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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Future evolution and uncertainty of river flow regime change in a deglaciating river basin" *by* Jonathan D. Mackay et al.

Anonymous Referee #1

Received and published: 11 November 2018

Dear editor, dear authors,

The authors present a climate impact study including uncertainties from different sources of uncertainties. They included an ensemble of RCPs and climate models as well as different model structures into the model chain. Additionally they used many objectives for calibration and for impact assessment (even if the terminus "multi-objective" or "many-objective" was never used in the presented paper). Although the methods of the study need to be more elaborated, the paper presents a comprehensive work. On larger spatial scales multi-impact-model applications have become very common. The authors try to implement such an approach by choosing different, commonly applied

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process presentations for the snow/ice processes and for runoff generation processes. For example, runoff generation is represented by two different linear storage methods. However, one limitation with regard to the choice of process representations is that they are, like in the mentioned example, quite similar looking at the variety of process representations possible. In general, the paper is quite long and has a lot of figures. The authors should try to limit word count and balance the length of the different chapters (the discussion is too short compared to other chapters).

I have four main points for the authors to consider.

1. Shortcomings in stepwise calibration The authors calibrate their model in two steps. First, they used different model structures and objectives for calibrating ice/snow processes (TIM) and second they applied different model structures and objectives for calibrating runoff generation (ROC). By calibrating in a first step only different TIM structures, the authors assume that the runoff generation processes will not interact with the TIM processes, which might be true. But I see a problem in the second step of calibration where they use only one TIM representation to further calibrate their model for runoff generation signitures. By this, the calibration is relying only on one set of TIM parameters and its water flux characteristics. I doubt that the selected parameter sets will represent the best possible parameter sets for runoff generation processes. The assumption that the selected 24 different TIM representations will not interact with a particular ROC is in my eyes not correct. I suggest to do a ROC calibration based on all unique TIM representations.

2. Longer periods for impact assessment In climate impact studies it is recommended to take at least 30 years of data for the reference period and a 30 years long future period for analysis. The authors take only 25 years. This limitation should be mentioned in the discussion of methods. Later on they even present their results on climate change impacts on the basis of 10 years slices. This is not acceptable. It is likely that e.g. the results on different uncertainty sources is biased and that the uncertainty coming from the “climate” is overestimated.

3. Separating impacts for different RCPs The authors present quite often a mean over both selected RCPs in the result section and partly in the discussion section. I suggest to show the results for both RCPs separately (or only one) as the results strongly depend on the warming level.

4. Extend discussion The authors discuss the consequences of their findings e.g. with regard to threats for infrastructure and changes in water availability. What I miss is the link of their findings to other studies and actual debates in science. The study would also benefit from a critical discussion of advantages (novelty) and limitations of applied methods.

Details:

Page 2, Line 21: Is “GHM” a common abbreviation for this type of models? This abbreviation is often used for “Global Hydrological Model”

Page 2, Line 30: Upper Yellow is written with capital letters, but upper Indus not

Page 2, Line 31: put “river” before the citation

Page 2, Line 31-33: Why have you decided to include another reference for Q90 that for Q10? Some of the previous mentioned studies (e.g. Vetter 2015) have also analyzed Q90.

Page 3, line 1: “:” could be replaced with “which are”

Page 3, line 27: change “21st century river flows” to “21st century river flow projections”

Page 3, line 28: instead of “emission scenario (ES)”, I suggest to use “Representative Concentration Pathway (RCP) “

Page 3, line 35: instead of “large basin-scale” use “regional-scale”

Page 4, line 3-5: I don’t think, that all of the hydrological models used in the mentioned studies match the terminology “Computational glacio-hydrological models”. I suggest

to use “impact model” instead.

Page 4, line 15: remove “future”

Page 4, line 31: Isn’t it 1988-2011 as mentioned in table A1?

Page 4, line 4: Can you include altitude and/or station name of the “terminus”?

Page 5, line 1-2: instead of “than to” use “compared”. Where does this numbers come from?

Page 5, line 8: give the installation time for AWS1!

Page 5, line 5-13: Please indicate all the climatic data which was used in your study. I think AWS1 was installed between 2009 and 2011. So there must be additional climate data. If only AWS1 was used, please remove AWS3 and AWS4 from Fig.1.

Why are the uncertainties coming e.g. from precipitation measurements/bias correction/downscaling not included in your study, as the authors did for other objectives (like runoff, snow coverage)?

Page 6, line 14: Replace “to 2100” by “until 2100”.

Page 6, line 16: GCM means General Circulation Model and not Global Circulation Model.

Page 6, line 27: Change “the RCP2.6 ES” to “RCP 2.6”

Page 6, line 21-30: I recommend to make a table where all GCM-RCM combinations are shown which were used in this study. This table could be put to the appendix.

Fig.1: The 0.11° EURO-Cordex grid could be indicated in the figure. Page 6, line 31-32: It is common to use at least 30 years, when the climate between two different data sets (here measured and simulated) is compared. Otherwise climate variability is becoming more dominant. If possible, please use 30 years, otherwise this limitation should be clearly mentioned.

Page 6, line 31 - Page 7, line 6: The authors try to analyze the skills of the RCMs by comparing them to the observed climate, but they do it in a very subjective (visual) way, without any statistics or a clear selection criteria. The authors state “Overall, the GCM-RCM performance is good . . .” without any proof or definition, what “good” means. The Authors should explain, why they have not defined any levels of acceptability, as they did for the other inputs.

Fig.2: It is hard to read this figure, especially regarding precipitation. The lines are too close. I guess this is because the extreme precipitation events from climate models are also included. Maybe the 99% quantile is sufficient? Also the ECDF curve for precipitation should start somewhere above the X-axes cross section, indicating the number of days without precipitation.

Fig.3: Instead of writing " based on the projections from 1 of the 14 GCM-RCMs with the RCP8.5 ES” you can give the exact name of the GCM/RCP combination which you refer to.

Page 7, line 19-20: It is common in impact studies to use at least 30 years for reference period and scenario period, respectively.

Page 7, line 25-28: Why don’t you calculate deltas as 25-years (or 30-years) moving window? Then you would not need any interpolation.

Page 9, line 5-8: You could put the information about the number of sub-samplings directly to step 5. There are two possible (re)sampling schemes: with or without replacement. Why have you chosen sampling without replacement?

Page 9, line 12: What is the resolution of you model domain?

Page 10, line 3: Are these new methods to calculate soil infiltration and evapotranspiration? Can you name the methods used to calculate infiltration and evapotranspiration? I can’t find/access the given reference Griffiths et al., (2006) in the internet.

Page 10, line 5: given reference Ponce (2014) is gray literature. Chapter is also not

given.

Page 10, line 11: Please be more precise here. What kind of observed data was used? How long are the observational periods.

Page 10, line 31: “The majority of signatures were selected from past studies . . .” please give a reference for this statement.

Page 12, line 5: Data description used to create 12 snow and ice signatures is missing

Page 12, line 28: Why always 5000 runs? E.g. for TIM1 you have 2 parameters, but for TIM3 5 parameters. Do you think that this makes a difference?

Page 13, line 5:”Accordingly, only those TIM compositions that captured this signature within the LOA were considered and the rest were discarded.“ I have the feeling that this is a very strong assumption. What would be the consequence of having a perfect/better DEM and as a result reducing observational uncertainties? You would probably need to discard most of your simulations.

Page 13, line 10: “. . . best captures the 11 remaining . . .” should be 10 instead

Page 14: The whole chapter 2.5.3 is about results and not about methods.

Page 14, line 23: “use model runs” instead of “model chain runs”

Page 14 line 27- Page 16 line 15: This paragraph should be shortened and transferred to the section on model description.

Page 16, line 19-20: It is common to take at least time slices of 30 years for climate impact assessment. By using only 10 years, you will increase the influence of climate variability" and consequently you are overestimating ES uncertainty. I also ask myself whether you have put the duplicated TIM and ROR parameterizations into the ANOVA? How many unique parameterizations do you have for a given TIM structure and a given ROR structure, respectively?

Page 16, line 23: Please check your calculation. I get 1x10ˆ8 (102.850.020)

Page 18, line 3: I suggest to write “[...] consistently show an increase relative to the reference period [. . .]” instead of “consistently predict an increase relative to the recent past”

Page 18, line 4: “The largest increases are predicted [. . .]” Please always use project and not predict for climate projections.

Page 18, line 7-8: Please present climate impacts separate for each RCP. Averaging over both RCPs is not very common.

Page 18, line 14: correct “Figure 5l”

Page 18, line 15: change “The sign of change” into “The direction of change”

Page 18, line 28: Maybe I missed that, but what exactly is “annual snow coverage”?

Fig.7: Why are the snow coverage confidence bands only positive compared to the mean? Does snow coverage also include ice, or does it only count as snow coverage if there is snow on the glacier?

Page 20, line 12: improve English: “The maximum coverage simulations show higher than . . .“

Fig 5. instead of “recent past “ use reference period

Page 20, line1: “projects” instead of “predicts”

Line 20, line 1-10: Again: I guess these results are highly dependent on the warming levels and it would be good to either choose one RCP or to analyze both separately.

Fig. 11: Could you please present the results in mm and not in km^3, because mm is more intuitive and easier to compare to other studies. Is snow included in rainfall? In previous plots you named it always “total precipitation”.

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