

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.

Anonymous Referee #2

Received and published: 9 December 2014

Calibration approaches for distributed hydrologic models using high performance computing: implication for streamflow projections under climate change

Wi et al.

General comment: The authors study the effect of different calibration methods on discharge prediction under current climate and future climate projections in a macroscale catchment of Afghanistan. They apply a fully distributed version of the rather empirical HYMOD model. For testing the performance of their various calibration methods,

[Full Screen / Esc](#)

[Printer-friendly Version](#)

[Interactive Discussion](#)

[Discussion Paper](#)



they use a set of different procedures. In a first setup, they follow a multi-site model calibration with a pooled and stepwise calibration approach. In a second step they use the best performing approach from the first step and consider three different spatial modes of parameterization, spanning from lumped over semi-distributed to fully distributed ones. In a third step, they then tested the model's capability of simulating interior catchments by using only data from the catchment's major outlet and the best performing approaches resulting from step one and two. Finally, the authors assess the uncertainty of climate change impact on projections of water availability and flood risk by applying various calibration approaches to climate change scenarios.

The paper is well written, easy to follow, clearly structured with interesting results, shown in high-quality figures. However, I see one major limitation of the paper that leads me to ask for at least minor, if not major revisions: there is not much of a scientific discussion. The authors discuss their results most of all "with themselves" by comparing the various results they obtained. The discussion is short of any discussion with findings by other authors (e.g. on P10294 L3 the authors cite other work for the first time in the results and discussion section. This is on the last page of an eight pages long results and discussion section). There is plenty of published work about the effect of parameterization and their spatial variation, lumped vs distributed calibration approaches, performances of models in simulating interior gauges not considered in calibration, see for example results of the DMIP and LUCHEM projects, amongst others. Additionally, climate change effects on discharge in Central Asian catchments has been in the focus of many, many studies – how do these related to the results obtained here?

Specific comments:

Title: High performance computing is mentioned in the title, but hardly presented in the method section, and not at all in the discussion. HPC in this paper is used as a technique to be able to run a large number of models, but it is not in the center of research as indicated by the title. I suggest to change the title.

C5591

[Full Screen / Esc](#)

[Printer-friendly Version](#)

[Interactive Discussion](#)

[Discussion Paper](#)



P10276 L26 There are a number of papers which looked at model performance when excluding/including interior gauging stations during model calibration and validation; see e.g. the DMIP projects (Reed et al., 2004; Smith et al., 2012), the LUCHEM project (Breuer et al., 2009) or work by others (Andersen et al., 2001; Lerat et al., 2012).

P10277 L1 You might want to have a closer look to a recent paper by Exbrayat et al. (2014) who investigated the contribution of uncertain model structures versus the impact of uncertain climate change projection to the global predictive model uncertainty. Even though not directly comparable to what the authors show here, it is worth considering and can be used in the discussion, which is lacking other researchers work (see general comment).

P10277 L18 I do not agree that HPC is so new in hydrological modeling. I rather think that many researcher use HPC without highlighting it. Also in the work presented here, HPC is a tool that is used, but not a method that is further developed or presented in detail.

P10278 L3 Is the annual precipitation 475 mm or are the 475 mm the 70% of total precipitation? Overall, the study area description is very short. Some more information about topography, soils/geology, flow characteristics, specific discharges from the sub-catchments, and land use/management would be helpful to better understand some of the results. How about irrigation? Is it an important land management and if so, how did you deal with water abstraction. Looking at the often poor model performance in the western part of your catchment around Kabul I assume that missing information on water abstraction substantially influences your model performance.

P10278 L21 Should it not be “a genetic algorithm” as there are many kinds of genetic algorithms available for model calibration? Or you should state “the genetic algorithm introduced by Wang et al. 1991”.

P10279 L5 I wonder how these monthly streamflow values were calculated if not from daily measurements. If there are only monthly data available, I also wonder if the NSE

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



is the best choice for goodness of fit criteria. Nevertheless, I like the argumentation given for choosing NSE but suggest to also mentioning here the use of KGE as another goodness of fit criterion for model evaluation (so far, KGE is introduced in chapter 5 in the discussion and not in the methods section).

P10282 L3 Are the numbers correct? The page before you present 15, 75 and 2400 parameter values being searched for in the various spatial set ups. Should it then not be 15x100 and 75x100? And why is 2400 multiplied by 200 and not by 100 as the others? Even though you state in the next sentence that the population/generation sizes were supported by convergence tests, the generation of numbers given here remains unclear.

P10283 L11 step-wise (not step-wide)

P10284 L12 the period “1960-1981” better covers all available discharge measurements given in Table 1.

P10294 L6 is shown in. . . (not was shown)

Section 6 Conclusion P10295 L8 until P10296 L16 This is an extended summary of the results presented rather than a conclusion of the work. I think more effort should be put into real conclusions – what do we learn from the study, what are suggestions for future research, are results transferable to other regions or modelling approaches?

Sections 5.2 and 5.3 The model performances for the upper subcatchments Kama and Asmar are generally very good. This is the same for Dakha (Figs 6 and 7). Glaciers have the largest extend in these subcatchments and I assume that they therefore contribute large volumes of water to total discharge at Dakah. Further, I assume that western catchments contribute only minor to total discharge as rainfall input is comparatively low (information on specific discharges for the various subcatchments would be helpful for a quick comparison). As you optimize your model using NSE, with NSE putting emphasis in matching peak flows, it does not come as a surprise to obtain good

[Full Screen / Esc](#)

[Printer-friendly Version](#)

[Interactive Discussion](#)

[Discussion Paper](#)

results for Dakah as long as subcatchments Kama and Asmar are calibrated sufficiently well.

Furthermore, the model performance of the ungauged sites Kama and Asmar are often very similar. Looking at the choice of stations that you treated ungauged and the general location of available gauging stations, I wonder why you have selected the Kama and Asmar, which belong to the same eastern area of the catchment. Why have you not selected the one in the west as a second interior test station (i.e. Daronta), or at least two subcatchments which are not draining into each other (e.g. Chaghasari and Asmar) and therefore being more independent than Kama and Asmar.

Section 5.4 Do you assume constant glacier volume to be discharging or are glaciers prone to glacier melt, resulting in smaller volume and spatial extend in the future and during your climate change simulation period. What are the expectations in glacier extend for the end of your simulation period in your catchment? Are calibrated model parameters still valid under these new boundary conditions? I expect not, as glacier melt is an important process, described by various parameters (Table 2) and needs rigorous calibration.

S2 Please describe the meaning of abbreviations in the legend or figure caption

S8 Is this a simulation of the 100 yr flood event, at least this is what I understand from the text (P10294 L6 and following).

References

Andersen, J., Refsgaard, J. C. and Jensen, K. H.: Distributed hydrological modelling of the Senegal River Basin – model construction and validation, *Journal of Hydrology*, 247(3–4), 200–214, doi:10.1016/S0022-1694(01)00384-5, 2001. Breuer, L., Huisman, J. A., Willems, P., Bormann, H., Bronstert, A., Croke, B. F. W., Frede, H.-G., Gräff, T., Hubrechts, L. and Jakeman, A. J.: Assessing the impact of land use change on hydrology by ensemble modeling (LUCHEM). I: Model intercomparison with

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

current land use, *Advances in Water Resources*, 32(2), 129–146, 2009. Exbrayat, J.-F., Buytaert, W., Timbe, E., Windhorst, D. and Breuer, L.: Addressing sources of uncertainty in runoff projections for a data scarce catchment in the Ecuadorian Andes, *Climatic Change*, 125(2), 221–235, doi:10.1007/s10584-014-1160-x, 2014. Lerat, J., Andréassian, V., Perrin, C., Vaze, J., Perraud, J. M., Ribstein, P. and Loumagne, C.: Do internal flow measurements improve the calibration of rainfall-runoff models?, *Water Resour. Res.*, 48(2), W02511, doi:10.1029/2010WR010179, 2012. Reed, S., Koren, V., Smith, M., Zhang, Z., Moreda, F., Seo, D.-J. and DMIP Participants, and: Overall distributed model intercomparison project results, *Journal of Hydrology*, 298(1–4), 27–60, doi:10.1016/j.jhydrol.2004.03.031, 2004. Smith, M. B., Koren, V., Reed, S., Zhang, Z., Zhang, Y., Moreda, F., Cui, Z., Mizukami, N., Anderson, E. A. and Cosgrove, B. A.: The distributed model intercomparison project – Phase 2: Motivation and design of the Oklahoma experiments, *Journal of Hydrology*, 418–419, 3–16, doi:10.1016/j.jhydrol.2011.08.055, 2012.

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

HESSD

11, C5590–C5595, 2014

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

