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Geoscientific Model Development Discussions

Interactive comment on "Modeling different freeze/thaw processes in heterogeneous landscapes of the Arctic polygonal tundra using an ecosystem model" *by* S. Yi et al.

Anonymous Referee #2

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In this manuscript authors present a validation study of the Two Directional Stephan Algorithm (TDSA) to model different freeze/thaw processes in Arctic polygonal tundra. First, authors try to compare the methods against the analytical solution, and then against the observational data.

== Scientific Comments ==

The comparison of the numerical and analytical solutions is inconclusive.

The TDSA relies on the so-called "bottom-up forcing". The typical values of this forcing are not provided, instead the reader is referred to a previous publication (Woo et al., 2004). Furthermore, the authors try to generalize the original TDSA algorithm (the

original handles only two freezing fronts) to simulate multiple freeze/thawing fronts in the soil column. This requires an implementation of "bottom-up forcing" at the each front. Although the original TDSA algorithm seems to be validated, there is no discussion/demonstration how to choose values/parameters for the "bottom-up forcing" at each front. The presented validation study does not deal with the multiple fronts. Therefore, a potential use of the presented model to simulate development of taliks is limited. It is highly suggested that the authors discuss the choice of the "bottom-up forcing" with the great details, show how the value of "bottom-up forcing" is selected, how it affects the talik development, etc.

The comparison of the modeled results and observed data needs improvement.

The authors introduce a list of assumptions that are hard to justify, or the reviewer could not find a well-written explanation. For example, the maximum snow depth is limited in the model to 10 cm, while the field observations show that the maximum snow thickness can be 40 cm. Another concern deals with the relationships between the air and surface temperatures. The authors propose to a simple linear regression model, however there are no indication how this linear relationship is established. What is the temperature range for which this linear formula works? What is the time interval used to develop this linear relationship? The model assumes that the eddy diffusivity is important in the lakes, but does not mention the natural convection processes. It looks like that the model was calibrated/tuned to reproduce the results. Please present a list of key parameters that are used to tune the model. Present a sensitivity study with respect to change in these key parameters.

Initialization: the authors propose to run the model 100-200 yrs to reach an dynamic equilibrium. It seems to be a rather short timeframe to reach the equilibrium under the lake. There is no definition of the equilibrium in the paper. Are the authors looks to the equilibrium at different depths, or just beneath the lake?

Temperatures of deep layers: the authors compare the computed temperature to the

temperature at the borehole, and then use a linear weighted averaging to account for different types of relief/landscape. The heat conduction is a non-linear process and it could not be linearly averaged. The authors need to compare the computed temperature against observations individually for the each type of the relief/landscape. How close is the borehole of the center/rim/lake? Does it only represent the center? Could the lake also influence the collected data?

Performance of the DOS-TEM: the authors claim that DOS-TEM model is very efficient and can model 100yrs in 10 seconds, while a numerical model with the apparent heat capacity needs about 30 minutes for the same run. This comparison is not fair, if the apparent heat capacity model employs the Newton-Raphson scheme to deal with the non-linearity, then the computation time is greatly reduced.

Outlook: this section is out the area of expertise of the reviewer, but it seems that it will required a significant effort to come up with the parameterization of the TDSA in DOS-TEM to accurately simulate the talik development.

Specific comments: The manuscript needs to be re-written in a more coherent way. There is no need to have sub-sub-sections, and two appendices (one appendix has two sub-appendices). Here is a proposed structure:

1) Please start with presentation of the model and proposed changes to the TDSA algorithm. Please describe the numerical scheme, the details could be moved to the appendix. 2) Present a comparison of the analytical and numerical solutions 3) Present a site description/Meteorological data/ 4) Present a parameterization of surface temperature/snow/water eddy diffusivity 5) Present a comparison of the modeling results to the collected data 6) Discuss the results and present limitation of the method 7) Conclusions

== Technical Comments == There are no comments yet, the manuscript needs to be re-written to address the scientific comments first.

Interactive comment on Geosci. Model Dev. Discuss., 6, 4883, 2013.