Interactive comment on "Calibration approaches for distributed hydrologic models using high performance computing: implication for streamflow projections under climate change" *by* S. Wi et al.

A. Efstratiadis (Referee)

andreas@itia.ntua.gr

Received and published: 3 November 2014

General comments:

The manuscript investigates alternative parameterization and calibration approaches for a conceptual hydrological model, aiming to represent streamflows at multiple sites across a large-scale river basin in Afghanistan. In particular, three different parameterization schemes (lumped, semi-distributed and fully-distributed) are combined with two multisite calibration strategies (stepwise and pooled), thus formulating a challenging

calibration exercise that takes advantage of high-performance computing tools. The outcomes of calibrations for the most promising modelling configurations are also evaluated by running the model with future climate projections, to estimate the response of the catchment against hypothetical changes in meteorological inputs.

The paper is well-structured, well-written and easy to follow. I am very happy with the experience gained from this exhaustive modelling experiment, which reveals the superiority of pooled calibration (i.e. estimation of model parameters on the basis of flow data at multiple sites across the basin) over the stepwise strategy, and also reveals the advantages of semi-distributed over (non-parsimonious) fully-distributed schematizations, by means of improved predictive capacity and reduced parameter uncertainty. These outcomes are in agreement with the "holistic" approach proposed by Nalbantis et al. (2011) and other researchers of the same philosophy, which recognize that: (a) model complexity should be as high as allowed by the available information, and (b) all available information – even a single measurement – is valuable and should be accounted for in calibration. Unlikely, this is not the dominant philosophy among modellers, thus I believe that this paper will be a significant contribution to both hydrological science and practice.

By reading this very good paper, I detected some issues to be clarified or further discussed, thus my recommendation is for a minor revision. In the following list, please find my specific comments as well as some technical corrections, to be addressed in your revision.

Specific comments:

1. p. 10275, lines 15-17: "Importantly, distributed hydrologic models can evaluate hydrological response at interior ungaged sites, a benefit not afforded by conceptual, lumped models." Please, remove "conceptual", which refers to the modelling approach behind the formulation of the governing equations and not the spatial discretization of the model domain. Apart from lumped models, semi-distributed schemes are also by

definition conceptual. Quoting Beven (1989), even a fully-distributed physically-based model can be regarded as conceptual, at the grid scale.

2. p. 10275, line 27 to p. 10276, line 2: "Parameters can be discretized across the watershed in several ways: uniquely for each grid cell (fully distributed), based on hydrologic response units (semi-distributed), or in the simplest case, a single parameter set for all model grid cells (lumped)." In hydrologic models, hydrologic response units (HRUs) are mainly used for distributed and less often for semi-distributed schemes (e.g. Efstratiadis et al., 2008). The concept of HRUs was introduced by Flugel (1995) to characterize homogeneous areas with similar geomorphologic and hydrodynamic properties. The one-to-one correspondence of HRUs and sub-basins could be considered a specific case, which is however not consistent with the rationale of HRUs, as far as sub-basins have arbitrary boundaries that do not necessarily ensure homogenous characteristics.

3. p. 10276, lines 26-30: "Many studies have reported that distributed models calibrated at the basin outlet are less accurate at interior locations (Anderson et al., 2001; Cao et al., 2006; Wang et al., 2012), but the extent of the error and uncertainty is unknown due to the computational expense needed to explore this issue." To my opinion (and my experience), the accuracy of predictions of runoff at interior points mainly depends on the local characteristic of the basin. In the case of strongly heterogeneous basins, it is far from reasonable to make estimations based on the lumped information obtained at the basin outlet. On the other hand, if the key properties of the basin that influence runoff generation (e.g., permeability, vegetation, slope) do not vary significantly, such estimations could be quite reliable. However, the latter is not the rule.

4. p. 10277, lines 1-2: "... for an alternative climate, which is required in climate change impact studies". My impression is that climate change studies over broader areas refer to systematic deviations from the average climatic conditions, and not to "alternative climates".

C4841

5. p. 10277, lines 22-24: "Water resources from the basin are shared by Afghanistan and Pakistan and serve as a water supply source for more than 20 million people." How significant are water abstractions in this basin? Are they accounted for in the modelling scheme? Are there any important regulations that modify the flow regime across the basin?

6. Section 2 (Study area): Here you should add information about the flow stations and the available data, and also provide synoptic statistical information about the hydrological characteristics of the basin, e.g. mean annual flow at the seven stations of interest, mean precipitation over the sub-basins, etc. (you can add these data to Table 1). It would also be useful to refer to the physiographic properties of the basin and the dominant runoff mechanisms, which are essential to interpret the model results and plausibility of the optimized parameter values. It is also essential to explain to which extend is this basin heterogeneous, thus justifying the implementation of each parameterization approach and better explain the model results.

7. p. 10279, lines 4-6: "However, in this particular study daily hydrologic model simulations can only be compared against available monthly streamflow records". It is not clear whether monthly streamflows are averaged values of daily (or hourly) observations or instantaneous values, gather e.g. from direct flow measurements. Such clarification is very important.

8. p. 10279, lines 18-20: "No matter the parameterization scheme, the model structure follows the climate input grids, i.e. the hydrological water cycle within each grid cell is modelled separately." In the revised paper, I suggest also employing the simplest of model configurations, assuming a lumped structure for both model inputs and parameters (i.e. using the averaged precipitation over the basin). This classical lumped approach considering 15 (or less) parameters would provide, in theory, the optimal results at the basin outlet with minimal computational burden, to be considered as "baseline scenario".

C4842

9. p. 10279, lines 21-24: "The parameter complexity will vary depending on the calibration experiment being conducted, but for each experiment regardless of the parameterization, the optimization is implemented 50 times using the GA algorithm to explore parameter uncertainty." Parameter uncertainty is a combined effect of multiple causes, one of which is inefficient calibrations (i.e. calibrations trapped to local optima). Even the use of robust and sophisticated evolutionary algorithms cannot remedy this problem, especially when a large number of parameters are considered. However, there are also other sources of parameter uncertainty, associated with data errors, unknown boundary conditions, etc. In this context, I propose avoiding the general term "parameter uncertainty" and focus to "calibration uncertainty", which is very well represented in your work, by implementing 50 independent runs for each optimization problem.

10. Section 3.1 (Multisite calibration): There are some important issues that are mentioned in next parts of the document, yet they should be also highlighted in this section. In order to better follow the modelling experiment, is essential to explain the sequence of sub-catchments, which strongly affects the outcomes of stepwise calibration (thus I propose moving Fig. S1 from the supplement to the main text). Another missing issue is the lack of overlapping data periods among most of stations, which is a bad coincidence, since this weakens the multisite calibration approach: in fact, you do not have simultaneous information on the basin responses, which would allow account for the heterogeneity of the associated hydrological processes.

11. p. 10281, lines 23-27: ".. the lumped version of the HYMOD_DS contains a single, 15-member parameter set applied to all model grid cells. The semi-distributed conceptualization of HYMOD_DS contains a single parameter set for each sub-basin, totaling 75 parameters. In the distributed parameterization ... the number of parameters requiring calibration reaches 2400." Here it is worthy reminding that for the transformation of rainfall to hydrograph at the basin outlet, only 5 to 6 parameters can be identified on the basis of a single observation set (cf. Wagener et al., 2001). Under this premise, the number of parameters for the lumped scheme is realistic, taking into account that

snow, glacier and flow routing processes are also modelled. For the semi-distributed approach, the number of parameters remains realistic, since external information is increased by accounting for interior flow data in calibrations. However, the distributed approach, with 2400 parameters to be optimized, is far from acceptable, and any attempt to interpret the outcomes of calibration is unreasonable.

12. p. 10284, lines 11-12: "Monthly streamflow observations for seven locations in the Kabul River basin (Fig. 1) were gathered between calendar years 1961–1980". The same equation with comment 6: why monthly flow data and how are these data extracted?

13. p. 10285, line 14-15: "The overall model structure of the HYMOD_DS and its 15 parameters are described in Fig. 4 and Table 2 respectively." The feasible ranges that are employed for the model parameters are extremely large thus resulting to huge parameter uncertainty (at least, a priori uncertainty). For instance, the maximum soil moisture capacity ranges from 5 to 1500 mm. I would expect that an experienced hydrologist would propose much more narrow bounds, taking into account the physical interpretation of those parameters and the local characteristics of the specific study area. I strongly believe that a hydrological model is not a mathematical game, and calibration is not a black-box exercise. In contrast, model parameters should always have some correspondence to the physical properties of the basin, which is yet not reflected in this work. In addition, a substantial reduction of feasible ranges would be beneficial for the calibration effort, which is tremendous (1000 parallel processors running for 7 days!).

14. p. 10286, line 15: The Hamon method for PET estimations is not widely known. Please, provide one or two sentences with a very synoptic description of this method (rationale, input data). Is this method suitable for the climatic regime of the study area?

15. p. 10290, lines 16-17: "High accuracy holds even under the Lump_Outlet, which is somewhat surprising given the spatial heterogeneity of the basin." I do not agree

that this is a surprising conclusion. The lumped configuration of HYMOD_DS has 15 parameters, which are far from sufficient to represent hydrographs of any complexity.

16. p. 10290, lines 25-27: "...the HYMOD_DS significantly overestimated streamflow at Daronta and underestimated flow at three sites in the eastern part of the basin" This is a strong evidence of the herogeneity of the basin. Please, provide some information on the properties of the basin (e.g. geology) that would justify these differences.

17. p. 10292, lines 7-9: "On the other hand, temperature clearly shows an upward trend for both radiative forcing scenarios. The average changes in annual temperature are +2.2oC and +2.8oC for RCP4.5 and RCP8.5, respectively". Which are the impacts of such difference in PET estimations?

18. p. 10292, lines 17-19: "For the historical time period, all calibration schemes match the observed climatology at Dakah well, but monthly streamflow is underestimated in most of months at Kama and Asmar under the basin outlet calibrations". If I understood well, you used as meteorological inputs the average projections of the 36 climate models during the period of observations. In that case, it is not clear whether the underestimation of monthly flows is due to inappropriate representation of past precipitation and temperature data by climate models or due to inappropriate calibrations at the specific flow stations. For this reason, it is essential providing results on model bias (apart from NSE and KGE).

19. p. 10293, lines 26-27: "Another clear point is that the uncertainty resulting from different climate change scenarios substantially outweighs that from parameter uncertainty." This is of course a very important conclusion, and would deserve further discussion about the misuse of such scenarios as "deterministic" projections.

20. p. 10294, lines 10-14: "While no observed data is available against which to compare the results, an inter-model comparison is useful to distinguish the differences between the parameterization schemes." Since observed flood data are missing, these comparisons are little safe. You may use them in the context of a theoretical calibration

exercise, but definitely not for decision-making purposes.

Technical corrections:

1. p. 10278, lines 23, 24: Please, change to read "Sutcliffe".

2. p. 10292, line 18: Term "observed climatology" is unclear. Climatology is defined as "the study of climate", while climate is defined as "as weather conditions averaged over a period of time" (http://en.wikipedia.org/wiki/Climatology).

3. p. 10292, line 21: Similarly, term "historical streamflow climatology" is not valid. I suppose that you refer to average monthly flow data?

4. p. 10304, Table 1: Please, use common symbols for dates, e.g. YYYY/M or M/YYYY (not YYYY.M).

5. p. 10316, Fig. 10: The coefficient of variation of which quantity is represented in the graphs? (similar for Fig. 12).

References:

Beven, K. J.: Changing ideas in hydrology – The case of physically-based models, J. Hydrol., 105(1–2), 157–172, 1989.

Efstratiadis, A., Nalbantis, I., Koukouvinos, A., Rozos, E., and Koutsoyiannis, D.: HYDROGEIOS: a semi-distributed GIS-based hydrological model for modified river basins, Hydrol. Earth Syst. Sci., 12, 989-1006, doi:10.5194/hess-12-989-2008, 2008.

Flugel, W.-A.: Delineating Hydrological Response Units (HRU's) by GIS analysis for regional hydrological modelling using PRMS/MMS in the drainage basin of the River Brol, Germany, Hydrol. Processes, 9, 423–436, 1995.

Nalbantis, I., Efstratiadis, A., Rozos, E., Kopsiafti, M., and Koutsoyiannis, D.: Holistic versus monomeric strategies for hydrological modelling of human-modified hydrosystems, Hydrol. Earth Syst. Sci., 15, 743-758, doi:10.5194/hess-15-743-2011, 2011.

Wagener, T., Boyle, D. P., Lees, M. J., Wheater, H. S., Gupta, H. V., and Sorooshian, S.: A framework for development and application of hydrological models, Hydrol. Earth Syst. Sci., 5, 13-26, doi:10.5194/hess-5-13-2001, 2001.

———————————————

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 10273, 2014.

C4847