

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

L. Rontu (Referee)

laura.rontu@fmi.fi

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This is a modelling study on future changes of ice conditions over the Northern European lakes. A stand-alone single-column lake model MyLake was driven by down-scaled reanalysis and regional climate model output. The simulation results show decrease of the ice duration up to 14 weeks till the end of 21st century compared to the control period 1961-1990. The MyLake model and the other methods were preliminarily tested against detailed lake observations on one Norwegian lake and against freezing and break-up dates over several Finnish and one Swedish lake. Forcing by synoptic observations as well as by climate simulations was applied for testing. The authors conclude that the model validated well and could be used for estimation of the changes in lake (surface) state between the present and future climate. The manuscript con-

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tains a comprehensive and detailed report of the data, methods and results, which are worth for publication in the Journal. However, a thorough revision is needed in order to focus in the most important issues of the study and to analyse the results and their uncertainties more systematically.

General comments

There exist quite a lot of studies and reports on handling of lakes in numerical weather prediction and climate models, relevant for the present study, which seem to remain unknown to the authors. Since 2008, three workshops on “Parameterization of Lakes in Numerical Weather Prediction and Climate Modelling” have been arranged. Their presentations as well as the follow-up papers have been published in internet and in three special issues of *Bor.Env.Res* and *Tellus A*. The web page of the latest seminar can be used as a starting point: <http://netfam.fmi.fi/Lake12/> The journal publications can be found at <http://www.tellusa.net/index.php/tellusa/pages/view/thematic> and <http://www.borenv.net/BER/ber152>. In these materials, several articles relevant to the present study are included, and perhaps could be discussed in its revised version, e.g. Samuelsson et al., 2010, Kourzeneva et al., 2012, Martynov et al., 2012 and others. One more possibly interesting reference, not seen via the above links: Yang, Y., Cheng, B., Kourzeneva, E., Semmler, T., Rontu, L., Leppäranta, M., Shirasawa, K. & Li, Z. J. 2013: Modelling experiments on air–snow–ice interactions over Kilpisjärvi, a lake in northern Finland. *Boreal Env. Res.*18: 341–358.

The results about the future change of the Northern lake ice cover are obtained here within quite a complicated chain of simulations: ERA reanalysis is first downscaled by RCA (to a horizontal resolution of ca 25 km), then near-surface atmospheric variables are extracted for driving of one-column MyLake simulations in hypothetical mini-lakes. This kind of downscaling procedure can be justified by practical considerations. However, it is known that the climate models like RCA contain advanced integrated lake parametrizations, which would produce similar lake ice/water output with a simpler and arguably more physical basis. It would be good to compare the present offline re-

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sults with such online climate model simulations if they are available (from the Rossby centre?). On the other hand, there exist vast amount of literature where offline lake models have been driven by NWP or climate model output in a similar way as here, but mostly for shorter periods of time. However, these studies contain discussions about the choice and importance of the different atmospheric forcing variables and the sensitivity of the lake models to them. For example, it is not easy to understand why the available lowest model level temperature, humidity and wind have not been used instead of the diagnostic screen-level variables or anemometer-level wind, why available simulated downwelling radiation fluxes have been replaced with simplified combination of cloud cover with solar radiation annual/diurnal cycles. The usage of precipitation, in particular snow precipitation, which are generally believed to be important forcing parameters for which the lake models are sensitive, has not been discussed sufficiently.

The connection between the increase of near-surface temperature and lake ice cover changes is mentioned in many places of the manuscript. However, without a more systematic analysis of the surface energy balance over lakes, the reader remains with an impression that the screen-level air temperature change is the cause of decreasing ice cover in the future climate. This is also related to the forcing variables for lake simulations, where the authors show that MyLake is most sensitive to the changes of T2m. This is most probably due to the assumption of the lake model, not because in the nature the direct sensible heat flux between air and lake would be most important component of the energy balance. In the nature (and in the climate models, too), we are most probably dealing with more complicated dynamical and radiational interactions in the atmosphere and in the lakes. It would be good to discuss these interactions in the introduction or in the summary, to give a wider perspective and envisage the way for further studies, where it might be possible to study the predicted changes in the lake surface energy balance components (SWnet, LWnet, sensible and latent heat flux, heat flux from lake water (ice + snow) to the surface) during different seasons? The results of climate models surely contain this information, produced without or even with lake parametrizations, also the stand-alone MyLake model output should most

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probably contain these variables. From the side of the hydrological cycle, the influence of precipitation and especially snow precipitation on the results of lake ice evolution will surely deserve further study. Here, the related assumptions should be discussed.

I agree with the comment by Jari Haapala that the description of MyLake model in Section 2 is large and technical. However, it seems that the original publication by Saloranta and Andersen is not easily available (e.g. I could not reach it via internet). Thus I would not suggest removing this description but perhaps condensing it in the main manuscript and leaving the more comprehensive documentation for an Appendix, which will stay available via this open access Journal. I cannot comment how good the MyLake description in the present manuscript is, hopefully the authors have checked this carefully (perhaps together with the author of MyLake whose name is mentioned in the acknowledgements?). As a reader, I think I have now understood the main principles of the model, that seems to be both quite simplified in some aspects (e.g. with respect to the treatment of atmospheric forcing) and quite ambitious in others (e.g. in the attempt to treat horizontal exchange including in- and outflow, different areas of the layers in depth inside the lake, which would imply the existence of detailed bathymetries of lakes – hope I understood this point correctly - etc). My impression is that the chosen model is one among the many existing (single-column) lake models, some of which have recently been compared e.g. within the LakeMIP intercomparison (<http://www.unige.ch/climate/lakemip>), each with its own weak and strong points. It is evidently not the task of the present manuscript to compare the lake models, but it would be good to shortly motivate why just this choice has been made for this study, and to discuss the possible uncertainties caused by the assumptions of MyLake on the present results.

Specific comments

I have written some detailed comments as notes into the pdf file extracted from the Journal site. The notes were written and should be readable by using a new version of Adobe reader (I used the version XI in a Windows system). The commented pdf

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is attached as supplementary material. I found the presentation and the language of the manuscript good and understandable, however there seem to be minor mistakes and typos which the authors should check, perhaps together with the Editorial office. I have only marked some of them, not systematically all. Also, I would agree with the earlier online discussion comments by Jari Haapala and suggest the authors to take them into account when writing the revised version.

Please also note the supplement to this comment:

<http://www.the-cryosphere-discuss.net/7/C2955/2014/tcd-7-C2955-2014-supplement.pdf>

Interactive comment on The Cryosphere Discuss., 7, 743, 2013.

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