Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2017-245-AC3, 2018
© Author(s) 2018. This work is distributed under
the Creative Commons Attribution 4.0 License.

Geoscientific Model Development Discussions

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.

Joni-Pekka Pietikäinen et al.

joni-pekka.pietikainen@fmi.fi

Received and published: 2 February 2018

General comments:

The manuscript presents a study of the impact of a lake model, FLake, in the regional weather model REMO over several decades. Results are presented of mean seasonal biases over the study area, together with mean annual and seasonal evolution at a number of selected lake sites.

The main finding of the study is that FLake produces good lake surface

properties, perhaps despite the characteristics of the model within which it is embedded. However, its inclusion exacerbates the worst existing model biases, and the modelled internal lake temperature structure may not be a good representation of the measured lake temperature profiles.

In terms of its significance, it adds to a number of studies (not all cited here) validating the use of FLake in global or regional weather or climate models. Previous examples may be seen in the special issue of Boreal Environment Research (vol.15(2), 2010), and the Tellus thematic cluster "Parameterization of lakes in numerical weather prediction and climate models".

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

Section 2.1
Given the apparent model biases, and later error attribution, the description of the boundary-layer and surface-exchange representation of REMO is inadequate. Of the references given (p3, line 22), Kotlarski (2007) is a PhD thesis and Rechid (2009) is no longer available at the URL cited. In any case some description of the main points should be given here rather than relying solely on references.
We have removed the citation to the old link and improved the description part. Revised manuscript: Sec 2.1

The description of tile structure is also confusing. It is stated that there are 3 tiles: land, ice and water (p3, line 23), but then that the standard land-surface scheme does not have a tile for lakes (p3, line 25). So does "water" mean "sea water", as on p4, line 15?

The original water tile included all water, including sea, lakes and rives. We changed this part to "REMO's surface pre-processor, which creates the surface related parameters including the fractions of different tiles (land, water and ice), was modified in this work to include also the lake fraction as an output variable. Thus, while the standard version of REMO lumps sea, lakes and rivers together in the water tile, here a separate tile is created for lakes and rivers." Revised manuscript: P4L23-27

Section 2.3

P4, line 19: The GLCC lake fractions were used instead of those from Choulga et al (2014), although the latter are consistent with the depths taken from the same dataset. How do the two lake-fraction datasets compare?

There are some small differences, but they don't make a big impact overall (the GLCC also sets Black Sea and Caspian Sea as lakes, so for larger European domains we use slightly modified GLCC data, where these areas are classified as seas). We could not use the Choulga et al. (2014) lake fractions, because this would have led to situations where the sum of tile fractions would have not been exactly one. Thus, the only way was to use the original GLCC fractions and use Choulga et al. (2014) for depths when available.

P4, lines 25–26: The fractional ice-cover in the original model is replaced with binary ice cover when FLake is implemented. It is possible that this change on its own may affect the model results. Its impact should have been estimated.

It is true that this will have some impact on the results, but we think impact is most

likely small. First of all, the resolution of the driving data (here ERA-Interim, the resolution approximately 80 km) is coarser than the resolution used in the simulations. This means that when taking the sea-ice fraction from ERA-Interim, the values are basically either 0 or between 0.9 and 1. Thus, in practice, the ice fraction values are close to binary already in the standard version of the model.

Second, in reality, freezing is fairly fast during autumn and the biggest effect comes during spring, when the shores can be ice free and the rest of a lake is frozen (eventually ice breaks up and there is a mixture of open water and melting ice sheets). The albedo of water is smaller than the albedo of ice and therefore, the assumption of binary ice cover (i.e., ice fraction of 1 when there is any ice left) might delay the warming of lakes in spring in our simulations. However, the water temperature rise is still restricted by the ice in the lake (energy is used to melt the ice) so we don't consider this as a major issue. Also, in reality, there are some moisture fluxes from the open lake areas, but since the LWT is still quite low, these are also small. In terms of the manuscript, we have added more text and discussion about this issue. Revised manuscript: P5L7-14

P5, lines 8–15: There is some discussion of heat conductivity for lake ice and snow on lakes, but no discussion of how snow on land is modelled. Snow heat conductivity is taken as a single number (0.14 W/(mK), p5, line 14) despite the fact that snow density is apparently not fixed (p5, line 9). Studies such as Calonne et al. (GRL 2011) show that conductivity varies greatly with snow density, and the weighted sum of equation (1) presumably acts partly as a substitute model for this variation. It may be speculated that over-simplistic representation of snow conductivity generally may contribute to the winter cold bias even in the control run.

Eq. 1 is for snowy surfaces on ice. As described in the text, it is a mixture of snow and ice hear conductivity. The snow heat conductivity on land in the model depends on the snow temperature, so does the snow density. We have added more description of the model's surface scheme to section 2.1. Revised manuscript: P4L1-4

Section 2.4

Snow albedo gets a subsection here, although the rest of the snow scheme is not described. As stated later (e.g. p10, line 33), albedo changes may have little effect in winter given the lack of solar radiation in these regions then, so the emphasis on this aspect needs more justification.

The albedo was a natural step for further development as we had to add a snow albedo scheme for lakes anyways. As stated, these changes may have little effect, but it was one model development step that needed to be checked and document (similar albedo changes done for ECHAM6/JSBACH also motivated us). Unfortunately this did not fix the cold bias issue, but now we can rule it out from the list of the possible causes for the bias. We have improved this section in terms of justification. Revised manuscript: P6L11-16

P5, line 29: The forest fraction mentioned here should have been described earlier in section 2.1.

Yes, it could have been also discusses also in section 2.1. Section 2.1 describes REMO overall and here we wanted to show more details about the albedo calculations. Thus, REMO's snow albedo part is in section 2.4.

P6, lines 3–11 seem to be a repeat of the previous paragraph.

This paragraph has been removed.

Section 2.5

P6, line 32 – p7, line 2: The model end-state (in March?) is used as the model initial state (in August?). Would it not have been more suitable to use an initial state from the same season, say from August of the penultimate year?

This part of the original text was misleading. Yes, we did match the months and we make it clearer in the text. Revised manuscript: P7L15-20

P7, lines 5–6: Given that one comparison is with 2 m temperature, the height of the lowest model level should be stated, as also the surface interpolation scheme should earlier have been described.

The height varies from place to place, but is overall in 60 m height. We have also added missing information to section 3 explaining that we have done orographic correction to the model data (the temperatures have been corrected based on the height difference in each gridbox). We have also improved the description of the surface scheme. Revised manuscript: P3L29-31 and P7L21-23

Section 2.6

P7, line 31 – p8, line 1: Apparently 7 out of the 18 chosen study lakes may have unrepresentative mean depths in the model dataset. However, lake depth is often described as the parameter to which FLake is most sensitive. Is the study intended to test, or contradict, this assertion?

We use the default value of 7 meter when there is no data. This value, as explained in the text, is also from Choulga et al. (2014) and is representative for our domain. If we look the values in Table 2, where the mean depths are shown, we see that although some lakes do not have exact mean depth information, they do not differ much from the measured ones even with the default value. Indeed, some lakes have more a large error in their depth and this has been discussed as an error source when the LWT's are analyzed.

Section 2.6

P 8, line 32. “The reason for the [control-run] cold bias is still unknown.” Given that on p2 (lines 7–9) it is anticipated that a better lake representation will reduce wintertime heat fluxes, it seems odd to prioritise implementation of a lake model into a weather model with an existing, unexplained winter cold bias. Not least, it undermines the ice thickness/duration comparison. (As an aside, does the cold bias correlate with snow depth, as hinted by p9, lines 4–5?). The neglect of the cold-bias problem is acknowledged, but not really explained, in the last Conclusion paragraph. As a related point, the discussion on p11, lines 3–7 seems to say that the snow conductivity has been adjusted over lakes only, but acknowledges that snow thermodynamics may be a large factor in the cold bias. So why not try and improve “land” snow behaviour before applying FLake?

In any climate model, including REMO, there are many sources of error that could influence the simulated temperature, and often these errors partially compensate each other. For us, the treatment of lakes in REMO was an obvious part to start with, as the treatment of lake physics was known to be inadequate and to give rise to artificial heat and moisture sources. It is only after the lake physics was improved that the winter/spring cold bias in REMO became patently obvious (for the standard version, the underestimate was limited to the eastern part of the simulation domain, see the first two panels of Fig. 3). Thus our experiments demonstrate that the relatively good temperature simulation for standard REMO is a result of compensating errors, which is important to know.

This is not to say that the treatment of snow on land, including snow heat conductivity, should not be studied and improved. Indeed, our results for REMO-FL make this need more obvious. But, had we started with land snow physics, without improving the lakes, we would have had largely the same dilemma as we have now: the results could be compromised by the errors in the other parts of the model (in this case, the

lake description). For example, hypothesizing that the cold bias is related to snow heat conductivity, tuning the land snow heat conductivity in a model with artificial heat sources from lakes could lead to misleading results. Overall, model development is done step by step, and including a lake model was one step that was needed for REMO.

We did check the correlation between snow depth and the cold bias, but no obvious correlation was found (in fact, they seemed to anticorrelate at many locations).

Section 4.1

P11, lines 27–32: “The underestimations in the model during spring can be mostly explained by these factors.” The factors are discussed in a rather vague and unquantitative way, hence the proposed explanations are not very convincing.

We added more discussion in this paragraph and changed the ending. Revised manuscript: P12L28-P13L8

P12, lines 1–6: Errors are attributed to having the wrong mean lake depth, or to river input/output. It might have been better to adjust the depths of the study lakes, at least, to try and differentiate between these possibilities. Not least, it would be interesting to definitely attribute errors to river flow (or not), since this has a bearing on the general usefulness of 1-D models with no hydrological connections.

It is possible to change the depths, but we have not tried this in this work. Perhaps in a research with shorter simulations this would be a nice approach to see the model’s sensitivity. The last point about the rivers is indeed interesting and should be investigated more. This would require higher spatial resolution and more detailed input information (or a module for rivers).

P12, lines 23–24: "Naturally, there are no thickness measurements for the beginning or end of the ice season as the ice is too shallow to make these manual measurements." Does this mean the thickness measurements are biased high?
Yes, if there is a lot of variation on the seasons start and end times this can have a small effect to the near freezing and melting periods of the lakes. We will improve the text. Revised manuscript: P13L33-34

P12, line 30 – p13, line 18; Again, the reasons for discrepancies are discussed in a qualitative way, with no quantitative testing or discrimination between the possible causes of error advanced.
With our current setup the reasons discussed cannot be the separately tested. This is why we brought extra information from the snow depths over land nearby the lakes (supplementary). Also, the processes affecting snow depth vary from year to year, which can be seen from the variation of the measurements.

Section 4.2
Figure 5: It appears that the surface temperature at Võrtsjärv is above freezing during some or all of the ice-covered period?
Lake Võrtsjärv is located so south that it has a lot of year-to-year variation in ice cover. The measured ice period in Fig. 5 shows the average time when lake Võrtsjärv can be frozen. There are also years when the lake is open throughout or part of the winter, which explains why the average LWTs are above the freezing point.

P13, line 28: "the main problem is the cold bias [of] the model." I believe this supports my earlier point about priorities.
We agree that improving the cold bias in the model is an important research topic. However, see our response regarding step-wise model development above.

And now we know more about this since lakes are much more realistic in the model which brings us back to the point-by-point model development. The next important step would indeed be to look into the cold bias issue, using knowledge gained in this study.

Section 4.3
Figure 6: I do not find the different (arbitrarily chosen?) depth scales for model and measurement profiles to be a useful way of making this comparison. Instead, profiles with a common depth scale should be shown. Then the authors can discuss (or the reader can decide) whether FLake gets the correct surface behaviour despite, or because of, its internal lake-structure model.
This is the only way as the model uses mean depths and measurements are from the deepest point. We did test of using the same scales, but this approach did not really tell anything about the vertical profiles. FLake's vertical profile is based on assumed-shape representation for the whole lake depth. If it is matched with real measurement from the deepest point using the modelled mean depth, we are not comparing the same things and as said, the figure has no informative value. Our approach is not perfect either, but it gives information about the profiles and how well the model captures them.

Technical points

Title: Should it be "freshwater lake model FLake"
Corrected as suggested.

p1, line 10: "are in realistic." ?
Removed "in".

p3, line 8: use NWP as defined on p1.
Corrected as suggested.

p4, line 11: Remove “on”
Corrected as suggested.

p5 line 1: Does “It” refer to FLake?
No, it refers to the sediment module. Corrected to “This module...”

Equation (1): hs does not seem to be defined.
It is the snow depth in m. This information is now in the manuscript.

p5, line 19: tp → to
Corrected as suggested.

p10, line 27: tot he → to the
Corrected as suggested.

Interactive comment on Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2017-245, 2017.