The Cryosphere Discussions

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Comment on tc-2022-31

Anonymous Referee #1

Referee comment on "Global evaluation of process-based models with in situ observations to detect long-term change in lake ice" by Mohammad Arshad Imrit et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2022-31-RC1, 2022

After my assessment, I believe this manuscript by Mohammad et al should be subject to a "conditional rejection".

The authors claimed three objectives and would like to answer "(i) how well do lake models capture the timing and duration of observed seasonal ice cover? (ii) does model accuracy differ across lake types and climatic regions?, and (iii) do lake ice models better capture long-term observed variability compared to short-term change".

These are very good questions and I applauded the authors. However, I have difficulty understanding the methodology and the real conclusions finally drawn from this work.

My major concerns are:

- What the lake ice research society can benefit from this study?
- What does "Global evaluation" stand? The authors claim "We obtained 61,690 records of in situ observations for 2,658 lakes spanning from 1874 to 2020," but in this study, only 4 lakes were investigated.
- "Process-based models": I don't see any description of what processes those lake models have been dealt with. Section 2.1.1 is very difficult to understand, perhaps except for those who were deeply involved in the project(s) mentioned by the authors. I would encourage authors to tell a lot more of those lake models and how those lakes models were implemented or driven by the CMIP5. How those lake model runs have been carried out on a "global" scale? I suppose this is linked with the massive record you have mentioned in the manuscript (see comment above). I think authors need to talk more about the overall background picture of lake ice simulation, in particular, how

can we understand the "process-based models"

- Section 2.2 is also difficult to understand.

- 2.2.1 What model performance? I see statistical methods. If you meant to discuss the statistical methods/models that are planned to use to handle the lake model results and in situ observations, please be more specific and write it clearly. So far, I see a mixture of many things

- 2.2.2. Same impression as for Section 2.2.

- The entire Section 2 need a substantial major revision. The questions that need to be answered are:

- What are the lake model data, i.e., simulations, domain, data coverage and what particular lake ice parameters do you want to investigate? I see only "ice on" and "ice off". Presumably, authors refer to freezing up date and breakup date, if so, I would prefer to use these terminologies for better clarity. How about other lake process-related parameters such as lake ice thickness, snow cover?
- What are the in-situ observations, i.e., domain, data coverage and what particular parameters do you want to use and compare with model results?
- What statistical methods/models do you want to use to applied separately or together on both lake model calculated parameters and in situ observed parameters (If I understood correctly).
- A strong argument to support that it is sufficient to investigate only 4 lakes out of 2658 lakes. Maybe it is even better to show a map of those lakes to echo the title of "Global evaluation".

5. I see the same problem for section 3. I think Sections 2 and 3 together need a major structural change. Now both Sections are mixed with data and methodology.

6. How can we understand the "Factors affecting model performance" in both sections 2 and 3? What authors listed are connected with "machine learning" something that does not necessarily represent lake ice physics, I am not sure how those considerations connected with lake ice phenology? I think anything linked with an artificial intelligence methodology such as machine learning on the investigation of Earth Science needs extra cautious and better/clear arguments.

7. In the end, the authors concluded A) "when using these data: i) consider the relationship between lake ice and extreme climate events, ii) be cautious with predictions for regions currently without in situ representation, iii) when possible, use ensemble model approaches to reduce variability in predictions, and iv) estimate long-term trends

rather than specific lake responses. and B) “For ice on, modelled estimates were often more conservative than in situ observations which predicted a later ice on date. In fact, the real-world observations had later ice on and earlier ice off dates than any of the estimates from all three of the RCP scenarios.”

My questions:

- What are “these data”? modelled and observed lake ice phenology, from those 4 lakes?

- The second point “be cautious,,,”, reminds me that authors stated: " there are an estimated 50 million lakes around the world that freeze each winter but do not have long-term observations. Thus, quantifying changes in lake ice worldwide requires modelling". So how can we “be cautious”, to what degree? I would rather see authors tell some concrete numbers such as without in situ observations, the lake model predicted lake ice phenology are likely to have AA and BB offsets of V1 and V2, for example. Where AA and BB represent the lake model parameters and V1 and V2 represent their values of them.

- Why do we need to reduce the “variability in predictions”?

- I don’t quite understand the point iv).

8. Let me copy and paste the objectives here again: “(i) how well do lake models capture the timing and duration of observed seasonal ice cover? (ii) does model accuracy differ across lake types and climatic regions? and (iii) do lake ice models better capture long-term observed variability compared to short-term change”.

So, I think:

(i) is answered; (ii) I can’t tell; (iii) While, I still can’t tell the answer for (iii), if I was told “estimate long-term trends rather than specific lake responses”.

I would encourage authors to make a resubmission focus on the comparison between lake observations and modelling and a clear picture of “Global evaluation of process-based models” and the “comparison with long-term observed lake ice phenology” so readers can get some concrete and crystal-clear final take-home knowledge of this study” to improve either their lake models or improve the in-situ observations or the climate model forcing that applied by the lake models.

I can’t recommend this manuscript to be published in TC as in the current format without substantial revision and rewriting, sorry I can’t be more positive than that.

Regards