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Comment on egusphere-2022-224

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Community comment on "Brief communication: Classification of thawed/frozen topsoil state by spectral gradient methods based on SMAP and GCOM-W1 radiometric data" by Konstantin Muzalevskiy et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-224-CC1, 2022

Dear Editors,

Two months ago the same article by the same authors, but in the Russian language, was received by Sovremennye Problemy Distantsionnogo Zondirovaniya Zemli iz Kosmosa (http://jr.rse.cosmos.ru/?lang=eng). As a reviewer, I recommended to reject it (see my review below).

After that the article was sent to two journals: Issledovanie Zemli iz Kosmosa (https://sciencejournals.ru/journal/iszem/, http://www.jizk.ru/) in Russian and The Cryosphere (https://www.the-cryosphere.net/) in English. The Russian journal also rejected the article after a negative review by another reviewer.

I believe the authors violated publication ethics by sending the same article to two journals at the same time.

Kind regards,

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My review of the article sent to Sovremennye Problemy Distantsionnogo Zondirovaniya Zemli iz Kosmosa (http://jr.rse.cosmos.ru/?lang=eng)

The article presents the results of analysis of application of spectroscopic methods for identification of thawed and frozen soil conditions based on brightness temperature measurements by SMAP and GCOM-W1 satellites in the frequency range from 1.4 GHz to 36.5 GHz. The analysis was performed for ten test sites located in the Arctic region in Russia, USA, Canada, and Finland for the period from 2015 to 2020.

The presentation of the material is awful, both the narration and the physics of the problem. There are plenty of trivial or mutually exclusive statements throughout the text. The analysis of the satellite data is done by juggling the values of either brightness temperatures for different channels, or their combinations. The suggested physical interpretations also leave much to be desired.

The first thing I would like to point out is the definitions of "thawed" and "frozen" states of soil, which are absent in the article. Abstract reads: "The data of soil-climatic weather stations at key sites on soil surface temperature at the transition through 0°C were used for ground validation of the thawed/frozen state of soil". I would like to stress that this is fundamentally incorrect, because soil can be thawed at negative temperatures as well (all depends on the soil structure) (see, for example, Ulaby, Long, 2014). Well, let’s leave it to the conscience of the authors. However, at the very end of Results and Discussion (p. 12), Authors report that two sites (SO and SA) were removed from the analysis due to big errors. Authors attribute these errors to

“…unstable soil freezing (soil surface temperature for most of the winter ranged from 0°C to -2°C-4°C according to weather stations)”. So, what is the "frozen" state of the soil, if from 0 to -4°C it is "not frozen"?

Second. The authors determine the effective temperature of soil using the AMSR2 6.9 GHz vertical polarization data. This is allegedly based on the assumption that for this frequency, the AMSR2 sounding angle (55 deg) corresponds to soil Brewster angle. The Authors are probably unaware that the Brewster angle of a soil is determined by its moisture and can vary quite widely (see, e.g., Ulaby, Long, 2014). Hence, it is wrong to arbitrarily assume the 55-degree angle to be the soil Brewster angle. Further, literally the next but one sentence reads: "Further, estimates of ΓH(f) will be considered as the apparent values of reflectivity, since the absolute value of TbV(6.9) does not coincide with the actual values of the soil surface temperature Ts0, but is only proportional to them.” Well, is it equal or proportional?! And what kind of physical characteristic is "apparent value of reflectivity"? To whom, how and why is it apparent?!

Third. It is absolutely unclear why the Authors use microwave radiometry data to determine effective soil temperature. There are much more effective methods that use infrared data. At present, methods using satellite microwave radiometry to determine soil temperature are still under development and are not finalized yet. I recommend the Authors the review of Duan et al. (2020) on this topic.

Fourth. The Authors analyze brightness temperature of the test sites obtained by two satellite sensors at different viewing angles: SMAP at 40 degrees and GCOM-W1 at 55 degrees. Thus, there is a comparison of brightness temperatures of different bands received at different angles: band 1.4 GHz at 40 degrees, and bands 6.9 GHz, 10.7 GHz, 18.7 GHz, 36.5 GHz at 55 degrees. And then the conclusion is made about the efficiency of bands 1.4 and 6.9 GHz, and bands 1.4 and 36.5 GHz. The question arises, "Have the authors heard anything about Fresnel formulas?" At different angles, the reflectivity of the same surface is different. The Authors operate with incomparable characteristics.

Fifth. On pages 6-7, Authors derive expression (3) for "isothermal and dielectric-homogeneous half-space." However, on page 10, when discussing Figure 3, namely the spectral gradients of brightness temperature and reflectivity, the Authors explain their highest and lowest values by “a significant contrast of temperatures and permittivities between the shallow and deeper emitting layers of soil". Again, one contradicts the other!

Further the authors engage in formula-juggling, deriving one expression from another. For example, from brightness temperature (with author simplifications) they get surface reflectivity; or from the gradient of brightness temperature spectral density - the gradient of reflectivity. The result is presented in trivial "flip-flop" graphs because one formula follows from the other. In the end, Authors conclude: “Both criteria give comparable accuracies of forecasting thawed and frozen topsoil state for tundra soil cover,” which is bluntly obvious, since one formula is derived from the other.

When considering the gradients of brightness temperature spectral densities and reflectivity “per unit interval of the frequency spectrum”, Authors find that they "seem to be larger for the narrower 1.4-6.98 GHz than for the broader 1.4-36.5 GHz frequency band." There can be no doubt about this, since the discussed characteristics are obtained by dividing by a smaller value (frequency interval) in the first case and a larger value in the second case.

The article is carelessly formatted. There are a number of typos both in the text and in the figure captions. In Figure 4, curve 1 merges in color with curve Ts0.

Also, I would like to draw attention to two more points.

In Introduction (p. 2), when considering various algorithms for determining the thawed and frozen soil states, the Authors mention the polarization index PR as an indicator. In the text, it is said that “The decision on thawed or frozen state of the soil is made when the normalized PR passes through 0.” Based on the expression for PR, it should always be higher than 0, because, for any frequency, the value of brightness temperature on vertical polarization is more than on horizontal. What then does the phrase "…when the normalized PR passes through 0" mean?

The last sentence of Introduction concludes: "Taking into account the development of domestic multifrequency satellite radiometric sensing systems and the expected launch in 2028 of the multispectral (1.4-36.5 GHz) Copernicus Imaging Microwave Radiometer (Kilic et al, 2018) of high spatial resolution (55-5 km), development of new multifrequency radiometric methods to identify thawed/frozen soil state is highly relevant". I wonder, what are these "domestic radiometric multifrequency satellite sensing systems" and why only Copernicus is given as an example, and not some domestic system?

Based on the above, I believe that this article is unacceptable for publication in Sovremennye Problemy Distantsionnogo Zondirovaniya Zemli iz Kosmosa.

References:

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