The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2018-27-RC2, 2018

The Cryosphere Discussions
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Interactive comment on "Theoretical study of ice cover phenology at large freshwater lakes based on SMOS MIRAS data" *by* Vasiliy Tikhonov et al.

Anonymous Referee #2

Received and published: 14 March 2018

Review of paper tc-2018-27 Tikhonov et al. "Theoretical study of ice cover phenology at large freshwater lakes based on SMOS data"

General

The study deals with investigating L-band passive microwave emissions over freshwater lakes from a theoretical perspective, employing a forward model to simulate brightness temperatures which are then compared to observations from the SMOS mission.

The subject is interesting as relatively few studies have focused on modeling the passive microwave signatures from lakes. There are none that I am aware of that focus specifically on L-band. Although at present the coarse spatial resolution of spaceborne passive microwave sensors at L-band ultimately constrains their applicability to

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a limited number of large lakes, the study is still relevant given the specific advantages of this frequency compared to higher bands. In this way, this study also has the potential of bringing new information to the ongoing discussion of an operational follow-on sensor to the SMOS and SMAP exploratory missions.

From a general viewpoint the study is well written, clear and easy to follow. However, insufficient detail is given on key factors regarding the model simulations; I am left wondering if some kind of tuning of the forward model has been done. If so this is fine but clear details should be given so other groups may seek to replicate the results. Furthermore, comparisons of model results to observational data are very qualitative, e.g. no numbers (bias, RMSE etc.) are given. These factors weaken the broader conclusions that can be drawn from this study.

An additional aspect that I feel the authors omit is employing the full potential of SMOS data by analyzing data from different incidence angles. This may have relevance especially concerning conclusions made about sensitivity to dry snow (see major comments below).

Last, the references should be expanded to acknowledge more modeling work done for lakes at other microwave bands and studies related to SMOS and the cryosphere. Authors now refer mainly to their own work (with the exception of papers by Kang et al., which are extensively cited). In light of the above the potential of the paper is not really achieved. I give several recommendations below to help make this happen; I would recommend the authors should carefully address these comments before publication.

Major comments

1. Introduction, line 23: Add appropriate reference to the SMOS mission, e.g. Kerr et al., 2010.

2. Introduction, throughout: The authors should refer more broadly to recent publications on applying L-band for the cryosphere. Studies on L-band signatures from the

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Antarctica (Macelloni et al.,), sea ice (Kaleschke et al.) and snow cover (Schwank et al.) have been published in recent years.

3. Although not dealing with lakes as such, the study by Schwank et al. (2015) should be of particular relevance as they also present a model for snow cover at L-band which should at least be cited. I assume the model could also be easily applicable for lake ice. Similarly, there are some other works looking at lake ice signatures at AMSR frequencies beyond Kang et al., which could be added (see e.g. Kontu et al., 2014).

4. Section 2.1: The authors use only an incidence angle of 42.5 degrees from the L1C data (actually a collection of snapshots from 40 to 45 degrees). Why was this angle chosen over others (available from nadir to 70 degrees)?

5. Related to the above: Incidentally, SMOS incidence angels close to 41 degrees were noted by Schwank et al. (2015) to be insensitive to dry snow (whereas observations at other incidence angles were found to be sensitive to the presence of dry snow). Close to 41 degrees sensitivity to dry snow cover disappears at V-polarization, due to the opposing effects of impedance matching and refraction. Perhaps this is the cause the authors see no change in observed or modeled Tb with dry snow cover, as stated on p12, section 4? This should be discussed. Looking at other incidence angles beyond 42.5 degrees might shed some light here.

6. Section 3 is quite long, I suggest to divide into subsections. Section 3.1 could present the model and relevant equations, while section 3.2 could present the simulation setup (use of data etc) specific for this study (i.e. starting from p8, line 18).

7. Related to the above: p10 lines 10-17: This does not really deal with results, but should be moved from section 4 to section 3.

8. Section 3, section 4 and Table 3: Judging from figure 5 an almost perfect match of model vs. observations is achieved but it is not perfectly clear if e.g., tuning of model input parameters (given in Table 1) was required to achieve this. For TR1 no tuning is

possible from what I see, but what about volumetric wetness for ice in TR2 and snow and ice in TR3? A range is given in Table 3, but it is not clear if these were defined individually for each lake? If no tuning was required, the authors could also highlight this.

9. P10 lines 5-7: Dry snow and ice are transparent at L-band yes, but they still affect the emitted brightness temperature from the surface beneath though impedance matching and a change in the refraction angle. See Schwank et al., 2015. I imagine these results are also applicable to the water-ice-snow system.

10. P12, line 19-20: Again, the insensitivity to snow may be due to the choice of incidence angle (although at H-pol, something could perhaps be seen). I suggest the authors analyze the response vs. incidence angle e.g. for one lake comparing simulations with the TR1, TR2 and TR3 setting against SMOS observations (e.g., the average Tb for those periods at different incidence angles).

11. Section 5: The conclusions section is too short. The authors should provide a more complete assessment of their study, including the main results.

Minor/Editorial

1. P8, lines 8 and 19, maybe elsewhere: I think the convention is to write “wavelength” as one word (not wave length). Furthermore on line 19, wavelength is not a synonym to frequency, as it is now implied by the parenthesis. Please rephrase.

2. P9, line 17: “Note that for both media. . .” Sentence seems incomplete.

3. P12, line 13: “most wet”. Unclear what is meant. Almost wet? Mostly wet?

4. P12 lime 14: Typo: capitalized ‘M’ in ‘cm’

5. P13, line 7: “even in the cold season”

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