

Dear Dr. Lars Kaleschke!

Thank you so much for your interest to our manuscript, remarks and suggestions to improve it. We tried our best to answer all your questions and make necessary corrections. Below are our answers (in blue).

To me it is not clear what the ultimate aim of the study is. To learn more about ice phenology of the large lakes or to show that the microwave model agrees with observations? Or the retrieval (?) of phenological parameters?

The goal of our study was to explain the observed seasonal variation of L-band brightness temperature at freshwater lakes. In addition, we wanted to show the advantage of low frequency (L-band) measurements against SSM/I, SSMIS, AMSR-E and AMSR2 in the investigation of the cryosphere of the Earth. This advantage lies in the penetration depth of the formation of radiation, which makes it possible to evaluate the characteristics of the medium and the processes occurring in it from a thicker layer. With this in view, we used our model called “Thermal emission of multi-layered non-isothermal media”. The model had been previously successfully employed in calculations of brightness temperature of sea ice, Antarctic snow/firn layers and snow cover. For the purpose of this work, the model was modified for freshwater lakes. We tried to explain our goals in Introduction and briefly in Conclusion.

It was not made clear what the advantages of the microwave model and the SMOS data as compared to AMSR-based methods.

The employed frequency range (L-band) allowed highlighting the state of ice and snow at greater depths compared to frequency ranges of AMSR-E and AMSR2. The theoretical analysis showed that L-band thermal emission was received practically from the whole lake ice column during both cold winter and melt periods. This makes it possible to remotely sense processes in ice and snow cover even in the melt period. For the first time SMOS MIRAS data were used to assess the phenology phases of freshwater lakes. Please refer to Section 4. We also added the corresponding explanation to Introduction and Conclusion.

The overall presentation quality is only fair. Some details are missing, e.g. the weblinks are not enough for the reproducibility. The data and the model could be described in more detail. Equation 1 comes with little motivation and introduction. Data sources and processing steps (smoothing, time averages?) for the figures are not clearly described.

We added more details about the used web resources. Also, Section 2.2 contains references to literature with climatic and glaciology data on the study lakes. In this section, Figs. 2-4 present multi-year data averages for all lakes.

We tried to explain in detail the essence of our model with a reference to the publication with complete formula derivation procedure (see Sections 3.1, 3.2).

In the calculations of brightness temperature, model input parameters were meteorological and glaciological data of the study regions and lakes. They were collected from a number of sources (publications and websites of scientific organizations). The collected multi-year data were averaged and approximated by smooth dependencies on the date of observation. See details in Section 2.2.

Brightness temperature was calculated for every day during the whole study period (2012-2013). So, no time averaging was performed. This is pointed out in Section 3.2.

The SMOS L1C grid cell size is about 15 km. Ice thickness cannot be uniform across such an area. So, in the calculations, brightness temperature was averaged over ice depth within  $\pm 10$  cm (Section 3.2).

We thank again the Editor for useful comments and improvement suggestions.

Important notice. In the period from 25 July to 15 August, 2018, all authors of the manuscript will take part in a scientific expedition in the north of Russia, out of reach by email, telephone, etc. In case of any necessity regarding the manuscript, please inform us either before 25 July or after 15 August.

Thanks for your understanding.

Sincerely,  
Vasiliy Tikhonov and Co-authors

**Anonymous Referee #1**

Submitted on 15 May 2018

We thank Referee #1 for the interest to the manuscript and the suggestion. We tried to make a corresponding revision in the manuscript. Our comment is below (in blue).

The authors have made necessary revisions and improved the manuscript significantly. There is one suggestion though. As my previous comment, I would recommend the authors discuss about the limitations of using climatology data for the model inputs and the assumptions made for effective temperature, lake ice thickness, standard deviations of ice grain and air bubble sizes, and surface roughness of snow. It is difficult to carry out measurements on all ice/snow parameters required for model inputs at large scales and over the satellite observation time, but the authors should make it clear to the readers that such in-situ data are necessary for more rigorous model validations.

In line with the suggestion, we added a discussion of the problem in the end of Section 4.

Many thanks once again for the attentive consideration of the manuscript.

Sincerely,

Vasiliy Tikhonov and Co-authors