

Interactive comment on “Hydrologic Diversity in Glacier Bay Alaska: Spatial Patterns and Temporal Change” by Ryan L. Crumley et al.

Kristian Förster (Referee)

foerster@iww.uni-hannover.de

Received and published: 18 February 2019

General comments

In their manuscript the authors describe a very detailed study on the glacio-hydrological change in the Glacier Bay, Alaska. The paper is interesting, written very well and fits well into the scope of “The Cryosphere”. The authors elaborated an energy balance snow model (SnowModel) which was extended by soil moisture and evapotranspiration in the framework of an earlier study. Elaborating energy balance approaches is timely and SnowModel is a suitable model to perform such type of analyses. A drawback is the static representation of glaciers. It is clear that dynamic glaciers are only represented in a small number of hydrological models. This topic is still subject to current research

[Printer-friendly version](#)

[Discussion paper](#)



(e.g., Seibert et al, 2018, Hanzer et al., 2018, or WaSiM which builds upon the work of Stahl et al, 2008). However, I would at least expect that this limitation should be discussed in more balanced way in the outlook section (e.g., utilizing more detailed approaches as future outlook).

Another limitation is the calibration of the model. In fact, the model applies “regional” parameters, which is sound, given that hydrological observations are not available in the study area. However, the discussion of the oceanographic data seems detached from the modelling experiment to a certain degree. Using this kind of data is really an asset in my opinion. Later, in Sect. 5, only a visual comparison is carried out. In my opinion, the comparison of the model with the oceanographic data (fresh water volume) could be considered in the calibration section (Sect. 3.4). Since this dataset is the only observational dataset available (and still very helpful!), you could consider showing its value in the calibration section. Even though this comparison is subjected to large uncertainties, addressing the model performance should be done quantitatively. The figure visualizing the trends in FWC could be moved to the Appendix (even though it is interesting, it is not clear to me why it is important in this context). Instead you could show a spatial representation of changes in SWE in order to better motivate the application of the very detailed energy balance snow melt model (250 m resolution which was mentioned to be an improvement compared to the existing work of Beamer et al., 2017). I did not fully understand the way how the scenarios are computed. I would encourage the authors to add some more explanations.

Overall, I think that the modelling experiment is sound and that the paper is a valuable contribution. Please find my comments below which might be helpful to improve the manuscript.

Specific comments

P1L17: Here, I would recommend to provide the annual runoff in mm per year too, since it helps to compare the values with other studies.

[Printer-friendly version](#)[Discussion paper](#)

P1L23: Please provide an explanation for the abbreviation CTD. In the current version of the manuscript, it becomes only clear on page 5.

P3L9: What do you mean by “large uplift rates”? Please be more specific.

P3L16: Here, you explain that the model output is available as daily output. What is the internal time step of the (energy balance) model?

P4L25pp.: Here you could provide some more details on the time step of the model. Since it is an energy balance model, I would expect sub-daily time steps (even though the output is daily).

P5L15p.: ET is computed by SnowModel? Was there any attempt to compare the results with the MODIS data – at least for reasons of plausibility, given that there is a mismatch in scales between MODIS and SnowModel?

P5L27: A new subsection 3.3.1 is introduced in section 3.3 but there is not any other subsection (e.g., 3.3.2 etc.). I was wondering if it is worth to merge the sections 3.3 and 3.3.1?

P6L19: The term “forecast” is used throughout the manuscript to describe the scenario data and the corresponding results. I am not sure if this term is correct in this context. I would suggest using “projection” instead since this term acknowledges additional uncertainty involved in climate scenarios which arise from uncertain greenhouse gas emissions (i.e. external forcing that is not exactly known). For instance, in a recent paper we also used the term projection to highlight this type of forcing (Hanzer et al., 2018). In contrast, according to Kirtman et al. (2013), the term forecast refers to initialized climate model runs (e.g., seasonal to decadal predictions, see their Box 1.1, or http://glossary.ametsoc.org/wiki/Climate_prediction).

P6L30: Here, I would suggest to add some thoughts why you have selected RCP8.5 only. It is clear that running impact models for numerous RCPs is expensive in terms of computational costs. However, you could argue that you are interested in a worst-case

[Printer-friendly version](#)[Discussion paper](#)

scenario to describe possible future changes.

P7L14: Does it mean that you did not apply SnowModel to future periods, e.g. by forcing the model with modified MERRA data (scaling of meteorological forcing)? From your explanations, you compared the historic run from SnowModel with the future Simulation of Beamer et al. (2017) in terms of long-term averages on runoff. If I understood this correctly, this would suggest a simple approach that contradicts the first line of your abstract (“... is used to estimate current and future runoff into Glacier Bay.”). I would encourage the authors to provide more details on the setup of future scenarios.

P12L32p.: I was wondering why only temperature and precipitation have been considered, given that SnowModel requires additional meteorological quantities?

P12L36: The validation could be done in a quantitative way too. The only linkage between your results and oceanographic data is provided on page 12, lines 7 to 8 (by comparing Figure 7a with Figure 9). Since the model calibration is done for another region (indeed, in which your region is included), I would expect a closer look on this dataset, since it is the only dataset available for assessing model accuracy.

P22L9 (Figure 7): Why do you plot ET derived by MODIS only, given that your model accounts for ET too? If ET computations are available for the model too, you could plot ET for the future scenarios as well. In my opinion, analyzing changes in ET would be an interesting asset to describe the hydrological change.

P23L10 (Figure 9): Why do we see a maximum in delta FWV in January? I would at least expect a brief discussion on that maximum in the text.

P27 (Table 3): It would be helpful for the readers to have a separate column for each existing column which provides the runoff in mm too. In your text, you already highlight the benefit of using specific runoff for reasons of comparison.

Technical comments

P7L17: There is no Sect. 3.4.1.

Printer-friendly version

Discussion paper



P10L: Figure 9 does not show any trends. Why do you plot the delta in FWV instead of FWV?

P11L12: Please correct the reference to the figure (there is no Fig. 9a).

References

Beamer, J.P., Hill, D.F., McGrath, D., Arendt, A. and Kienholz, C.: Hydrologic impacts of changes in climate and glacier extent in the Gulf of Alaska watershed, *Water Resources Research*, 53, pp.7502-7520, doi:10.1002/2016WR020033, 2017.

Hanzer, F., Förster, K., Nemeč, J. and Strasser, U.: Projected cryospheric and hydrological impacts of 21st century climate change in the Ötztal Alps (Austria) simulated using a physically based approach, *Hydrology and Earth System Sciences*, 22(2), 1593–1614, doi:10.5194/hess-22-1593-2018, 2018.

Kirtman, B., S.B. Power, J.A. Adedoyin, G.J. Boer, R. Bojariu, I. Camilloni, F.J. Doblas-Reyes, A.M. Fiore, M. Kimoto, G.A. Meehl, M. Prather, A. Sarr, C. Schär, R. Sutton, G.J. van Oldenborgh, G. Vecchi and H.J. Wang: Near-term Climate Change: Projections and Predictability. In: *Climate Change 2013: The Physical Science Basis. Contribution of Working Group I to the Fifth Assessment Report of the Intergovernmental Panel on Climate Change* [Stocker, T.F., D. Qin, G.-K. Plattner, M. Tignor, S.K. Allen, J. Boschung, A. Nauels, Y. Xia, V. Bex and P.M. Midgley (eds.)]. Cambridge University Press, Cambridge, United Kingdom and New York, NY, USA, 2013.

Seibert, J., Vis, M. J. P., Kohn, I., Weiler, M. and Stahl, K.: Technical note: Representing glacier geometry changes in a semi-distributed hydrological model, *Hydrology and Earth System Sciences*, 22(4), 2211–2224, doi:10.5194/hess-22-2211-2018, 2018. Stahl, K., Moore, R. D., Shea, J. M., Hutchinson, D. and Cannon, A. J.: Coupled modelling of glacier and streamflow response to future climate scenarios, *Water Resources Research*, 44(2), W02422 1-13, doi:10.1029/2007WR005956, 2008.

Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2019-1>, 2019.