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Interactive comment on "Sensitivity of lake ice regimes to climate change in the nordic region" *by* S. Gebre et al.

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This manuscript present estimates of the ice cover changes of the Nordic lakes. The authors are using stand alone lake model because of a complex and relative small spatial scale of the lakes, physics of the lakes aren't realistically represented in regional climate models. This approach is not a new, this kind of modelling studies have conducted already several years ago (c.f. Elo et al. 1998. The effect of climate change on the temperature conditions of lakes. Boreal Environmental Research, 3, 137-150). The new aspect of this manuscript is that instead of detailed simulations for particular lakes, authors are using an idealized lake bathymetries for providing general picture of ice cover changes.

The manuscript is rather well written, but its nature is more like a technical report

than a journal article. It's lacking in depth analysis and discussion of the results, it's also too lengthy, some chapters aren't scientifically interesting and it repeats itself. Considerably shortening would make this manuscript much more digestible.

My specific comments are :

1. The chapter 2 (model description) could be considerable shortened. The authors are using an existing lake model. Since the model has been described earlier, model equations are not needed to present unless there has been modifications to the original equations or parameters. If the equations are necessary to show, then it's important to provide a complete description. Now there is some oddities, for example, it's not clear how the eq. (4) is related to (1) and (2), or how the heat flux from sediments is determined.

2. The model applied is rather simple. Basically, temperature profile of water is calculated by the heat diffusion model and the ice growth model follows simple analytical solution of the real physical model. State of art 1-D lake model would include turbulence model for a water column and a thermodynamical ice/snow model where temperature profile inside the ice and snow layer is resolved. Authors should justify their choice of modelling approach and discuss on weaknesses of the modelling approach.

3. I am very surprised that the authors don't discuss anything about the impact of snow cover changes on ice thickness. According to the previous studies, winter time precipitation will increase but less in form of snow fall. These changes would have two-fold effect on ice thickness. Increase of snow thickness would decrease thermal growth of ice due to the insulation effect and on the hand, increase of snow loading would imply higher potential for snow ice formations. According to the equation (6), snow insulation effect is not included in the model. If that is true, it is major simplification and certainly cause a large uncertainty on model results.

4. Analysis of the changes in meteorological forcing (chapter 4.5 and figures 7 & 8) is superficial and unnecessary. Authors can refer to the BACC assessment in this

context.

5. Two last paragraphs of the Summary and Conclusions chapter are very general and don't provide any new information. Authors should discuss more specifically what is the impact of the expected changes of lake ice cover or remove paragraphs totally.

6. Some technical comments

- use freezing date and break-up date terms only, now sometimes ice-on, freeze-up and ice-off terms has been used.

- p745, l26. Instead of "that are used to compute heat balance on lake surface" write "control heat balance of lake surface"

- p747, l1. Word "coupling" is not suitable in this context, express your shelf like "Utilizing prescribed gridded atmospheric data ..."

- p751, l5. Use only the term "snow-ice".

- p752, l17. abbreviation of the Norwegian Meteorological Institute is the met.no

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