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The Cryosphere Discussions

Interactive comment on "Multi-scale validation of a new soil freezing scheme for a land-surface model with physically-based hydrology" *by* I. Gouttevin et al.

Anonymous Referee #2

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This paper evaluates the impact of a soil freezing scheme within the land surface model ORCHIDEE. The discussion of soil freezing schemes and their interaction with soil hydrology and the comparison against the Stefan solution at the beginning of the paper is comprehensive and thorough and is a strength of the paper. The authors demonstrated, not for the first time, that a realistic soil freezing scheme is required for the realistic simulation of ground climate in cold regions. In my opinion, the quality of the paper drops off somewhat in the sections where the model is validated against data. This is partially due to the fact that the data against which the model can be tested is limited, and therefore it is difficult to demonstrate simply and conclusively the qualities of the soil freezing schemes. The model with a soil freezing scheme generally performs

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better than without, but it is difficult to distinguish which of the soil freezing schemes is really better. I sympathize with the authors as I have had similar difficulties in the past with respect to finding reliable and comprehensive data to aid in the development and validation of models in cold regions. The results section would benefit from an additional thorough edit to improve clarity, grammar, and sentence structure. It appeared to me that not as much effort was put into editing this section as the introductory sections on the model and the soil freezing parameterizations, which read well. Overall, I found this paper to be very thorough and forthcoming with regards to the strengths and weaknesses of the model and the various solutions to the soil freezing problem. The analyses presented reasonably evaluate the model across a range of spatial and temporal timescales as well as across relevant variables and processes. I find it acceptable for publication, pending some minor revisions outlined below.

Minor points

1. P. 2200, line 15: I don't agree with the statement that most existing models with soil freezing and physically-based hydrology are not designed to be coupled with a detailed carbon cycle module. For example, of the efforts cited in the previous statement on soil freezing and hydrology, both MOSES/JULES and CLM are coupled to carbon cycle models.

2. P. 2204, line 12: Probably it is worth mentioning that the latent heat sources and sinks are due to freezing and melting of soil water just to make it clear for the reader.

3. P.2206, line 3: I couldn't quite understand what is meant by this statement.

4. P.2214, line 2: It would probably be better to be more specific or used different terminology than the optimal vertical resolution is centimetric, at least partly because centimetric is not a commonly used word.

5. P. 2220, line 17: Please elaborate on the comment that soil moisture is modeled in terms of an anomaly.

TCD

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6. P.2221: A comment to the effect that strong conclusions cannot be drawn regarding the soil freezing parameterizations when testing against only one site. Additional cold region sites with the full suite of soil temperature and moisture and flux measurements would be helpful.

7. Section 4.2: In several places, the term 'active layer' is used when it should really be 'active layer thickness'. For example, p. 2221, line 18: "The active layer is the maximal annual thawing depth in permafrost regions" is an incorrect statement. The active layer is the thawed portion of the soil which grows throughout the summer season. What you are referring to is the maximum active layer thickness, which peaks at the end of summer.

8. P. 2222, line 9: "spatialized" should be "spatially explicit"

9. P. 2224, line 25: Representing the impact of organic horizons may warm the soil in winter, but it cools the soil in summer (see, e.g. Lawrence and Slater, 2008), possibly exacerbating the summer temperature biases.

Lawrence, D.M. and A.G. Slater, 2008: Incorporating organic soil into a global climate model. Clim. Dyn., 30, doi:10.1007/s00382-007-0278-1.

10. The lack of a representation of organic matter seriously confounds the analysis against active layer thickness. Several studies have shown that omission of organic matter results in soils that are too warm and active layer thicknesses that are too deep in summer (Nicolsky et al, 2007, Lawrence and Slater, 2008, Yi et al. 2007).

11. Figure 5: Check the labels on the right hand plots. There are two t=12h lines.

12. Figure 7: Seasons should be defined (i.e. which months are included).

13. Figure 8: Figure caption "overestimated by the model (snow underestimation)" should be "overestimated by the model (snow overestimation)"

14. Figure 11: This figure is very difficult to read as there are too many lines plotted on

each figure.

Interactive comment on The Cryosphere Discuss., 5, 2197, 2011.