Geoscientific Model Development Discussions

Geosci. Model Dev. Discuss., referee comment RC2
https://doi.org/10.5194/gmd-2021-234-RC2, 2021

Comment on gmd-2021-234

Anonymous Referee #2

Referee comment on "SELF v1.0: a minimal physical model for predicting time of freeze-up in lakes" by Marco Toffolon et al., Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2021-234-RC2, 2021

The authors have developed an excellent minimal process-based model to project the timing of ice on in lakes. By showing how it gives similar results to a complex vertical 1D hydrodynamic model and outperforms popular empirical approaches (negative degree days), the authors give a convincing argument that the methodology should be applied to global lake studies. The language of the manuscript is very good, and the methodology is well laid out. Although some supplementary information could arguably have been part of the main text, the methods and considerations are clear and described in detail. Especially the pseudo analytical solution of the model framework using MCMC could provide the basis for future ice phenology studies. This manuscript is clearly suited for GMD and should be published soon.

Major comment:

- L363-368: I couldn't follow the argument here that because the importance of wind increases with lower values for heat loss and due to global warming, we would expect more year-to-year variability in ice-on timings, but we still can't use lake ice phenology as sentinel for climate change. Why is that? Wouldn't the decreasing heat losses due to global warming make ice phenology a prime example for an example of the consequences of global warming? Or is the argument here that changes in the wind field are not primarily influenced by climate change?

Minor comments:

- L71: The phrase "cooling progressively diffuses downwards" is a bit vague to me, as

cooling as a heat loss flux should not diffuse. The thinking behind the statement is clear that cooling causes stratified conditions, but isn't this more related to the closeness of the atmospheric boundary condition? Further "warming below 4 deg C" is also confusing in my opinion. Is the argument here that if the surface layer warms up closer to 4 deg C, this will cause convective overturn with lower layers that are less than 4 deg C? It would be good to make these two sentences clearer

- L74: the text switches between describing the curves as concave or parabolic, maybe sticking to one would be easier for the reader
- L85: W for the wind speed wasn't introduced yet and should be defined here
- L96: Shouldn't this be "(ii) the final temperature profile in the newly created mixed layer after cooling caused stabilizing conditions (phase B)", or something similar?
- L107: I think it would be good to state that the (potential) energy is given in J/m2 as otherwise some readers would be confused why you don't integrate over A(z)
- Equation 6: It would be good to state in a line before that Ec = rho cp h delta T / 2 (supplement S.18) to help the user to get the step to eq. (6)
- L168: It should be "in closed form" here
- 4: I really enjoyed reading that you argued the discrepancies between the two process-based models and the logger data is due to shortcomings of the meteorological driver data, and I totally agree with that statement
- L299: I would exchange "surprising" with "promising"