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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Contribution of snow and glacier melt to discharge for highly glacierised catchments in Norway" *by* M. Engelhardt et al.

Anonymous Referee #2

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Contribution of snow and glacier melt to discharge for highly glacierised catchments in Norway

by M. Engelhardt, T. V. Schuler, and L. M. Andreassen

Summary of the manuscript

This manuscript presents the application of a conceptual hydrologic model (the name of the model was not mentioned) to three glaciated Norwegian study sites (i) Alfotbreen, ii) Nigardsbreen and iii) Storbreen). The model was calibrated using 10'000 MonteCarlo simulations and selecting the best runs in regard to measured discharge and glacier mass balances, similar to the approaches presented in Konz and Seibert (2010) and Finger et al.(2011). Model results were validated against weekly melt rates

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between 2002 and 2012. Subsequently, the model results are used to quantify the contribution of snow and glacier melt water to discharge from the three study sites. The study concludes by identifying the increase of climate "continentality" from west to east as main driver for differences between the three investigated catchments, rather than catchment size or glacier coverage. The manuscript convincingly presents model set up, calibration and validation. In particular the mutli-variable validation can be highlighted, as the multi variable calibration allows identifying an ensemble of good runs while accounting for the equifinality problem.

The presented topic has been discussed by many authors in recent years, however the study can contribute with two new aspects to the ongoing discussion of snow, glacier and rain contribution to runoff: i) the authors investigate climatic differences across a west – east transect in Norway and ii) the assessment of glacier contribution to runoff may impact the downstream watershed, as the authors briefly state in the introduction. Accordingly, I do think that the topic of this study may be suitable for publication in HESS, after adequate revision regarding the thereafter listed comments. Also, in line with reviewer 1, I also think that additional information is needed to clarify the modeling approach.

General comments:

1) The validation of the model performance could be improved. In my opinion it would be a lot more useful to compare simulated and measured discharge rates directly rather than monthly averages or scatter plots. A direct comparison in form of hydrographs and time series of sonic ranger measurements would reveal how the model actually performs during low flow and high flow. As an illustration of up to 40 years may not be very easy to illustrate in one figure, I would also suggest to discuss model validation for specific time periods (e.g. the 70's, the 80's, 90's ect.)?

2) In the introduction it is briefly mentioned that discharge from glaciated catchments provide water resources to hydropower dams in Norway. This statement is certainly

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true, but was not discussed in more detail and the results of the study could be put into the context of potentially declining water resources for hydropower production. What can be learned from the simulations? And is the model accuracy high enough to assess impacts on water availability for the downstream areas?

3) Structure of the manuscript: In my opinion the validation of model performance should be discussed before presenting the results. Accordingly I would first present all validation figures, then present the estimates of snow, glacier and rain contribution and finally put these results in context of potential effects on downstream water availability.

4) The model uncertainty in regard to the conclusion could be assessed and discussed. For instance, Finger et al. (2012) used the variance within the ensemble of 'good' simulations in order to assess the uncertainty and subsequently performed an analysis of variance to discuss the origin of uncertainty in the conclusions. A similar assessment could be performed here; subsequently the consequences of the results for stakeholders in downstream areas could be discussed.

I believe that if the four points mentioned above are addressed adequately, the study would improve significantly, making it a substantial contribution to the ongoing discussion about snow glacier and rain contribution to downstream water resources. Accordingly, I would also suggest a more focused title, e.g: Assessing changes in the contribution of snow, glacier and rain to downstream water resources in three glaciated Norwegian catchments.

Specific recommendations:

Abstract: Ln2: it is rather the rate of glacier melt that influences stream flow, not the catchment itself Ln 6: does the model have a name? Ln 16: to what does the % refer to? What is the reference for 100%? Ln 23: a general statement how these results contribute to hydrology would be helpful here.

Introduction:

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Ln26: This is a contradictory statement, as your own results show that rain contributes more than glaciers (fig 3) Pg11487, Ln 1: amplified or balanced? If it is both then it should be explained why, otherwise this statement is redundant Ln4: 15% comes from glaciers? What is the reference? And if this has been assessed, did they also estimate contribution for the sites presented in this study? Ln12: many studies demonstrated that in Europe glaciers only melt in late summer, in spring it is primarily snow melt Ln13: Here you may give a number on how much the glacier contribution is in large downstream watershed, as estimated by Huss Ln19: cite original references, the listed references surly did not apply a temperature-index model for the first time Pg:11488, Ln1: 'better results' in regard to what? Ln7: What is the name of the model? Has it been newly developed? Has it been applied before? References?

Study sites

Ln18: As you present also data I suggest the following title: "study site and data" Pg.11489, ln7: Fig2b should come before Fig2c Ln12: is this increase significant?

Methods

Pg 11490, Ln3: Model name? Ln 6: a reference is needed at the end of the sentence, the following sentence is redundant Ln10: valuable in regard to what? Ln16: Units for equation is missing Ln17: S should be rather called deltaS Pg11491, Ln 4: Q should be on the left side Pg11492, entire page: In accordance with reviewer 1, I also think that the model description is poor. Who developed the model, was it used before, on what previous versions/models does it rely on... etc... if the model was newly developed. The authors might also consider submitting a complete description of the model as non-print auxiliar material. Pg11493, Ln1-2: More detail is needed to describe how the model accounts for changes in glacier volume. Pg11494, Ln1: SMB needs to be described in more detail... is it every 100 m altitude band? How long are the seasons? Ect.

Calibration and validation

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Ln24: the model is calibrated the model performance is validated; not the parameters Ln26: Calibration and validation periods are not defined Ln 17 to end: the validation of discharge needs to be explained here as well.

Results

General comment on results: the numbers of increase may not be significant: a climatic change can only be observed between periods of about 30 years, otherwise it might just be due to inter-annual meteorological variability. I strongly recommend revising this entire section. This could be done along with the major comment. Finger et al. 2012 have done something similar. Also, long term trends in discharge may be discussed with measured discharge and compared to modeling results. However, the contribution of snow and glacier is highly interesting and can only be estimated with modeling.

Discussion

The discussion is valuable and interesting, but the results from other studies should be put in context to the presented modeling results. Also, as stated in the major concerns above, the discussion section would improve if an assessment of the uncertainty regarding the estimations of snow, glacier and rain contribution was included. Furthermore, the relevance of the results for downstream water availability could be discussed. Why is this work necessary, what is novel? Pg11498, Ln13: Just because the correlation coefficient for T is biggest for Storbreen does not necessary proof that glacier melt is most sensitive to temperature.

Conclusions:

Pg11500, Ln24: why are differences due to increasing climate continentality? What differences are meant?

References:

Finger, D., F. Pellicciotti, M. Konz, S. Rimkus, and P. Burlando (2011), The value of glacier mass balance, satellite snow cover images, and hourly discharge for improving

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the performance of a physically based distributed hydrological model, Water Resources Research, 47, doi: W07519, 10.1029/2010wr009824.

Finger, D., G. Heinrich, A. Gobiet, and A. Bauder (2012), Projections of future water resources and their uncertainty in a glacierized catchment in the Swiss Alps and the subsequent effects on hydropower production during the 21st century, Water Resources Research, 48, doi: 10.1029/2011wr010733, W02521.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 11485, 2013.