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Interactive comment on "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.

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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.

We thank the reviewer for valuable comments for improving the manuscript. Throughout the text reviewers comments are marked with boldface and after each comment follows our reply. We also provide the page and line number(s) of the revised manuscript where the modification(s) can be found.

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?

We will improve this part of the manuscript. The lake related variables (temperature, albedo, roughness length, latent and sensible heat flux, specific humidity etc.) are calculate tile-wise while the model physics mostly uses mean values over the grid cells (weighted by tile area fractions). Tile-wise variables are included in the diagnostic output of the model. Revised manuscript: P3L22 and forward.

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.
It varies slightly during the simulations, depending on the PBL depth, but on average for this domain there are 6 layers describing the PBL. Overall, the vertical resolution is higher in the lower troposphere than higher up. Using more vertical levels should improve the results, but for our simulations we are confident that this would not change the results substantially. The choice in number of the vertical levels (and spatial resolution) has to be always balanced with the computational resources. We have added the information about the number of levels within PBL in the manuscript. Revised manuscript: P7L21-23

Is there an implicit coupling between the atmosphere and each surface tile?
The calculations are as following: during a time step all tile-related variables are calculated separately and later on grid box variables (2-meter temperature, humidity etc.) are calculated as a mean value weighted by the tile area fractions. Naturally, many physical routines (radiation, cloud scheme etc.) use these grid box variables and thus, during the next time step all tiles will get updated information (like radiation fluxes) that was based on tile averaged values. We will improve the description of this part. Revised manuscript: P5L11-14

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?
We will improve this part. Shortly, we did not implement nor test the SfcFlx module, because we wanted the lake tile surface fluxes to be in equilibrium/consistent with the other tiles. A similar approach has been used in previous FLake-RCM/GCM coupling works. Revised manuscript: P5L15-16

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?
The original paragraph has some missing information and is not clearly written. We will improve this part and add the missing information. In short, the FLake webpage says "Apart from shallow lakes, the bottom heat flux can be set to zero... Experience suggests that for lakes deeper than about 5 m the heat flux through the bottom can safely be neglected." This was also the case with our implementation. Also, when FLake is used for deeper lakes, a 50 m "false bottom" is used for temperature profile and the bottom sediment is switched off. Naturally, already the first 5 m cut-off limit switched off the bottom sediment module, but we left the possibility to use it also for deeper depths than 5 m (as is the case with default FLake as well).

So the impact was negligible, because in Fig. 1 there were only few grid boxes with depths less than 5 m. Revised manuscript: P5L20-27

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.
We are using the default configuration. There has been some papers about this and indeed they suggest using different values, depending on the locations. Overall, it seems that a global map for the light extinction coefficient(s) is needed and perhaps even a time-dependent approach could be utilized (Zolfaghari et al. (2017)). In this work, we have not tried to solve issues with this part of the FLake model. In terms of the manuscript, we will add information about using the default values for the light extinction coefficient. Revised manuscript: P6L9-10

Citation: Zolfaghari, K., Duguay, C. R., and Kheyrollah Pour, H.: Satellite-derived light extinction coefficient and its impact on thermal structure simulations in a 1-D lake model, Hydrol. Earth Syst. Sci., 21, 377-391, https://doi.org/10.5194/hess-21-377-2017, 2017.

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?
We did no make any changes to Semmler's approach. It is an improvement to the default setup already as it is (others have done the same, also on the NWP side). It is

very probable that this approach could be improved with more local information.

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?
The motivation was the literature based values (Roesch et al., 2001) which showed that the limits could be decreased. In this work we followed the Gao et al. 2014 approach as was shown to work better for the domain used in this work (e.g. temperature was closer with measurements). We did also make a test simulations with the default REMO values and got similar results as Gao et al. 2014.

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.
Orography influence should be always taken into account and we have also done it. This information was missing from the text and we have added it. CRU data would

have been another source (we have used it before), but in this work we concentrated on E-OBS. We also checked for Finland if using FMI's 10×10 km^2 resolution dataset would bring some more insight to the analysis, but the differences to E-OBS were small (even during summer when there are a lot of convective events over Finland). This, and the knowledge that the E-OBS observation network density is quite high over the Nordic countries, convinced us that using only E-OBS is more than sufficient for our study. Revised manuscript: P9L8-9

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 REMOST. 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?
A good suggestion. We have added the requested figure and discussion to the analysis. Revised manuscript: Fig. 4, Sec. 3.1

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?
The original formulation was misleading. The aim was to say that during open-lake season (no ice) the LWT's can act as a proxy for the 2-m temperature and we will analyze these later. Here, the compared temperatures are the real 2-m temperatures from the model and there is no artificial bias coming from the method. We will improve

the text to avoid any further misunderstandings. Revised manuscript: P9L19-20

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.
We have added S1 to the main text and improved the figure a bit. Statistical tests were not done before, but we have now added this analysis and all the figures showing differences against E-OBS (also in the supplementary material) show now the statistical significance with p-values < 0.05 with black dots. Overall, it can be said that the differences are statistically significant. Revised manuscript: Figs. 2 and 3 + Sec. 3.1

Technical comments

Regions 126, 130 and 132 are not explicit, please explain what they correspond to?
This information was too detailed and we have changed the ending of the paragraph to: "For points where Choulga et al. (2014) has no data, a default value is used and it can be set beforehand within the surface pre-processor. In this work, a default value of 7 m was used, which is a typical mean depth for our domain based on the data by Choulga et al. (2014)."

Page 6, there seems to have a redundant paragraph. "The radiation… 2014" should be removed.
This is true → paragraph was removed.

Page 6, line 17, repetition of "grain growth effect…"
Corrected to "the first represents the effect on snow grain growth due to vapor diffusion"

Page 10, typo: tot he → to the
Corrected as suggested.

Page 12, line17: "means" is used three times in the same sentence. You could replace for instance, the second one by "indicates".
Corrected as suggested.

Page 12, line15: data have → data to have
Corrected as suggested.

Page 14, line14: only → on
Corrected as suggested.

Figure 3 refers to CLARA-A1 whereas manuscript refers to CLARA-A2
The figure title had old information (at an earlier stage of manuscript preparation, we used CLARA-A1 data). Changed to CLARA-A2.

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