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Hydrology and Earth System Sciences Discussions

Interactive comment on "GloFAS – global ensemble streamflow forecasting and flood early warning" *by* L. Alfieri et al.

Anonymous Referee #1

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In this paper, the feasibility of running a Global Flood Awareness System (GloFAS) is evaluated and its performance tested. GloFAS runs globally on a daily basis and establishes probabilistic stream flow predictions. I recommend to consider this manuscript for publication in HESS after major revision. My main comments are given below, along with some specific comments to particular parts of the manuscript. Specific detailed comments are given in the annotated manuscript, delivered with this review.

Main comments

1. Structure of the paper: the chapter naming and subsequently the structure of the paper need a bit of work. Chapter 2 is called 'Data and methods' while Chapter 3

is called ‘Methods’. Please make sure that all the data and methods descriptions are in one chapter. Possibly you could call chapter 3 ‘Performance evaluation’ to make a clear distinction between what you describe in Chapter 2 and 3.

2. Reasons for performance: it is not clear what component of the forecast chain provides good performance at which location. In most cases it seems that good performance occurs in relatively large basins, which gives rise to the idea that most performance is gained by the initial states of the system, rather than the meteorological forcing. In fact, Fig. 8 looks like a blue-print of the travel times of the considered rivers, suggesting that most skill comes from the initial conditions. In other words: is it really necessary to run a full ESP prediction? This needs to be discussed an supported by the analysis.

3. The term ‘value’ is misused in the manuscript and should be replaced by ‘skill’. The value of a forecast is not determined by PSS or any other skill score. This is determined by the user decision and whether the forecast results in a better decision (i.e. cost-loss ratio of the mitigative action based on the forecast should be positive). For instance, a flow forecasting system for the Sahara will have perfect skill but has no value at all for any user.

4. The length of the retro-active forecast (2 years) is very short! Since the ECMWF forecast system is used, a much longer retro-active forecast could have been used. I am not requesting a new analysis based on a longer retro-active forecast, but please discuss the possible effects of the length of this short retro-active forecast on the results more clearly!

5. Discussion: some points brought forward in the paper are not properly discussed. I missed two things: first, what are the possible consequences of drift in the initial states (mentioned on p. 12302, l. 13). The second point refers to p. 12310, l. 23. Apparently warnings may be given in heavily regulated areas based on simulations that assume natural flow conditions. Could it be that end-user are

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misinformed by forecasts from such locations? This needs to be discussed as well.

Particular comments to parts of the manuscript

1. Section 2.2.2. The description of the Lisflood component is very limited. It is not clear why sub-surface runoff from HTESSEL needed further routing through lisflood linear reservoirs, how it is subdivided over the different linear reservoirs in Lisflood and how consequently the residence times of these linear reservoirs are estimated over the whole globe. This needs clarification.

2. Section 2.3 It would help the reader if you can draw a timeline (i.e. an additional conceptual figure) indicating how initial states are produced and how a forecast is prepared in a forecast batch.

3. p. 12307. l. 13-14. 'this type of analysis. . ..weather predictions'. I think this is not true. In many places, the skill is to a large degree a function of the memory of the hydrological system, in particular storage of soil moisture, groundwater, surface water and snow pack (where and when occurring). In l. 17-19, this is also hinted at but it is not very clear, since only the relation between the upstream area is mentioned. Please make very clear which components could impact on skill.

4. Section 4.3. The case study is interesting, but could be strengthened very easily. Now only one forecast is shown. What would make the case much more interesting is too see how far ahead a warning level could have been detected using the system and discuss the established warning lead time, accuracy of peak and timing of the peak as the forecasts progress in time. So you should include a number of earlier forecasts to show this. Finally, discuss again what the cause of performance is. Is it the quality of the meteorological forcing? Or is it the dominance of the initial states that result in a good performance?

Some references that should be added or updated:

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(a) p. 12295, l. 15. A good reference to the forecasting warning and response chain is Carsell et al. (2004).

(b) p. 12296, l. 15. Reference to Candogan Yossef et al. (2011) should be updated to (Candogan Yossef et al., 2012)

References

Candogan Yossef, N., Van Beek, L. P. H., Kwadijk, J. C. J. and Bierkens, M. F. P.: Assessment of the potential forecasting skill of a global hydrological model in reproducing the occurrence of monthly flow extremes, Hydrol. Earth Syst. Sci., 16(11), 4233–4246, doi:10.5194/hess-16-4233-2012, 2012.

Carsell, K. M., Pingel, N. D. and Ford, D. T.: Quantifying the Benefit of a Flood Warning System, Natural Hazards Rev., 5(3), 131–140, doi:10.1061/(ASCE)1527-6988(2004)5:3(131), 2004.

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/9/C6069/2012/hessd-9-C6069-2012-supplement.pdf

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 12293, 2012.