The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2019-1-RC2, 2019


The Cryosphere Discussions
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# *Interactive comment on* "Hydrologic Diversity in Glacier Bay Alaska: Spatial Patterns and Temporal Change" *by* Ryan L. Crumley et al.

**Anonymous Referee #2**

Received and published: 20 February 2019

Comments to the Author Summary of the manuscript This manuscript (ms) presents current and future fresh water runoff into Glacier Bay in Alaska in order to link it to current trends of the fresh water content in the fjords of Glacier Bay. This seems to be important as CTD measurements might indicate a change of fresh water content in the ocean water. For this purpose the authors used the high resolution "SnowModel" to calculate changes in runoff from three sub catchments in the Glacier Bay National Park and Preserve (GBNPP) group between a present time period and a future time period under climate change scenarios. The results indicate that fresh water inflow may decrease in spring but increase during the summer month under future climatic conditions. The study concludes that this is validated by the CTD observations in the bay.



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Discussion paper

Evaluation I think the topic of this manuscript is highly relevant and important in order to understand and anticipate climate change impacts on the sensitive marine ecosystem in Glacier Bay. Accordingly, I do think that this the topic of this ms should be considered for publication. However, I have my major doubts if the presented ms convincingly demonstrates that the climate projections based on hydrological modeling can explain the CTD observations. My main concerns are the following: i) the link between modelling results and CTD observations is weak, lacks description and a convincing discussion. ii) the calibration of the modelling chain lacks description and convincing results. iii) an uncertainty discussion of the modelling results is missing. iv) the structure of the ms is at some locations mixing methods, data description and results v) due to the concerns above the conclusions are vague, speculative and lack conviction.

I leave it up to the editors to decide if the present ms can be revised or should be resubmitted. I would recommend to address the following concerns prior to publication:

Major concerns: 1. The linkage between FWV and FWC would require a thorough discussion of the mixing of freshwater into ocean water. Numerous paper exist on this topic but this ms fails to review the literature and discuss this complex topic in a convincing manner. 2. The description of the calibration is weak: if the authors claim to make realistic projections of future FWV into the ocean, I would expect a thorough discussion of the efficiency regarding snow melt, ice melt and rain runoff of the model; the necessity of multi dataset calibrations for hydrologic modelling under climate scenarios have been discussed in the literature. 3. The uncertainty of the results are not discussed; are the projected changes significant? What is the uncertainty of the future scenarios?

Specific comments: 1) Title: what is hydrologic diversity? This term is never mentioned in the ms accordingly it seems misleading to use it in the title. I would be helpful to have a title that reflects the content of the ms. 2) Abstract: An introductory sentence explaining the problematic and the purpose of the ms is missing; L16 why “wide variety”, the same “variety” exist in any glaciated catchment; L24: this sentence is redundant, as

it does not contain any conclusive information about the study: 3) Introduction: Nice description of the study site; however, a description of the linkage between fresh water inflow in an ocean bay and the subsequent impacts on marine life is missing; also a review of the literature of intruding freshwater into water bodies would be helpful. pg3, L 15: “the goals are different”: it would be helpful to outline the goals; L20: the results present do not convincingly present changes in the coastal runoff (see major concerns). 4) Methods1: pf4, L 8: model chain? Only two models are used, one for the reanalysis of the forcing data and the SnowModel. 5) Methods2: I think the clarity of the ms would improve if methods and data were two separate chapter; 6) Methods 3: 3.4. describes in a very rudimentary way model calibration; r2 and NSE values are provided. Since the authors claim to provide an “added understanding” and “constrained estimates of how costal runoff will change in the future” I would expect a thorough discussion on the efficiency of their modelling in regard to runoff, snow melt and ice melt contribution during the calibration period. If the calibration is not presented adequately, how can one trust in the results of future runoff? 7) Results: pg7,8: here results and methods to calculate the results are in the same chapter; I think a clear separation between methods and results would be helpful; 8) Figures 1,2 and 5 (in total 8 maps) all show specific aspects of the study site; this information could be combined and presented in one or perhaps 2 large panels. 9) Figure 3: I do not understand why contour plots are used here; bars indicating the exact value of T and P change would be more helpful. 10) Figures 4, 6, 7, 8, 9 and 10: it would be helpful to add an uncertainty to each point; e.g. stdev from the mean over the 30 yrs (but this would only account for climatic availability); I recommend checking recent literature on this topic.

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