackson, T.J.: Improving satellite-based rainfall accumulation estimates using spaceborne surface soil moisture retrievals, in press, J. Hydrometeorol., 2009. (Draft of accepted manuscript available online at: http://www.ars.usda.gov/SP2UserFiles/person/30797/pdf/Crow.etal.pdf)

Crow, W.T., and Van Loon, E.: The impact of incorrect model error assumptions on the sequential assimilation of remotely sensed surface soil moisture, J. Hydrometeorol., 8(3), 421-431, 2006.

Minor points

1) Comment: Page 2008, lines 24-27: Why does this opportunity "only" exist for systems with near-real time precip forcing? Is it because they are of poor enough quality for the procedure to have a measurable positive impact? It is not clear that "rainfall forecasts from a NWP model" would be any better. Perhaps you wanted to say "gauge-corrected reanalysis precip"?

Response: Our point here was that our approach was only viable using near-past measures of precipitation (either model or observation-based) and could not be applied when using precipitation forecasts as forcing data (since, obviously, no remote sensing observations are available to correct future precipitation). In hindsight, this is an obvious point and our clumsy attempt to articulate it in the original manuscript only

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serves to confuse the reader. Therefore, to avoid confusion, the text referred to by the reviewer has been removed from the manuscript.

2) Comment: Equations (10), (16), and (18): This may sound silly, but please consider changing the symbols for the "max" capacity parameters. At first reading, I did not see that e.g. UZFWC was different from UZFWM, because the acronyms differ only in the fifth letter. How about something like UZFWCmax?

Response: Good point - suggested change has been made throughout manuscript.

3) Comment: The phrase "they are re-applied" could use some clarification and elaboration. Presumably, you first run a single ensemble member with all perturbations turned that serves as the Open Loop. Next, you use the perturbed forcings as inputs into an ensemble integration in which the forcings and the state variables are again perturbed with the same perturbation parameters that were used in the Open- Loop. Does that make sense?

Response: Additional elaboration (along the lines suggested by the reviewer) has been added to the phrase "they are re-applied" in the 2nd-to-last paragraph of Section 4.

4) Comment: Page 2018, lines 15-20: These lines are redundant and can be deleted.

Response: Agreed - text has been deleted.

5) Comment: Page 2020, line 23: Replace "you can" with "one can" (or change the sentence to the passive voice).

Response: Text has been changed to "one can".

6) Comment: Page 2023, line 9: Replace reference to "Fig 2" with reference to "Fig 1"?

Response: Typo has been corrected

7) Comment: Page 2024, line 13, also Figure 9 (graphic *and* caption): Are SER and SIR the same? Please clarify and/or correct accordingly.

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Response: Text references to SIR were a typo and should have read SER. Fixed in revised manuscript.

8) Comment: Page 2028, line 20: delete "for approach" from Section heading

Response: Good point - "for approach" has been deleted

9) Comment: Page 2030, line 24: It would seem that flood forecasts (as opposed to retrospective runoff estimation) *must* use precipitation forecasts (rather than near-real time satellite rainfall) from at least the time of the production of the forecast out to the forecast lead time. The precipitation inputs up to the production time may be from a variety (or hierarchy) of precipitation data of various quality and latency.

Response: Not necessarily, in large basins possessing appreciable saturation time scales, stream flow forecasts (at a downstream point) can be generated with multi-day leads times based solely on real time rainfall/runoff modeling. This type of hydrologic predictability is, of course, lacking in flash flood events in which real time rainfall/runoff modeling will provide little or no forecasting lead time. New text in the second to last paragraph of Section 7 clarifies this point.

10) Comment: Figure 2: Red and black curves are not labeled (which is model stream-flow, which is USGS obs?)

Response: New labels have been added to Figure 2 to clarify which are observations and which are SAC model predictions.

11) Comment: Figure 2: It would be good to provide lat/lon information within the Figure or in a separate map or Table about the location of the five basins.

Response: Latitude/longitude information has been added to Figure 2.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 5, 2005, 2008.

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