Anonymous Referee #1

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We thank Referee #1 for carefully reading our manuscript and the comments. We attempted to provide answers to all his/her questions. They are below in red.

The authors studied L-band microwave emissions over large high-latitude lakes (except Huron) likely experiencing different lake ice phenology phases. The seasonal variations of SMOS brightness temperature were simulated and interpreted using a theoretical model developed previously for sea ice studies. The model-based approach is needed for lake ice studies and there are no major issues with the analysis; however, I do not find major contributions to the current understanding of remote sensing of lake ice either.

The theoretical model we use for the interpretation of seasonal variations of freshwater lake ice brightness temperature was not developed specially for sea ice. It is a model of thermal emission of multi-layered non-isothermal media (Sharkov, 2003). We have added the reference to its original description. The model provides satisfactory results for microwave emission of sea ice, Antarctica ice sheet, and snow cover. The references to the corresponding papers (Tikhonov et al., 2008; Tikhonov et al., 2013; Tikhonov et al., 2014; Tikhonov et al., 2017) are added to “Introduction”.

Our “contribution to the current understanding of remote sensing of lake ice” is detailed in Sections 3 and 4 where we discuss radiation penetration depth that is much higher at 1.4 GHz than at AMSR-E and AMSR2 frequencies of over 6.9 GHz. The frequency range we use allow understanding the state of ice and snow of greater thicknesses. In cold season, the radiation can be received from the entire ice column, and almost likewise in the melt period. Thus, the processes in ice and snow can be estimated using remote sensing techniques even during ice melt. For the first time the SMOS MIRAS data are used to assess the phonological phases of freezing freshwater lakes. Up to date, higher-frequency channels of AMSR-E and AMSR2 have been used for the purpose. We have mentioned this in the revised “Introduction”.

Major comments:

(1) This is basically a theoretical study but no major improvements or new developments in the microwave emission modelling are found. Section 3 is based on the available literatures and has significant overlap in content with (Tikhonov et al., 2014) (e.g. Eq. 1 and 3, Fig. 7 are presented in both literatures). It is necessary to review the theoretical basis, but this new study is not supposed to limit to a recap.

We use our model that was previously presented and employed for theoretical description of microwave emission of sea ice, Antarctica ice sheet, and snow cover (see above). It is not the aim of this manuscript to present a new model of thermal emission of multi-layered non-isothermal media. We attempt and, in our view, succeed to demonstrate the utility of our model in assessing the state of lake ice throughout the seasons. So, the validity of our model is once again proven. A few key formulas and figures are reproduced in the manuscript from previous works only to make the model easier to understand for the reader.

(2) Section 3: What about the incoherent scattering from a rough surface of wet snow? How do you consider layered snow and/or ice in the modelling? What about the temperature vertical gradient of snow and/or ice layer, which is not considered in the model or simulations?

Certainly, snow cover surface roughness is considered in model calculations. For the purpose, the model of scattering on statistically rough surface (detailed in (Choudhury et al., 1979)) is used. The model suggests that there is no correlation between the amplitudes of the waves scattered by two points on the surface. This is justified because the ice and snow that cover the lake surface during the cold period are seasonal phenomena, unlike multi-year hummocked sea ice. We do not focus on the issue and give only a reference to the detailed description of the "layered surface – atmosphere" emission model (Tikhonov et al., 2014). However, since Referee #1 has raised the question, we add a short description of the way the scattering on rough snow cover is taken into account (Section 3.1) and the roughness characteristics used (Section 3.2). Note, that here we once again cannot help overlapping with our previous results (see Comment 1). As said already, ice and snow over the study lakes are seasonal, so they do not have any prominent layered structure and we may neglect it. The beginning of Section 3 reads: "…the radiative system consists of lake water surface covered with lake ice and then snow…". Figure 6 presents this schematically.

In Section 3.2 we mention, that, in the calculations, we use the so-called effective temperature of the ice cover equal to the mean of air temperature and water temperature beneath ice layer, but not higher 0^{o}C. Such approach is widely used in modeling of emission of various layered media (atmosphere, snow cover, etc.) because it is impossible to properly consider all the media characteristics (see, for example, HUT model (Pulliainen et al., 1999)).

(3) The explanations on the brightness temperature patterns of ice, dry snow and wet snow are reasonable as similarly described in other literatures (e.g. Ulaby et al., 1986). The study would be more interesting if more deepened analysis was added. For example, an additional analysis on the quantified differences between multi-frequency (e.g. SMOS vs AMSR) responses to typical lake ice conditions and the implications to lake ice remote sensing.

Perhaps it skipped the attention of Referee #1 that the end of Section 4 is dedicated to the discussion and comparison of our results (SMOS) and similar results (Kang, 2012; Kang et al., 2012) obtained from AMSR data.

(4) Fig. 5 compares the simulated and observed SMOS brightness temperatures. Considering a large number of inputs of the theoretical model and limited knowledge of the ground truth (e.g. the snow and ice parameters), such simulations can only be used for qualitative purpose. It would be more rigid to have the model validated first using a controlled or field experiment with thorough measurements of the required parameters. It is also recommended to state the input parameter uncertainties and their impacts on model simulations.

The modeling results presented in the manuscript demonstrate that there exist not just qualitative but also quantitative agreement between theoretical calculations and satellite data. In Section 4, we discuss in detail the influence of thickness and wetness of ice and snow on brightness temperature of the system. Such studies were previously conducted for sea ice and snow cover (Tikhonov et al., 2008, 2013, 2014, 2017). The corresponding references are given in the manuscript. So, in our view, it is incorrect to say that “such simulations can only be used for qualitative purpose”. All presented modeling results agree with satellite data for the whole study period and for all lakes. As to the influence of input parameters variation on modeling results, this problem was investigated in (Tikhonov et al., 2014), where the influence of media inhomogeneities (ice grains, air pores, water droplets, etc.) on its scattering and absorption was examined. The aim of the present work is to explain dramatic and fast brightness temperature changes that are revealed from SMOS data over consecutive seasons at large freezing freshwater lakes. And we achieve it quite elaborately.

Minor comments: Section 2.2: detailed descriptions of the data sets (e.g. specific parameters; measurement accuracy; spatial and temporal representativeness) used in this study are needed instead of a simple list of data sources.

The data used in the modeling and presented in Section 2.2 (temperature, wind speed, snow and ice thickness, etc.) are taken from the web sites of well-known and reputable research institutions and belong to them. We only use these data. Their detailed discussion and estimation of measurement accuracy and spatial and temporal representativeness are outside the scope of our work and demand a separate extensive investigation.

We would like to thank again Referee #1 for his/her time and effort and helpful comments.

Sincerely,

Vasiliy Tikhonov and Co-author

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