

## ***Interactive comment on “A past discharge assimilation system for ensemble streamflow forecasts over France – Part 2: Impact on the ensemble streamflow forecasts” by G. Thirel et al.***

### **Anonymous Referee #4**

Received and published: 21 June 2010

Review of the submission “A past discharge assimilation system for ensemble streamflow forecasts over France – Part 2: Impact on the ensemble streamflow forecasts” by Thirel et al., submitted to HESSD.

#### Summary:

The present paper is the companion paper to Part 1, which describes and validates the assimilation system. Here, the authors present a real application of the system, using different forcing data (from operational to reanalysis data). They then go into detail, studying the levels of performance of the system for a number of stations, before vali-

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dating the results against semi-independent other stations. The results show promising improvements to the performance of the forecasting system, in particular when using the new soil parameterisation scheme. Furthermore, the paper strengthens the point for streamflow data assimilation, using ensemble forecasting systems. I recommend that this paper to be published after some minor edits. Let me also say that both papers are very well written, both from a technical and linguistic point of view (except for the occasional laps into colloquialisms, see below).

#### Technical comments:

1. One of the things that I miss in this paper (and actually in all its predecessors, be it Seo, Georgakakos, Rabuffetti or others) is the lack of validating the analysed soil moisture states against real observations. Given that this paper is written in the context of an operational system, every aspect has to be considered, which also includes the general hydrologic state. The authors implicitly acknowledge some of this, by stating that the system should preferably not affect the fluxes (I assume in order to keep the energy balance with the atmospheric forecast in the operational system), but only using the increment size as an indicator of quality for the soil moisture has its limitations. I believe that France does have some soil moisture networks in the south-west and in the Seine basin, why not cross-validate with those observations (especially since the performance of SIM has been validated against remotely sensed observations in the past)? Then discuss its performance quantitatively, rather than qualitatively.

2. How do you estimate the observation error? This is not clear.

3. Another aspect missing is also the discussion of the fairly coarse resolution pixels. I understand that in Part 1, it is said that there are some subpixel parameterisations in Modcou, but what are those and how is the surface routing done internally.

4. The previous comment leads to another question. How do you address the fact that runoff is essentially an integrated effect of the upstream conditions. However, those have to be in all four dimensions, as the runoff from the same catchment

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can cause river discharge at significantly different event windows in the stream. Eg. Pauwels et al. (in two papers) discussed the time-lag quite extensively, does the BLUE take this implicitly into account?

5. It is stated in the paper that the best results are found for small catchments. Due to the size of the pixels, I would expect that, as the background error is likely to be higher, when just a few relatively large pixels have to represent a small catchment, rather than a large number of pixels in a large catchment. A more detailed discussions into the aspects of such errors/problems could go into section 2.1.

6. This study is very specific for the conditions prevalent in France, however, what do you expect for other regions, for example the Amazon, where soil moisture would hardly play any role, or Siberia, or as another extreme Australia?

7. In section 2.1 you state at the end that SIM has been validated for various states, but not SM. However, you are analysing SM, so what is happening to those other states, when SM has been updated? Are they still acceptable (ie. are there no inconsistencies)?

8. (This may refer more to Part 1, but also to section 2.3) Could you be more specific as to how you defined the ensembles (ie., upper and lower bounds, spread etc.)

9. The last paragraph of p. 2461 talks about validation of the assimilation system. Do you consider spatial correlations in the soil moisture fields or were all pixels independent? Could you discuss your choice?

10. In the same paragraph, I am not sure why dry period should only have small increments. I would suspect that in very dry conditions even large increments may not greatly affect the streamflow conditions, as the surface may be well below the critical limit from which onwards surface runoff is produced.

11. p. 2467, last paragraph: can the effects of this over- and underestimation of the precipitation be somehow quantified for the assimilation scheme.

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12. The conclusion section is fairly long. More so, a large part of it is a plain summary of what was shown previously. Please rephrase the conclusions so that they are read as a critical conclusion from the results or split it into a summary and conclusion section.

Editorial comments: p. 2457, l.23: replace “observed streamflows” with “streamflow observations” (I recommend throughout both papers).

p. 2457, l.27: “Lakshmi” not “Lakskmi” (also in the reference list)

p. 2458, l.28: remove the “a” in front of “statistical”

p. 2459, l.8: “... system, first for 148 ...”

p. 2459, l.12: “... is a hydrological ...”

p. 2459, l.18: remove “used”

p. 2461, l.10: “soil moisture states”

p. 2463, l.6-8: it would help if you could be more specific here and briefly discuss the why the operational system would be so different to the EPS.

p. 2465, l.15-18: reduce the sentence to: “this reference Brier Skill Score was adjusted following the method developed by Weigel et al. (2007) to correct the bias of the BSS caused by the lower number of members in the ensemble.”

p. 2465, l.20-21: “The BSS [...] showed that the best performance was achieved for IS2, then for IS1 [...], and the lowest skill was for ...”

p. 2465, l.26: I don't think “very bad” is an appropriate expression for a paper. Replace “bad” with “low”.

p. 2469, l.5-7: please rephrase this sentence. I am not sure what you want to say here.

p. 2472, l. 8: replace “consequently to” with “after applying”

p. 2472, l.11-13: Either drop Fig. 9 or discuss it in more detail. But any figure that only

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merits three lines of discussion is not really worth putting into a paper.

p. 2476, l.18: replace “and can cause trouble” with “which may lead to problems”. Also add why it may cause problems!

p. 2476, l.21: replace “especially” with “so”

Figures: I feel that amount of figures is an essential overkill. Please consider either combining some (eg. 6&7) or transfer them into tables (eg. Fig. 8). But 13 individual figures are too many.

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 2455, 2010.

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