Review of the paper “The regional climate model REMO (v2015) coupled with the 1-D freshwater model FLake (v1): Fenno-Scandinavian climate and lakes” by Joni-Pekka Pietikäinen et al.

General comments:

This paper describes how the regional climate model REMO was coupled with the 1D lake model FLake, and what were the impacts on regional climate over the Fenno-Scandinavian region. For that purpose, a specific tile was added to the existing ones to represent lakes, and several experiments were conducted first to assess the impact of using FLake coupled to REMO and then to study the sensitivity of the coupled REMO-FL model to the snow albedo parameterization. These simulations were evaluated against in situ measurements for lakes and regional datasets for the coupled system. The main conclusions are that the coupled system improves the realism of simulations but enhances an already existing cold bias.
There's a high interest in understanding lakes behaviors for their coupling to the atmosphere, especially in a context of climate change and the effort made to couple REMO to FLake is very valuable. Moreover, regional climate models tend to increase their horizontal resolution where surface is better defined. In regions with a high lake density, more and more small lakes will be resolved and potentially will have an impact on the local and regional climate.
The paper is well written and structured, and a substantial effort was performed in the evaluation of the coupled model. I found it very interesting and easy to follow. Although the subject is of interest, I have noticed several aspects that needed to be more documented. Therefore, before accepting the manuscript for publication, minor modifications are required. These are listed below.

Specific comments

Coupling:

- This is not clear how the coupling between the surface and the atmosphere is performed. I understand that there are tiles over which calculations are performed at each time step of the model. Then what are the quantities that are aggregated in the gridbox? Are surface fluxes or surface variables aggregated and then transferred as boundary conditions to the radiation and turbulence schemes?
- How many layers are used to represent the PBL? It may be important to well capture the PBL representation especially in such a work where surface is a key component and can affect processes like lake breeze, evaporation, low clouds formation, etc.
- Is there an implicit coupling between the atmosphere and each surface tile?

FLake:

- The choice of using the REMO module to computed surface fluxes was not explained. This is not said if the SfcFlx module was tested in REMO and what were the results as compared to the REMO module. Was it at least tried and surface fluxes compared to some in situ measurements?
- The authors decided to activate the bottom sediment for lakes having a real depth smaller than 50m. There are however no measurements available (or only a few) within the bottom sediment layer to validate this module and I’m not convinced that a 35-yr spin-up is enough to correctly initialize sediment characteristics. A drawback of the activation of this module

could be an enhancement of a temperature bias at the lake bottom that could affect the whole temperature profile. What was the strategy to decide to use or not the sediment bottom module? What is the impact on the simulated temperature profiles?

- Nothing is said on the light extinction coefficient which is another important FLake parameter. Which value was chosen for REMO-FL? Does it vary in space, in time? Depending on its setup it may have a non-negligible impact on surface temperature and consequently on surface fluxes during the ice-free periods, but also on lake temperature profiles.

REMO:

- In the Semmler approach for snow conductivity, the C coefficient is an empirical constant. It was setup for Bear Lake in Alaska. Is the formulation adapted for snow over lakes in the Fenno-Scandinavian region?
- The snow albedo is limited to 0.25 in case the gridbox is forest-covered. However, in Kolarski et al. 2007 (reference of Gao et al. 2014 in the manuscript that refers to it), the figure 3.6 shows that the lower limit is 0.3 and not 0.25. What has motivated the choice of using 0.25 instead of 0.3? Are there simulations showing that this particular value performs better in terms of snow thickness, temperature?

Evaluation:

- E-OBS was chosen as the truth for the evaluation of the coupled system. It's clearly mentioned that E-OBS relies on the observation density network, which is true. However, how is the comparison between observations and simulations for 2m-temperature performed: is the difference of orography accounted for in E-OBS and in the model? This can have a very strong impact in mountainous regions. On the other hand, climatology of CRU is usually used (this is not the only one) to evaluate screen variables in climate models. Even if the resolution is coarser than what E-OBS can provide as interpolator, it would have been worthwhile comparing the simulations to another dataset. Particularly, it would be interesting to know if E-OBS is able to well capture convective events since there is a large difference with the simulations.
- As a complement, it would help the readers to know the impact of using REMO-FL on other near-surface fields like 2m-relative humidity or 10m-wind speed by comparing with REMO-ST. Because for example differences in humidity will have a direct influence on evaporation. Could you add a figure showing how lakes modulate these fields and add a discussion on this point?

Discussion:

- The authors mention page 8 that LWTs are usually close to T2M. This is not really true because it assumes near-neutral conditions which are unlikely to occur especially in winter when the lake is ice or snow-covered. In that case there is a decoupling between the surface and the air just above leading to very cold temperatures at surface. This decoupling may be responsible for the model cold bias. Can you add a comment on that?
- In page 9, figure 2 shows only relative differences for precipitation between model and observations. The absolute value is lacking (a difference of 50% for a 100mm rainfall will not have the same impact than on a 10mm one). Same for temperature, the reference is lacking. Please add the figure S1 (E-OBS reference data) from your supplementary material in the manuscript. On top of this remark, are the differences significant (for instance, was a Student test performed)? If not it would be worth examining if all differences experienced in the domain are significant or not.

Technical comments

- Regions 126, 130 and 132 are not explicit, please explain what they correspond to?
- Page 6, there seems to have a redundant paragraph. “The radiation… 2014” should be removed.
- Page 6, line 17, repetition of “grain growth effect…”
- Page 10, typo: tot he → to the
- Page 12, line17: “means” is used three times in the same sentence. You could replace for instance, the second one by “indicates”.
- Page 12, line15: data have → data to have
- Page 14, line14: only → on
- Figure 3 refers to CLARA-A1 whereas manuscript refers to CLARA-A2