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Geoscientific Model Development Discussions

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8, C2252–C2254, 2015

Interactive Comment

Interactive comment on "Simulating the thermal regime and thaw processes of ice-rich permafrost ground with the land-surface model CryoGrid 3" *by* S. Westermann et al.

Anonymous Referee #1

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This manuscript is comprised of three fairly different components: model description, model validation, and future experiments. These components are each very interesting on their own, but I don't feel that they fit together as well as they could in the current version, and I might suggest that the authors try to make a more unified manuscript by better linking the various pieces.

Overall, two main areas of mechanistic development in the model description are explored in the manuscript: surface energy fluxes, and excess ice/subsidence. The more novel of these is the description of the excess ice parameterization (section 2.7). The observational testing of the model (section 5.1) is limited to the thermal dynamics and some surface energy budget calculations; are there other observations that could be

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used to test certain aspects of the subsidence model? For example, how does the simulated soil thermal regime when submerged below a shallow water layer compare with observations of soil temperatures below shallow lakes? How do surface energy budgets of subaerial or subaqueous soils compare to each other, both in the model and observations (if available)? How do surface energy budgets compare between the more vegetated versus less vegetated terrains in the Lena Delta? Can the comparison between model and data in figure 3 be broken out more (for example as a function of time) to give more insight into the dynamics?

The simulations to explore future sensitivity (section 5.2) are very interesting but not clearly related to the observations shown in section 5.1, other than being at the same location. What observations would best allow the testing of the excess ice parameterizations? What observations would be required to run the excess ice model at larger scales?

The discussion of metastable states (section 6.3) is also very interesting, but I wonder if the discussion is missing something by being informed by a model that only considers the ice loss processes and not the ice formation processes. What role may ice formation processes play along with the feedbacks described here in setting slow oscillatory behavior of ice wedge growth and decay under stable climate regimes? Also, are there ways of using models such as this to identify where the thresholds of rapid ice loss lie? How are the positions of these thresholds sensitive to landscape features such as the depth of organic horizon, depth to massive ice, water content of excess ice, etc? It would be really interesting to use this model to explore these sorts of phase spaces and identify particularly vulnerable or resistant landscapes to warming.

Minor comments:

p. 6935 l. 21: If hydrology is not specified, are soils held at a fully-saturated state?

p. 6945, line 9: How are the saturated/unsaturated dynamics considered if hydrology is not prognostic? Where does the air come from?

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The model as described here uses only conductive heat transfer, so I am not sure I understand the criticism of conductive heat transfer in line 11 of page 6964 (and also the first sentence of the abstract). The real issue isn't conductive versus non-conductive heat transfer but the complex hydrology and volume changes associated with excess ice.

Interactive comment on Geosci. Model Dev. Discuss., 8, 6931, 2015.