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Interactive comment on "A Novel Approach to Map the Intensity of Surface Melting on the Antarctica Ice Sheet using SMAP L-Band Microwave Radiometry" *by* Seyedmohammad Mousavi et al.

Anonymous Referee #1

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The paper deals with the detection and quantification of snow melting in Antarctica. The topic is very timely given the changes that the continent is facing due to the climate forcing. Basically, the work done is based on two synergic "modules". Half of the work is focused on the identification of the regions experiencing seasonal melting, the onset and the phenomenon and its ending; the other part is focused on the retrieval on the liquid water content of snow. Albeit being synergic, the two parts are self-standing can be discussed separately. Hereinafter general comments will be provided on the two parts, followed by a detailed discussion of the paper. The first part of the paper describes the algorithm used to detect melting areas and the duration in Antarctica. The algorithms leverage previous results well established obtained over both Alpine

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(e.g. Macelloni et al. 2005, doi: 10.1109/TGRS.2005.855070.) and polar regions (e.g. Picard et al. 2007 doi:10.3189/172756407782871684). The physical mechanism behind the melting detection is clear and only an extensive validation is lacking, for instance by using air temperature records from AWS or reanalysis. The second part deals with the quantification of the liquid water content in the snow and it is the most problematic. Mainly because the model and its inputs are partially described, and overall any validation is provided. This last point is the most impacting. Even if I recognize that a validation can be difficult, providing just a parameter estimate is not a good practice. This is what stopped many scientists in this effort.

Hereinafter, a point by point review of the paper follows:

Line 44 – The high penetration capability (up to hundreds of meters) is true only for dry snow or the ice of the ice sheets. When the snow contains liquid water or the ice is saline (e.g. sea ice) the penetration depth of e.m. waves at L-band strongly decreases to values on the order of 1m or less. The text must be improved.

Line 73 – Here and in the following: the cited Ulaby & Long book has almost 1000 pages and contains all of the main topics of microwave remote sensing. Citing this book without referring to the specific paragraph or the page is not a good practice. Please add detailed references. Also, some more specific papers can be cited like DOI: 10.1109/TGRS.2005.855070 or doi:10.5194/hessd-9-8105-2012.

Line 105 – This section should be shortened. The mathematical derivation of the final expression can be found in several books and derived easily (e.g. the cited Ulaby & Long, 2014; Tsang et al. "Scattering of Electomagnetic Waves...", 2000). It would be enough to report the final formulae for the brightness temperature. Also, Antarctic snow covers present a characteristic layering and parameters profile (e.g. temperature, density, etc) that must be account in the modelling (e.g. Leduc-Leballeur et al, 2015 DOI: 10.1109/TGRS.2015.2388790, Brogioni et al, 2015 DOI: 10.1109/JSTARS.2015.2427512). Any approximation that can simplified the modelling must be justified.

Line 111 – Volume and surface scattering can be neglected for the snowpack that never experienced melting. However, in coastal regions where snow melting happens seasonally, volume scattering can be appreciable because of the metamorphism taking place in the top snow layers.

Line 149 – This section should have addressed the main point of the modelling, i.e. the validation of the model, but it is limited to show that models with two given set of parameters can simulate the NPR behavior. The basic point is that many sets of input parameters exists that can provide a similar behavior. A throughout and rigorous validation of the model has to be provided to substantiate the validity of model estimations.

Lines 156 and 161 – It is not clear the origin of these parameters: if they come from conventional measurements made by the authors or from a publication. Are they representative of the Antarctic coastal regions? Some snow characteristics usually change throughout the melting season, for instance the density of the superficial layers. 450Kg/m3 is typical of aged snow layers that already underwent many melting/refreezing cycles while at the beginning of the melting season superficial snow density remains more or less the same as in the winter (about 250-300 Kg/m3). Why the wet snow layer thickness has been set to 3 cm? The same observations apply the parameters of medium 3. Another fundamental point: which is the permittivity model used? The reference provided doesn't help. In the years several models have been proposed and validated, and their estimates can differ appreciably. Why this one has been chosen?

Line 171 – As the authors show in the paper, melting take place only in the coastal regions and on the ice shelve, which are a limited portion of Antarctica (with respect to the size of the continent). Their geophysical characteristics are different from the inner part of the ice sheet. It is not clear the reason to consider "all Antarctic pixels" in the tuning of the empirical algorithms given that pixels that has never experienced melting outnumber the other ones. This point should be clarified.

Line 201 – This table is redundant and the information already stated in the text. It can be removed.

Line 207 – These plots are important given they provide a first assessment of the algorithm in Antarctica and should be commented more in depth. For instance, melting is supposed to take place whenever the air temperature is above 0°C however the proposed algorithm missed the melting that took place before mid Dec 2015 and after mid Feb 2016. This is not unexpected given that Tb at L-band is less sensitive to melting with respect to Tb at higher frequencies. These observations have already been showed at some symposium (e.g. DOI: 10.1109/IGARSS.2017.8127587) and further analysis/comparisons should be done here.

Line 215 – I found that a complete legend would makes the figure easier to be read. Also, maximum and minimum temperature should be represented with a different style (maybe lighter with the same color): in the present way they seem three different time-series.

Line 220 – Same comment as line 215.

Line 226 – Which are the specific "realistic ranges"?

Lines 230-231 – This implies that liquid water does not percolate in the lower layers. Is it an assumption to simplify the modelling? Is it possible to substantiate it?

Line 234 – NPR can increase if TbV increases or TbH decreases. So it seems that "In NPR-INCR pixels, NPR and TBV change in positive and negative directions, respectively" should be corrected as well as the sentence that follows.

Line 235 – It is not clear if the retrieval algorithm is run for each pixel flagged as melting area, or in a different way.

Line 236 – Which kind of thickness is retrieved? Thickness of medium 1 and 2?

Line 239 – “. . .,by comparing. . .”which type of comparison has been made? Has a cost function been used? In case, which is its formula?

Line 240 – “. . .there is no vertical layer structure during 240 the FS in the dry snow layers. . .” this is true only for the very simple model used. Actually a real snowpack has many layers that must be considered in an accurate modelling of the scene (Leduc-Leballeur et al, 2015 DOI: 10.1109/TGRS.2015.2388790 and Brogioni et al, 2015 DOI: 10.1109/JSTARS.2015.2427512). If the layering is disregarded it is impossible to reproduce the microwave angular signature of the dry snow emission. This was a problem highlighted by the SMOS team at the beginning of the mission. Given the authors use a very simple model, the parameters that they will retrieve can be considered “equivalent” and useful only to get the final product.

Line 242 – In this minimization there are several free parameters and few measurements. The problem is clearly ill-posed. How the authors can be sure that the geophysical parameters found by the minimization are the best set for the snow covers instead of others? The fact that it is possible to simulate correctly the TbV timeseries does not imply that the geophysical parameters found are correct and could provide the subsequent reliable liquid water content estimation (actually the fitting of TbH shown is poor).

Line 246 – Why in the fitting process the authors consider H and V pol SMAP timeseries independently instead of searching for a inputs that fit both of them together? Given a set of ground parameters the model should be able to simulate both polarizations. If not the case, the model is not accurate enough, the input parameters are incorrect, or both of them.

Line 251 - It is somewhat weird that the ERR() operator stands for a temporal mean. Is the text correct?

Line 252 – It is not clear between which quantities the bias can be found. Could the authors comment this sentence?

Line 252 – It is not clear the aim of this operation. The liquid water content, along with the other inputs of the models, has been derived by fitting the SMAP measurements. Then the same retrieved geophysical parameters are used to simulate the Tb with the same analytical model and assess in this way the reliability of the method. If all the input parameters come from the fitting and the analytical model is the same, why the Tb simulated should not agree with SMAP measurements?

Line 256 – what is mv2ref?

Lines 268-270 – This seems to be more related to my previous comments than to the validity of the method. The plot seems to suggest that SMAP TbV timeseries is used to determine the input parameters of the model, that in turn produces the best Tb match.

Lines 271-275 – Here lies the other major weakness of the paper: there isn't a validation of the algorithm. The results are plausible but a validation has not been performed. The fact that the liquid water content of the snowpack ranges between 0 and 2.5% is encouraging, but it is more or less the same range of common alpine snow. Also, the referred paper deals with snow ontop of sea ice which undergoes to different processes with respect to snow on land. I am well aware that validating a retrieval method in Antarctica can be a tough task but it is to be done.

Line 296 – Table 4 is useless: it contains the same timeframe for all the seasons. The information can be stated in the text.

Line 232 – As said before, it is not only matter of higher penetration depth. Indeed the timeseries represented shown that L-band starts detecting the melting later than the physical onset (air temperature above 0°C) and stops earlier. It is more a different interaction between e.m. waves at these wavelengths and the snow/ice medium.

Line 373 – Willat et al. (2010) performed measurements on the sea ice off the East Antarctica coast and not on land. As said previously, the LWC range of 0-5% is characteristic of the snow in general: above few percent of LWC liquid water start to percolate

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in snowpack so it is difficult to find higher values.

Summarizing my review, I found that the paper is composed by two parts being the mapping of regions experiencing melting more advanced that the liquid water content estimation. This latter part is at an early stage, lacks of a proper validation and it is not ready to be published. Even if all of my comments will be addressed (either because I misunderstand the paper or the authors improve it) it is not possible to publish a method that is not validated. This is the weakest point that must be addressed properly.

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