

# Author Response to Reviewer 1

We thank the reviewer for taking the time to read our manuscript carefully and for their insightful comments. We have revised the manuscript in accordance with the inlined responses. In particular, we have restructured the paper to clarify that the work is inspired by the philosophy of conceptual models and is therefore not intended to be predictive. Our model is meant to be only a first-order approximation of a simple phenomenon (increased ice melt due to warming and water drainage), it is meant to be rather an extension of the model by Levermann and Winkelmann (2016) than a process model. For this reason, it has limitations in terms of accurate testing and realistic predictions. These limitations will now be extensively discussed in the revised manuscript.

The authors have developed a model that describes the effect of supraglacial lakes on the surface mass balance of ice sheets. They describe the model, sensitivities of simulations to certain model parameters, and the effect of global warming scenarios on ice sheet elevation as simulated by their model.

I think that the effect of supraglacial lakes on the surface mass balance of ice sheet is relevant and deserves to be studied. Modeling it may be a way to better understand effects of these lakes, hence I welcome the model development effort. However, the model that has been developed for this study is neither evaluated, nor is its setup justified, nor are any hypotheses developed that would allow a later test. Hence I cannot recommend publication of this manuscript without major modifications. I will discuss these main concerns further in the following paragraphs.

In the introduction, the authors state that "the motivation behind [their] work" is to "better understand and model their [supraglacial lakes] formation and evolution in a fashion that could be adapted from a climate model". In the conclusions the authors seem to have forgotten this motivation. Potentials and limitations for including the author's model in climate models are not discussed. Instead, in the conclusion the authors seem to concentrate on their "additional motivation", "the necessity to characterize better the melt elevation feedback [...] in Western Greenland". They do this in particular by summarizing the results of projections using their model: "under reasonable conditions, the models suggest that the melt-induced feedback loop enhanced by lake formation may become a dominant contributor to Greenland mass loss as early as the end of the century." A central issue I have with this manuscript is that for both purposes, inclusion in a climate model and use for offline projections, the model should be evaluated. An effort to do this is lacking in the study. I'm happy to admit that a direct evaluation is probably very difficult due to the lack of adequate data. An interesting result of the study may provide an angle for evaluation: In the simulations, supraglacial lakes increase in size (both depth and horizontal extension) over time. Can this effect of lakes on the ice-sheet landscape be observed?

Answer: Many thanks for pointing out that we should better connect the introduction with the conclusion - this is certainly right. We have rewritten the introduction to better explain the role of

conceptual models for providing insights into the important processes which need to be included in a comprehensive model. With a conceptual model, one can test which processes need to be elaborated further because of their significant impact on the feedback loops. In this sense, it is not our goal that our model should be implemented in climate models, but it is meant to be a test bed for modelling strategies to describe feedback, for example, between drainage and albedo changes. We have also rewritten the conclusion section to emphasize the limitations of the conceptual model and how it relates to more complex models. Additionally, we have included a paragraph presenting a coarse validation that is feasible within the framework of our model, further highlighting that detailed testing is not possible.

Instead of evaluating the model, the authors argue that their model "reproduces the relevant interactions between ice, snow, and water". This, however, largely remains a claim. For example, the authors ignore a growing body of literature on effects of convection in melt ponds on the melting rates. Yang et al., e.g., argue for a bistability in radiatively heated melt ponds. In their direct numerical simulations depth of a pond and the insolation are key factors. However, the model presented here parameterizes the melting rate as a function of surface temperature that additionally accounts for lake depth. The authors would need to argue why despite ignoring insolation and in-lake circulation, the model does reproduce the relevant interactions. The authors write in L115 that "albedo plays the key role in the feedback loop". It would need to be well justified why one can ignore solar insolation in the model and parameterize the melt rate depending on temperature. I assume that ponds exposed to more or less sunlight might behave very differently for the same temperature.

Not to be misunderstood, it may be very valuable to develop a simple model. I could imagine even simpler models and the insensitivity to some parameter changes indicates that the proposed model is in some respect more complicated than necessary. However, without any evaluation and/or discussion of potentially missing processes that are discussed in the literature, the usefulness of the presented approach cannot be assessed by a reader.

We have included an extended paragraph detailing the processes that are neglected, comparing our conceptual model with more complex models aimed at achieving greater physical realism.

A formal, but nevertheless not small issue I have with the manuscript is the lack of rigour in the use of math.

In particular the use of symbols is incoherent. Of course, there is no full consensus in the scientific community, but as a guideline I like to recommend the NIST guide: <https://www.nist.gov/pml/special-publication-811/nist-guide-si-chapter-10-more-printing-and-using-symbols-and-numbers>. In this manuscript the use of roman and italic font seems mostly random. For some variables both font types are used. I also suggest to avoid the use of letter combinations as variables, here, e.g., MRI (used roman, italic, and with varying spacings between letters, spacings make it easy to misunderstand it as a multiplication of variables), TMA, SE, etc.

Answer: Following the reviewer's suggestion, we have uniformed the use of symbols using the guide provided. We have also modified the function names MRI and SE in the text, while retaining TMA and TMJ to reflect the abbreviations used in the original paper in which this approach about positive degree days has been introduced (Reeh, 1989).

Some variables are not defined (e.g.  $x$  as a horizontal coordinate, which is additionally used in another function). In the algorithm,  $M$  is not defined, and  $T$  is used for tolerance thresholds while it was earlier used for temperature.

Answer: We have defined  $x$  and  $M$  within the text, we have additionally modified the threshold variable name to epsilon to avoid confusion with the notion of temperature.

$W$  in Eq. 3 probably needs a unit.

Answer: We added the unit to the formula.

For the state variables  $W$ ,  $S$ , and  $I$  it is never said that they are functions of  $x$ , which creates an issue, e.g., in Eq. 4.

Answer: We have now specified in the text that these are functions of  $x$ .

Units are used inconsistently. In line 19 "y" is used for years, in the caption of Fig. 3 "yrs". "a" would be most common.

Answer: We changed the occurrences of "y" and "yrs" to "a", but we use "years" in the sentences in the text.

"mean" (eq. 8) needs to be specified and not indicated by text.

Answer: We have revised the equation to adopt a clearer expression and clarified in the text that it denotes the mean.

L208: "long-term mean July air temperature TMJ". Why is TMJ suddenly a long-term mean air temperature and what exactly are long-term means? In the equation above TMJ was defined as future temperatures (without any definition for future).

Answer: We removed "future" and clarified what TMA and TMJ are. We have also slightly rewritten the sentence for clarity.

In Eq. 14 the subscript  $y$  is used to indicate a yearly mean why earlier subscript  $a$  was used.

Answer: We have uniformed the use of subscript, now using "a" in the text.

Eq. 15 uses TMA for two different things, both different to the earlier use of TMA.

Answer: We corrected Eq. 14 and 15, adding the correct initial temperature, additionally, we have used a different name for the new forcing temperatures.

The above list of issues is likely incomplete.

Answer: We have looked in detail through the manuscript, further correcting mathematical notation (e.g., conflict of variable names in the algorithm, figure abbreviations etc.)

I list further issues according to their occurrence in the text:

L17 “Assuming ...”: Please provide a reference for this rate. And which temperature is meant? At the surface, at 2m, something else?

Answer: We added a reference and specified that this is temperature at 2m.

L47 “effect is comparable”: Effect on what?

Answer: We have removed the sentence, rephrased the text around it.

L58: This sounds as if ice sheets are generally modelled in state-of-the-art climate models, which is still the exception.

Answer: We rephrased our sentence to indicate that only a few complex models include the Greenland Ice Sheet.

L65: I don't understand this comparison of complexity. Please be more precise.

Answer: We have rephrased the sentence to highlight the conceptual nature of our model.

Fig. 2: This is difficult to understand. Can snow accumulate on liquid water? Try to describe this precisely.

Answer: Due to the simplified assumptions of the model, the snow can accumulate on the water layer, but this is not meant literally and that might not be clear enough and we are rephrasing this in the revised version. Our assumption is that when the snow melts then there is snow and water mixed in the same location, i.e. slush is formed. To avoid introducing a new variable "slush" together with a new dynamical equation for it, we opted for a simpler approach by keeping only snow and water thickness as variables and do this as a simplification of a mixture of both, but still as individual amounts. Our figure was misleading since we symbolized that with a layer of snow on top of the water, instead of representing it as the coexistence of both phases. Therefore, we have removed this figure and replaced it by another for the summer situation, where we only have water and ice, and all snow is molten and give a reference to the equation section that details that.

L107 (and section 4): “we consider  $m_l = 2m_i$ ” Why isn’t the melt rate considered an uncertainty factor to be studied in Section 4, given that the factor of 2 is apparently based on a single study with two observed ponds.

Answer: We have added the limit melt rate analysis to the plot (old figure 6).

More generally in Section 4 the comparison of the different factors needs to be discussed. Directly comparing effects for the same relative uncertainty range of  $[1/2,2]$  makes only sense if the relative uncertainties are similar. Wouldn’t one expect very different uncertainties e.g. for seepage rate and atmospheric lapse rate?

Answer: Since this is a conceptual model and some of the values of the parameters studied in the sensitivity analysis are not known, we had to specify them to certain values, which are a modeler’s choice. Moreover, the reviewer is right to assume that uncertainties for different quantities are different. However, for most of the quantities analyzed here, the uncertainties are also not known. Hence, we used factors instead of exact intervals to couple the size of the intervals with the values of the quantity. This is again a modeler’s choice.

Eq. 4: Is this indeed a local equation? Can’t non-local melt fill a lake through run-off?

Answer: The equation is local; however, we further specify in Section 2.2 that water flow, including runoff between nearby lakes, is simulated using the water-flow algorithm.

L149 “called frequently after integration steps, in particular ...”: Sounds odd. So when else is the water flow calculated? What is the time step?

Answer: We removed “frequently” from the sentence. We have also specified the integration step length.

L153 “Most of the computations ...”: Please be precise.

Answer: We specified that we refer to the computation of water volume in each basin.

Algorithm “Move 50% ...”: Why only 50%?

Answer: We specify that other fractions of water could be used, but this choice is inspired by the bisection method used in numerical analysis.

L189: Define acronyms.

Answer: The acronyms have now been defined in the text.

L190: A concentration on Greenland’s western coastal zones is fine, but doesn’t it clash with the stated intention to develop a model that can be adopted from a climate model? Also the

conclusions would need to be restricted to Greenland's western coastal zones. Another question: Does rain play a role in filling ponds?

Answer: As we said above, our model is not meant to be implemented in a climate model, but to inform which processes should be implemented in a climate model. In the revised version, we mention that the choice of parameters is exemplary, and that other parametrizations for other areas may be used instead. Additionally, we also mention that rain plays no role in our simplified model.

L243: How is runoff calculated?

Answer: We specify in the revised version that we calculate runoff as the mass loss in form of water.

Table 1: Where does the value of 5m for instant drainage come from?

Answer: According to the conceptual assumptions of the model, the 5 m threshold represents a characteristic shallow-water depth at which rapid drainage is assumed to occur. While this value is a modeler's choice, we explicitly test its influence in the sensitivity analysis to assess the robustness of the results.

Fig. 4: The color scale used in panel c is very hard to identify, probably because the cross markers are very fine.

Answer: We do not fully understand this comment. There are no cross markers in the figures; the panels show the temporal presence of supraglacial lakes across the domain. We have clarified this in the figure caption and added a note that the figure is best viewed on zoomable screens. Additionally, we have tested several alternative color scales, but these were either less effective at representing blue lakes on white ice or presented challenges for colorblind readers.

Seepage rate and Laplacian coefficient: I think that if indeed these two parameters have basically no effect on the simulation result, the inclusion of these processes in a model for which the goal of simplicity is stated should be revisited.

Answer: Although the model exhibits low sensitivity to the precise values of the seepage rate and the Laplacian coefficient, the processes they represent are fundamental to the physical consistency of the model. The inclusion of seepage ensures a realistic representation of the underlying physics with respect to secondary water removal processes and the inclusion of the Laplacian contributes to the inclusion of small scale changes of the ice thickness and additionally to the numerical stability of the simulations according to dos Santos et al. (2021). Therefore, despite the stated goal of simplicity, we believe that retaining these processes is justified. This clarification has been added to the revised manuscript.

L289 "Although it does not affect the scenario if supraglacial lakes are not present ...": Isn't this a banality? No lakes, no drainage.

Answer: We removed the sentence.

L294 "complete melting of the ice profile in less than 200 years": This value seems arbitrary because likely it strongly depends on modeling choices like thickness of the initial ice sheet and temperature.

Answer: We agree that the estimated timescale for the complete melting of the ice profile depends on the model assumptions and boundary conditions. In particular, it is influenced by the initial conditions and parameter values representative of the area surrounding the KAN-L base, including the initial ice thickness and temperature. These assumptions are detailed in Table 1. We have clarified this point in the revised manuscript to emphasize that the reported timescale is not universal but specific to the chosen model setup.

L329 "sea ice"?

Answer: Corrected the sentence. Our new text now correctly mentions the Greenland Ice Sheet.

L339 I'm still confused by the treatment of snow on lakes: Is the assumption that lakes can lose liquid water all year long, even when they are snow covered?

Answer: We understood that this is somewhat confusing. Our original intention here was to include an average water removal, as it would happen as melt water retention in firn, narrow and shallow cracks filling. These processes occur only during the melt season. As we are working with a conceptual model, we simplify this with an average value. This could be restricted to summer, but as it is actually only removing a very small fraction of water, we applied it to the entire year - and with that also when the lake is covered with snow. This is just one form of simplification.

According to the concerns of another reviewer, we have also added scenarios with refreezing. In this case, snow accumulates on top of the refreezing lakes or on the ice lid that covers the lakes. In the latter, water removal from the profile is interrupted in the lakes where a lid forms.

L346 "However ....": Simplicity is fine, but I find it hard to understand this sentence.

Answer: We completely rephrased the sentence to stress that this model aims at highlighting physical mechanisms at a conceptual level. The new text specifies that while our model does not provide a fully realistic representation of lake processes, it suggests that the enhanced melt-albedo and the melt-elevation feedbacks, together with the effects of the water removal,

are crucial and should be incorporated into improved parameterizations in more complex models.

L350: What is meant with the term “structural differences”?

Answer: We rephrased the sentence, highlighting that differences in model assumptions can affect projections of the ice sheet.

L351: What are "reasonable conditions"?

Answer: We specified that in the revised manuscript and refer to the simplified assumptions in our model.

L352: If I read Fig. 8 correctly, the inclusion of lakes enhances melt in all cases, but is not the “dominant contributor”, but rather increases the ice loss by often only 10% or so. It would be good to provide such a number, or a range of this number for different scenarios and conditions.

Answer: We have revised the text by replacing “dominant contributor” with “relevant contributor” to better reflect the magnitude of the effect. In addition, we now explicitly report quantitative estimates of the impact of supraglacial lakes on ice loss. The inclusion of lakes increases total ice loss by up to 20% in the CESM simulations and up to 22% in the NorESM simulations, with values varying across scenarios.