

We would like to thank the referee for the valuable comments. Please find below our point-by-point replies to each of the comments.

Reply to Referee #2

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.

Reply:

A verification study (comparison with analytical solution) can only be undertaken for very simple cases, for example, with homogeneous material, a constant surface temperature, and constant initial condition. We therefore agree with the referee that the comparison cannot deal with multiple fronts. This verification work aims to demonstrate the ability of our model to simulate fronts and soil temperatures at different depths, for simple cases. It is as important as validation studies (comparison with real world measurements) but although all models have been validated, few have been verified. We therefore believe that the verification work in this manuscript retains some merit.

The only parameter determined empirically was the distance between a front and the forcing position (DZT and DZB, respectively, in Figure A1b in the manuscript). We carried out sensitivity studies on the effects of varying the DZB, from 0.5 m to 20 m, and compared the results to those obtained from analytical solutions. We found that changes of DZB have very little effects on the simulated front (Figure 3).

We agree with the referee that we did not provide enough detail in the original manuscript presented. We have now added more details in Appendix A of the revised manuscript.

In the previous version we only implemented bottom-up forcing for each front. In the revised version we have implemented both bottom-up and top-down forcing for each front. This resulted in a greater unfrozen soil thickness at the lake site, but had very little effect on the simulated surface soil/water temperatures of all sites. We have updated the

figures and manuscript (subsection 3.2.3) accordingly.

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.

Reply:

We have now described the maximum snow depth (MSD) on subsection 2.4.2. The assignment of MSD is based on our field experience, which indicates, relatively little snow on polygon rims and lakes but more snow on polygon centers. We therefore assigned a 15 cm MSD to both polygon rims and lakes. The MSD of a polygon center depends on its maximum depth (WD_{max}) and water depth (WD) - see Figure A3 in the revised manuscript. In our study, the simulated MSD for polygon centers is 35 cm, which is reasonably close to the measured figure of 40 cm (Figure 5 in the manuscript).

In the revised version of the manuscript we used an MSD of 15 cm for polygon rims and lakes and 40 cm for polygonal centers, based on field measurements (see subsection 2.4.2 and Figure A3 in the revised manuscript).

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?

Reply:

We have modified the text in subsection 2.4.2. to clarify the model.

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.

Reply:

Convection currents due to complex lake topography and density instability were not explicitly simulated. Instead we followed Subin (2012) to increase the eddy diffusion coefficient to simulate convection implicitly (See subsection 2.4.2). We also tested the sensitivity of the model to changes in eddy diffusivity (see Figure 9).

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?

Reply:

We agree with the referee that 100-200 yrs is too short a time for equilibrium to be reached beneath the lake. This is discussed on subsection 4.3, where we mention that it took less than 100 yr for the DOS-TEM to reach equilibrium at the polygon rim and center sites but that equilibrium was not reached at the lake site, even after 600 yr.

In the revised manuscript we have updated the text with respect to equilibrium runs in subsection 2.4.2 (in the Methods section), subsections 3.2.3 and 3.2.4 (in the Results section) and subsection 4.3 (in the Discussion section).

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?

Reply:

We have added information on the borehole location in subsection 2.1 (Site Description) and updated subsection 3.2.4 (of the Results section)

In subsection 2.1:

A 26.75 m borehole was drilled in 2006, in an area that consists of about 60% polygon centers and about 40% polygon rims, with a negligible areal proportion of ponds (Figure 1).

In subsection 3.2.4:

The centers and rims are typically about 10 m across and horizontal temperature differences due to surface heterogeneities can be assumed to be largely averaged out at depths greater than 10m. The borehole temperatures at depths greater than 10 m therefore represent an average temperature beneath both polygon centers and polygon rims. If the simulated temperatures from the rim and center sites were averaged by 40% and 60%, respectively, then the overall mean simulated temperature at 26.75 m depth would be -9.55°C, which is about 0.75 °C colder than the temperature recorded in the borehole at the same depth and over the same period of time

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.

Reply:

We agree with the referee that the comparison is not a fair one. We have therefore deleted this comparison in the revised manuscript.

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

Reply:

Thank you for your suggestion. We believe, however, that it is better to retain the current structure with separate sections for the methods, results, and discussion, rather than mixing together the methods and results.

We have included additional details on the TDSA in Appendix A.

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