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Hydrology and Earth System Sciences Discussions

Interactive comment on "Skill assessment of a global hydrological model in reproducing flow extremes" *by* N. Candogan Yossef et al.

Anonymous Referee #2

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Summary Comments

This is a well-written paper which addresses an important research need within the hydrological modelling community to corroborate hydrological extremes simulated by global hydrological models. The paper is clearly written and utilises some effective means of data analysis, which are to be commended. However, the use of bias correction on hydrological model output may prevent the publication of this manuscript. This approach is highly questionable in the circumstances it is used, and without further elaboration on the reasoning for this or details of it, it is difficult to approve this paper for publication. The content of the paper is also weighted too heavily towards data and methodological considerations, leaving insufficient time for description and explanation

of results. In general, there is a lack of rigour in the discussion, with results presented often without providing any explanation. Should these issues be resolved, there is enough promise demonstrated by this paper that it could proceed towards publication (following major revisions).

Technical Comments

The main issue with this paper is the use of bias correction of global hydrological model (GHM) output. The presentation of raw output from the GHM (i.e. not bias corrected) in this paper does not give any consideration to the possibility that there may be issues with the GHM (structure, parameterisation, etc.), and I am not sure I agree with the assertion that non-bias corrected data look 'reasonable'. I find it difficult to believe that the often vast differences between simulated and observed data are entirely determined by the climatic input. If bias correction is justifiable, the reasoning given for its use (Pg 3476, Ln 5-6) is too brief and not sufficient, and there is insufficient information provided on the correction procedure. I am not surprised that bias correction improves the simulated data (Fig. 3), given that the procedure is simply based on comparing to the mean monthly flows of other years. Some of the non-bias corrected hydrographs suggest that it may not be necessary to bias correct.

Given that this paper is supposed to be a 'skill assessment of a global hydrological model', it appears that any analysis of data after bias correction is more of an investigation of how successful bias correction has been. To truly explore the ability of the model in reproducing flow extremes, it would be more pertinent to analyse the non-bias corrected data, which should give a better reflection of potential shortcomings in the skill of the GHM. See Haddeland et al. (in references) for an example of a study which assesses GHM outputs (without bias correction) against observed data, attributing differences between models and observations to varying model structure. See also Doll et al. (2003) for an example of a study of a GHM that analyses model skill against observations using non-bias corrected data. The WATCH Special Issue of the Journal of Hydrometeorology also features a number of papers within which GHM outputs have been used without bias correction. In this instance, it seems as though bias correction may have been adopted in order to improve agreement between simulated and observed data presented in the hydrographs, whilst turning a blind eye to the issues with the raw simulated data and therefore the GHM.

The selection of some of the observed data from the GRDC can also be questioned. Some of the observed river flow timeseries are very short (e.g. the Ganges, Indus, Brahmaputra and Zambezi). For the Zambezi in particular, it is difficult to validate model output for 1958-2001 on four years of data, and removing this would still leave sufficient geographical coverage of four basins in Africa. If two basins are sufficient for North America and South America, then four should certainly suffice for Africa. It seems strange to have five of the twenty basins, a quarter of the total, in Africa, with far fewer per continent elsewhere. It is also unclear why the Murray, Zambezi and Parana have been selected when the GHM cannot simulate agricultural impacts on the hydrological regime. If the model cannot hope to produce reasonable results for these three basins, this does not appear to add much to the paper. Furthermore, I would have expected more than three of the selected basins to be affected by artificial influences. Perhaps consider including 16 basins, and use a minimum criterion for data availability of at least half of the 1958-2001 study period.

A further issue is the use of monthly data itself in a study on the ability of a GHM to reproduce hydrological extremes, for which daily data are most appropriate. Twelve of the twenty basins selected have daily data on the GRDC for at least half of the study period, including some of those basins that have very short monthly records (e.g. Zambezi). It may also be possible to find medium-sized basins which do have daily data for more than half of the timeseries, with the gain of validating using daily data outweighing the loss of areal coverage. Given that the GHM runs on a daily timestep, and that daily discharge data appear to be available, and that the study concerns hydrological extremes, it may be prudent to analyse the model on a daily timestep; currently, aggregation to the monthly timestep represents a loss of information on model performance

and does not capitalise on the full extent of the opportunities in this study.

The use of terminology such as ‘forecasting’, ‘hindcasting’ and ‘reproducing’ needs to be rationalised throughout the paper. It is important to note that anything related to past hydrological extremes cannot be termed forecasting, as it is on some occasions throughout the paper. Since the GHM is being used to simulate past observed hydrological extremes, I would favour the term ‘reproduce’.

Pg 3470, Ln 22-24: I would be careful with this statement (repeated towards the end of the conclusion; Pg 3485, Ln 17-18). This paper has shown that there is some potential for reproducing past hydrological extremes, but that is somewhat different to using the GHM to forecast monthly river flows into the future.

Pg 3477: the use of 25th and 75th percentiles suggests that anomalous flows occur 50% of the time (25% highest flows, 25% lowest flows), which would make them just as likely as ‘normal’ flows, therefore not particularly anomalous. For an indication of high flow and low flow anomalies, I would suggest using 15th and 85th percentiles as a minimum (perhaps even 10th and 90th), which would be a better test of model skill in reproducing anomalous flows. The reproduction of the anomalies is much better than the reproduction of the hydrographs because the target window (top 25%, bottom 25%) is so large. It would be interesting to see the effect of varying percentiles in the second analysis and varying return periods for the third analysis.

Pg 3482, Ln 1-16: the results presented in Fig. 2 have been caveated in this paragraph, although it is surprising that deficiencies in the GHM are mostly neglected as a potential explanation. I would not expect issues concerning climate input data to be entirely responsible for all of the differences between simulated and observed river flows presented.

Pg 3483, Ln 4-6: all results prior to this were discussed in terms of MSESS, with R2 and NS neglected. This imbalance needs to be corrected, or alternatively, if R2 and NS do not lend anything to the discussion, perhaps they should be excluded.

Pg 3484: it is correctly stated twice on this page that the GHM shows more skill in reproducing floods than droughts, although no potential explanations are highlighted. It would be useful to propose some ideas on the possible reasons for this.

Pg 3485, Ln 21: I would be interested to know which GHMs are analysed in Sperna Weiland et al. 2010b. In general, I would be wary of talking about other GHMs that have not been analysed within the same framework as is used in this paper, particularly when it is unclear whether the other GHMs have been bias corrected.

Specific Comments

- global hydrological model (GHM) is preferred over macro-scale hydrological model (MHM)

- Pg 3471, Ln 4: Nijssen et al., 2001a is used to support model development over the past two decades, yet it does not cover the last ten years; more up-to-date reference required

- Pg 3472, Ln 11: CRU should be written in full, as it has not been used yet

- Pg 3472, Ln 25: GRDC should be written in full, as it has not been used yet

- Pg 3474, Ln 5: 'in surface runoff' should read 'into surface runoff'

- Pg 3475, Ln 7: 'non-regulated', 'unmodified', 'natural' have same meanings; one will suffice

- Pg 3477, Ln 20: need references for Heidke skill score and the Peirce skill score here

- Pg 3477, Ln 21: Gandin and Murphy already abbreviated to GM earlier; can just use GM

- Pg 3484, Lns 8-18: all numbers written as digits (e.g. 0) should be written as words (e.g. zero)

- Pg 3496: tables are the same as those on the previous page; second ten should be

shown

- Fig. 3: blue colour on graphs is not as distinct from black as red on previous graphs

- Fig. 4: unclear what the different colours represent in the reliability diagrams

- Fig. A1: might be useful to have index of correlation on this graph

Suggested References

Doll, P., Kaspar, F., and Lehner, B. 2003. A global hydrological model for deriving water availability indicators: model tuning and validation. Journal of Hydrology, 240(1-2), 105-134.

Haddeland, I., et al. Multi-Model Estimate of the Global Terrestrial Water Balance: Setup and First Results. Journal of Hydrometeorology, early online release. http://journals.ametsoc.org/doi/pdf/10.1175/2011JHM1324.1

WATCH Special Issue of the Journal of Hydrometeorology. http://journals.ametsoc.org/page/WATCH

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