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Hydrology and Earth System Sciences Discussions
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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.

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We would like to thank the editor, Dr. Efrat Morin, and the two referees, Dr. Andreas Efstratiadis and an anonymous reviewer, for their constructive suggestions on our manuscript. The comments greatly improved our manuscript. Please find detailed answers to all the comments of the reviewers. We also enclose a word file in which all the changes are highlighted to be easily tracked.

Response to Anonymous Referee #2

General comments:

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?

Thank you for pointing out this. We also realized that there were not much discussion in the section "Results and Discussion". To try to follow the reviewer's suggestion, we expanded our discussion. First, we decided to focus on our results in the result section and change the paper's structure accordingly. Now we combined the discussion section with the conclusion part. Also, we expanded our discussion by introducing additional references in relevance to our work as suggested by the reviewer. Please find the revisions made in the section "Discussion and Conclusion" and detailed answers to all the specific comments in the following.

Specific comments:

0 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.

We understand your concern. We have changed the title to highlight our focus on a poorly gaged basin (which we feel is the more important emphasis of this work anyway). However, we do feel that the use of high performance computing is an important component of this work, so we tried to emphasize the necessity of exploiting parallel computing power to implement this kind of study in the abstract:

“To address the research questions, high performance computing is utilized to manage the computational burden that results from high-dimensional optimization problems.”

0 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).

Thank you. We have added the recommended references.

0 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).

Thank you for suggesting this useful reference. We expanded our discussion with the suggested reference. “These results agree with other studies showing the dominance of GCM uncertainty in future hydrologic projections (Chen et al., 2011; Exbrayat et al., 2014). . . . In addition to the uncertainties surrounding model parameters and future climate explored in this study, there is also significant uncertainty in streamflow projections stemming from structural differences between applied hydrologic models, which can be especially pertinent where robust calibration is hampered by the scarcity of data (Exbrayat et al., 2014). Further, the residual error variance of hydrologic model simulations would increase the effects of hydrologic model uncertainty as compared to that of the climate projections (Steinschneider et al., 2014). These issues need to

be addressed in future work for exploring a comprehensive uncertainty assessment of climate change risk for poorly monitored hydrologic systems.”

0 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.

We understand and have removed the language suggesting HPC is new in hydrological modeling. While we still feel that the use of HPC is uncommon and adds new possibilities for research questions, we agree that we are using HPC as a tool – it is not the focus of our study.

0 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 subcatchments, and land use/management would be helpful to better understand some of the results.

We dropped the number in the text to avoid any confusion caused by that. The number was meant to be for annul precipitation and is now provided in the updated Table 1. Figure 1 has been updated with more information (topography, soil types, and vegetation cover). We expanded the study area description accordingly.

Figure 1. Kabul River Basin.

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.

We completely agree with reviewer’s concern about human interfere. The Kabul River has the largest flow of all of Afghanistan’s rivers, but it can irrigate only a limited area because there is little land suitable for agriculture in the Afghan part of the basin (Ahmad and Wasiq, 2004) – for the most part, the river flows through mountainous or rocky areas. According to World Bank, (2010), about 2,927 km2 (4.3% of the total basin area) is agricultural land and the average annual flow of the Kabul River is approximately 24,000 million cubic meters (MCM). Irrigation is a large water demand since the annual water demand estimate for the agricultural use is about 2,000 MCM, or about 8.3% of the total annual flow. In our hydrologic modelling process, the water consumed by irrigated croplands is implicitly accounted for by the evapotranspiration module. We note that the degree of irrigation impact during the time frame used for calibration (1960-1981) is likely much smaller than the current level.

The Naglu dam, which is located in the western part the Kabul River basin (upstream of the Daronta streamflow gage), forms the largest and most important storage among dams in the basin (World Bank, 2010). The live storage of the Naglu dam is 379 MCM. We expect that using monthly data for calibration somewhat reduces the bias from human interference, particularly the daily operations of Naglu dam. Nevertheless, the calibration results for the gage below this dam (Daronta), and to a lesser extent the basin outlet (Dakah), should be approached with caution. Given that a majority of the gages examined in this study are on an underdeveloped branch of the Kabul River, issues of human interference on calibration are somewhat mitigated. We also note that the poor performance at Daronta is likely due in part to the impacts of water abstraction and the operation of Naglu dam.

This information has been provided accordingly in the text. "Similar to most other hydrological models (Efstratisdis et al., 2008), HYMOD_DS is not designed to model water abstractions for agricultural lands and dam operations within the basin. According to World Bank (2010), water demand for agricultural use is about 2,000 MCM (million cubic meters), or about 8.3% of the total annual flow. The Naglu dam (Figure 1) upstream of the Daronta streamflow gage forms the largest and most important reservoir in the basin, with an active storage of 379 MCM. In our hydrologic modelling process,

the water consumed by irrigated croplands is implicitly accounted for by the evapotranspiration module. We note that the degree of irrigation impact during the time frame used for calibration (1960-1981) is likely much smaller than the current level. We also expect that using monthly data for calibration somewhat reduces the bias from human interference, particularly the daily operations of Naglu dam. Nevertheless, the calibration results for the gage below this dam (Daronta), and to a lesser extent the basin outlet (Dakah), should be approached with caution. Given that a majority of the gages examined in this study are on an underdeveloped branch of the Kabul River, issues of human interference on calibration are somewhat mitigated."

0 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".

We made this clearer as suggested.

0 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 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).

Unfortunately, the only observations that are available for public use are monthly. There is a report (Olson and Williams-Sether, 2010) clarifying that each monthly streamflow is the mean of the daily values for the month, and monthly values are calculated from daily values for all complete months of record. However, the daily values are not made available because there are political issues surrounding the trans-boundary use of the river's waters and potential projects planned on the river.

We have added the following details in the manuscript to clarify the immediate question regarding the data: "Streamflow data were not collected in Afghanistan after September 1980 until recently because stream gaging was discontinued soon after the Soviet invasion of Afghanistan in 1979 (Olson and Williams-Sether, 2010). Though measurements were taken at a daily time step, data are only made available for public use at monthly aggregated levels, calculated using the mean of the daily values."

We acknowledged the limitation of the use of NSE for a model evaluation metric by writing this: "However, in this particular study daily hydrologic model simulations can only be compared against available monthly streamflow records, reducing the number of viable objectives against which to calibration. That is, statistics representing peak flows, extreme low flows, and other daily flow regime characteristics often used in multi-objective optimization approaches are unavailable. We believe that the use of a monthly NSE value as a single objective, while coarse, does not inhibit our ability to provide insight into the research questions posed."

Also, we now introduce the KGE earlier in the Methods section to make clear that we are considering more than just the NSE for model diagnostics.

0 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.

We set up different numbers of population and generation in the GA algorithm according to the complexity of parameterization scheme. For instance, for the lumped parameterization, the number of parameter to be optimized is 15 and we considered 150 parameter sets. Those 150 parameter sets evolve through 100 generations, and the result of our convergence test showed a convergence while going through 100 generations. For the distributed parameterization scheme, there are more number of parameters to be calibrated. We considered 2400 parameter sets to calibrated 2400

parameters. Although it can be argued that having 2400 parameter sets to optimize 2400 parameters is not enough, we confirmed from the convergence test that this calibration setup shows a convergence behavior with 200 generations. Below, we enclosed the convergence test results.

GA convergence for the semi-distributed parameterization scheme with 750 parameter sets (population) and 100 iteration (generation)

GA convergence for the distributed parameterization scheme with 2400 parameter sets (population) and 200 iteration (generation)

0 P10283 L11 step-wise (not step-wide)

Done.

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

Yes, you are right. We changed it.

0 P10294 L6 is shown in: : : (not was shown)

We corrected it.

0 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?

As suggested, we tried to focus on the points that should be addressed in the conclusion part.

0 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 con-

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

We updated Table 1 with more contents including the information on specific discharges for the sub-watersheds. In our study, we always treated Kama and Asmar as ungagged sub-watersheds, which means that we never tried to calibrate those two sites. All the available data at those sites were used for the validation purpose only. Dakah (the basin outlet) is the one against which the model calibrated. One of the main ideas we try to show in Sections 5.2 and 5.3 is that the calibration based on only the basin outlet does not provide a good performance at Kama and Asmar, while the pooled calibration does.

0 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.

The Government of Afghanistan with the support of the international donors (e.g. The World Bank) has developed comprehensive plans for the development of new hydropower projects, irrigation schemes and rehabilitation of old schemes on various rivers including the Kabul River (IUCN, 2010). Recently, Afghanistan and Pakistan reached an agreement in working on a 1,500MW hydropower project on Kunar River as part of the joint management of common rivers between the two countries (DAWN, 2013).

For this study, Kama and Asmar were chosen and treated as ungagged sites in the processes of multisite calibrations because they align with the potential dam project.

This information has been provided accordingly in the text. "The Government of Afghanistan has developed comprehensive plans for new hydropower projects on the Kabul River owing to its advantageous topography for the development of water storage and hydropower (IUCN, 2010), and recently reached an agreement with the Pakistan government to work on a 1,500MW hydropower project on the Kunar River (one of major tributary in the Kabul River basin) as part of the joint management of common rivers between the two countries (DAWN, 2013). . . . Kama and Asmar stations are treated as ungaged sites because they align with the potential dam project on the Kunar River tributary."

0 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.

The hydrologic model (HYMOD_DS) used in this study does account for the changes in volume but has no ability to trace explicitly the spatial extend of glaciers. At the beginning of the simulation we were informed by the glacier volume (the amount water stored in the glaciers) which is provided by RGI3.2 and the area-volume relationship. A simple and possible way to trace the glacier extend from this study is to back-calculate the area with volume remaining at the end of simulation using the area-volume relationship. The model parameters related to the temperature-index glacier model stay the same once those are calibrated. Therefore, water from glacier melt with respect to a temperature above the threshold temperature will be same as long as glacier keep existing. We agree that it is hard to expect the calibrated parameters to be valid under new glacier conditions. For our 20-year historical model simulation, we checked that the glacier volume decreases due to the ablation of glaciers larger than accumulation in the sub-watersheds that produce annual total flow larger than annual total precipitation as shown in the new Table 1. We argue that the high ratio of streamflow to precipitation is unrealistic and might be caused by error in precipitation data used in this study since precipitation measurement in high mountain areas is highly uncertain (Immerzeel et al., 2014). What we checked for the 20-year historical simulation and 30-year future simulation is that glaciers still stored enough water at the end of the simulations. In our discussion for future work, we note the necessity of exploiting remote sensing and satellite products with which the evaluation of distributed hydrologic models with respect to model internal processes (e.g. snow, evapotranspiration, and glacier) becomes possible. .

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

We put the description in the figure caption.

0 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).

We assumed that the reviewer meant Figure S6, not S8. No, this figure is showing the variability of optimum parameters derived from 50 trials of semi-distributed and distributed pooled calibrations. Here, we tried to explore the variability of 100-year flood estimates using 50 calibrated parameter sets for each calibration approach. Specifically, every time when the model was run with an optimum parameter set, we estimated the 100-year flood using the Log-Pearson III distribution for three locations (the basin outlet and 2 ungagged sites). With 50 100-year flood estimates for each calibration approach, we then examined the influence of the parameter variability on the flood estimates by comparing the flood estimates resulting from two calibration approaches.

Thank you.

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While we were revising our manuscript, references listed below were added accordingly in the text.

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Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/11/C5932/2015/hessd-11-C5932-2015-supplement.zip

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