Response for Short Comment (J. Haapala)
01. April 2014

**Thank you for taking time to comment on our paper and for providing comments that will significantly improve the manuscript. We have provided answers to the comments below the respective comments. The answers are provided in bold.**

This manuscript presents 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 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.

***We will shorten the manuscript (detailed model description, for example, will be included as supplementary material) wherever appropriate as suggested further below and by the other reviewers.***

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.

***In light of similar comments from the referees, we will shorten the model description in the manuscript and present the more detailed/complete model description as a supplementary material to give readers access to what type of equations are used in the model. The oddities mentioned in relation to specific equations will be made clear in the revised manuscript.***

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.

**The model choice has been a trade-off between model complexity and ease of use and computational efficiency for the climate impact assessment setup used in the paper. Our assessment relates more to finding mean changes in the future climate rather than a detailed analysis where, as mentioned in the comments, temperature profiles inside the ice and snow layers are resolved. Further, we have no observations of temperature measurements in the ice layer and will be difficult to validate any such simulations. We will add a paragraph on model selection (in the methods) and another on the limitations of the modelling approach in the discussions.**

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 causes a large uncertainty on model results.

*As stated the change in precipitation (from snow to rain in the future climate) very much affects ice thickness growth and the form of ice (congelation vs snow-ice). The MyLake model actually considers the snow insulation effect, and has been as a matter of fact one of the factors to select MyLake. This is done while computing the temperature of the ice surface, $T_{ice}$.*

*$T_{ice}$ = (p\*$T_f$ + $T_a$) / (1+p), where $T_f$ is water freezing point, $T_a$ is air temperature.*

*And, the factor p is parameterized as:*

*p = max ($k_{ice}$\*$h_s$ / $k_s$\*$h_{ice}$ , 1/(10$h_{ice}$)), where $k_{ice}$ and $k_s$ are thermal conductivities for ice and snow. Thus, when there is snow cover $T_{ice}$ becomes warmer than $T_a$ due to the insulating effect.*

*We will include in the model description that the snow insulation effect is parameterized in the model and provide the equations in the supplementary material. In addition, we will add a discussion of precipitation effects (form and magnitude) in ice regimes. As stated by the reviewer, reduction of snow on ice will on the one hand reduce the effect of insulation that facilitates thermal ice growth, while on the other hand this will imply reduction of snow/white ice.*
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.

*As suggested, we will revise this section refer to the BACC report (Assessment of Climate Change for the Baltic Sea Basin,* **The BACC Author Team (2008)***), which we are able to access through Springer and also consider removing the figures (at least figure 7). We, instead, will include other figures showing results for the various lake*

*depths analyzed (i.e., expand Figures 9 and 11) as also suggested by the second referee.*

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.

*The paragraphs will be revised so as to discuss specifically the possible impacts of the expected changes in lake ice cover.*

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.

*Suggestion accepted and will be corrected accordingly.*

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

*Accepted.*

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

*OK.*

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

*OK.*

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

*There are instances where DNMI and met.no have been used as abbreviations. We will stick to met.no as suggested.*