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The Cryosphere
Discussions

Interactive comment on "Modeling the spatio-temporal variability in subsurface thermal regimes across a low-relief polygonal tundra landscape" *by* J. Kumar et al.

Anonymous Referee #2

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The authors present a new model (PFLOTRAN), in the framework of the New Generation Ecosystem Experiments initiative, which can assess the thermal hydrology for permafrost regions. The authors apply the model at four study sites in Barrow, where they compare the model performance with in situ collected data. The model presented is interesting and physically sound, and the the authors carefully run tests to evaluate model against measurements, On the other hand, some information on the model is missing, and the structure of the paper seems sometimes unclear to me. Therefore I recommend the manuscript for publication after major revisions.

Major points:

1. There is a problem in the data-model comparison that the authors fail to mention.

In Figures 8-11, the authors state that the model performs well against data except from the deeper soil layer. This is not actually true, since the model seems to largely overestimate the soil temperature in the summer months (May - October) also in the upper layers, as measurements and model only agree well at 5 cm depth. This seems to me to be a major issue, since this influences the estimation of key variables as the active layer depth. This problem, of course, would greatly limit the applicability of the model and its potential coupling to any biogeochemical model for estimation, e.g., of methane emissions. The authors should at least discuss this point in the paper. Does PFLOTRAN provide estimations of the active layer depth? If yes, how do they compare against measurements? What can be done to improve this key issue? And why does the model overestimate the temperature in the first place? It is clear that biogeochemistry is outside of the purposes of the paper, but nevertheless there seems to be a relevant issue, since this overestimation occurs in all four sites.

2. Another issue is the interannual variability. The authors state that 2013-2014 was the only year in which the needed information was available at the desired time step. It would be though helpful to show simulations also of other years, if possible, also against partially complete datasets. If only one year is shown, it is difficult to assess the validity of the model in dealing with potential differences in, for example, precipitation regimes. The authors state that the model is not calibrated for a specific year and a specific time, but nevertheless, this issue should be addressed.

3. On this note: how would such a topography-based model react to topography changes due to, say, ice-wedge degradation? This seems to be an important issue, as highlighted by Liljedahl et al., 2016. This is a big issue in the sense that if the model cannot cope to topography transformations, it would only work under present-day conditions, and would not be very useful for future simulations.

4. There is also a problem of scalability of the results. The model seems to be very complex and computationally demanding, since it is a 3D representation of thermal regimes working at a very small spacial scale. The authors mention a potential coupling

with CLM. How this coupling could work is not clear to me. Also, upscaling the detailed information of the 3D model at "just" the ecosystem-scale would be already a significant first step. How do the authors imagine such an upscaling? A stochastic approach would may be help, but how to link the very detailed and small-scale information needed to initialize the model to the larger scale ecosystem dynamics?

Minor points:

1. Page 1, Line 19: blank between words and hyphen.

2. Page 2, Line 20: the link does not help readability. I suggest to insert the link in the Appendix.

3. Page 2, Line 25: Please check the citation, it should be Cresto Aleina et al., 2013.

4. Figure 2: The Paper has a large amount of Figures, and I am not sure if all of them are needed. This Figure, for example, only shows data that are used in the model, but might as well be shown in the Appendix.

5. Page 7, Line 11: "the features": which features? Please, be specific.

6. Page 7, Line 23: the information about the contribution of Dr. Craig Tweedie can be inserted in the Acknowledgments.

7. Page 8, Line 13: You do not enforce any information on polygonal shape. But this information seems to me to be needed for simulation of other properties, such as, for example, water table dynamics. What do you mean then to scale the results to the whole region? Please elaborate this sentence, since it is not clear how this scaling would work.

8. Figure 3 and Figure 4 could be incorporated.

9. Page 11, Line 21 and further: please highlight in the text which ones are the prognostic variables you are evaluating in the equations and which ones the parameters. What is the model time-step? I might have missed it, but it should be clearly stated

here.

10. Page 14: The first paragraph of Chapter 4 is about Methodology (initial and boundary conditions) and should should be moved there.

11. Page 14, Line 17: How long is the spin up?

12. Figures 8 to 11: Please check the style of the caption. There isa a racket at the beginning of the sentence that does not make sense.

13. Figure 12: Why do you only show Site A? Is the behavior representative for the other Sites?

14. Figure 13: Is this figure really needed? In any case, if yes, it should go in the methodology. The color scale should also be changed, to improve readability for colorblind readers.

15. Tables 2 to 5. I do not think that all this information is needed. I suggest to give the information only at the depths showed in Figures 8 to 11. In this way, the authors could summarize the 4 tables in only one, improving readability.

16. Page 21, Line 6: Are such surface processes then implicitly included in the model? If yes how?

17. Page 25, Line 3: Which parameters could be tuned? It would be interesting to understand how tuning which parameter would improve model-data comparison.

18. Figure 15 does not seem to be needed in the text and be moved to the Appendix.

19. Page 27, Lines 4 and further: This discussion can be moved in the Conclusions, since it outlines ongoing and future work, which is not part of the paper.

20. Page 27, Line32: please change "models" in "model".

References:

Liljedahl et al., Pan-Arctic ice-wedge degradation in warming permafrost and its influence on tundra hydrology, Nature Geosciences, 2016. doi:10.1038/ngeo2674

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