

Interactive comment on “Technical note: A sensitivity analysis from 1 to 40 GHz for observing the Arctic Ocean with the Copernicus Imaging Microwave Radiometer” by Lise Kilic et al.

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Received and published: 14 January 2021

First, we thank the reviewer for reading our paper and for his/her comments.

General comments Firstly, I'd like to thank the authors for their work. I have marked for major revision as I think more is needed to address the benefits arising from having the CIMR channels on one platform. I did not feel the paper addressed this, despite saying it was very important. The paper updates a popular figure that has been used for many years to illustrate the sensitivity of low frequency microwave observations to a range of geophysical products. The major change is that the new plot takes account of the atmosphere and adds SIC and additional plots show difference between tropics,

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middle latitudes and polar latitudes and details for the arctic. The updated plot and new plots will be of interest but the paper does not give any particular new OS insights. It's a short paper to revisit the plot and look at sensitivity in the context of CIMR. It feels to me to be a first useful step in a wider study that will give a deeper understanding, but more progress is needed to justify publication.

The CIMR mission has been described with more details in a previous publication (Kilic et al., JGR, 2018) and other papers describe some potential applications of CIMR (see for instance a list of publication at <https://cimr.eu>). The famous Wilheit figures has been used widely by the community, for CIMR and other missions. It was felt necessary to 'officially' update this figure and to provide a reference for this update. This is the goal of this technical note. We are aware that this is not a full detailed study and this is the reason why we chose the technical note format.

Scientific significance: Fair The paper does not reveal any substantial new understanding of ocean science.

Scientific quality: Good

The paper addresses the value of CIMR by examining the sensitivity of microwave frequencies to a range of geophysical parameters. Scientifically this is fine and the calculations are state of the art. However, it could perhaps have addressed more the linkage between frequencies. The paper says this is important, but then does not address this aspect.

Presentation quality: Good The results are clear, but presenting only normalised sensitivities can give a misleading impression of the relative importance of sensitivities at a particular frequency, it only shows well the point of maximum sensitivity for each geophysical parameter. This is discussed further below.

Specific points requested by OS

1. Does the paper address relevant scientific questions within the scope of OS? CIMR

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is an important mission for OS, so the question is relevant.

2. Does the paper present novel concepts, ideas, tools, or data? The paper updates the work of Wilheit, but is not novel.

3. Are substantial conclusions reached? The variability with region is shown to be significant, but I think this is well known since papers such as Phalippou (1996) and of course earlier work by Dr Prigent amongst others.

4. Are the scientific methods and assumptions valid and clearly outlined? Yes, though I would have liked to see more to address linkage between the frequencies, as this is a key aspect of CIMR, having 1 to 40 GHz on the same platform.

5. Are the results sufficient to support the interpretations and conclusions? Yes

6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? Yes

7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution? Yes, given the limited ambition of the paper, though a more complete analysis of the MIMR, AMSR-E and AMSR2 literature is relevant, as CIMR only adds the L-band channel to the capability of these sensors.

8. Does the title clearly reflect the contents of the paper? It is not obvious to me why the authors choose to imply their paper is about the arctic. Fig. 2 is middle latitude, Fig. 3 compares arctic, middle latitude and tropics. Fig. 4 is arctic with a couple of paragraphs of discussion. Yes, SIC has been added compared to Wilheit. But overall there does not seem to be a particular emphasis on the arctic in the analysis. It's a global study.

The initial Wilheit figure was about open ocean at mid latitude. We added arctic simulations and sensitivity to sea ice.

9. Does the abstract provide a concise and complete summary? Yes

10. Is the overall presentation well structured and clear? Yes

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11. Is the language fluent and precise? Yes

12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? Yes

13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? No

14. Are the number and quality of references appropriate? Yes – but more on MIMR etc literature would be appropriate, as noted above.

A reference to MIMR has been added.

15. Is the amount and quality of supplementary material appropriate? No supplementary material needed

Specific comments

P2 L28 “No gap in coverage at the pole” What the authors mean is every orbit will see the pole, so there will be an observation every c.100 minutes. No gap may mislead some readers who are not familiar with polar orbiting satellites.

The sentence has been clarified. It has been replaced by “CIMR . . . will fully cover the poles”.

P2 L31 Not sure why you use the word harsh. It will observe all aspects of the arctic environment. This seems poetic language for a scientific paper.

“harsh” has been deleted

P2 L33 Could you be more specific on the range of terrestrial products that CIMR will improve analysis of, that are additional to those you have just listed. Its not clear what you are talking about.

Some precisions have been added "(e.g. soil moisture, vegetation dynamics, snow water equivalent)"

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Equation 1: You have assumed a specular reflection. How realistic is this for some of the snow and ice surfaces you are concerned with? Suggest to clarify.

As demonstrated in Matzler (2005), the distinction between specular and lambertian scattering is not an issue for conically scanning instruments such as CIMR with incidence angles close to 55° . The problem arises for incidence angle close to nadir as can be the case for cross-track sounders such as AMSU or MHS.

Matzler, C. (2005). On the determination of surface emissivity from satellite observations. IEEE Geoscience and remote sensing letters, 2(2), 160-163.

Equation 2: Why do you use finite difference rather than differentiating the code and how have you ensured that your dx is appropriate to get a robust estimate of the local gradient?

In the microwave windows between 1 and 40 GHz, the variations of T_b as a function of the different parameters are quasi-linear, therefore we can use the finite difference to compute the sensitivity and the choice of dx is not very critical (contrarily to what happens in the calculation of the gradients in spectral lines).

Equation 3: Whilst normalising like this maintains consistency with the Wilheit figure it may give a misleading impression of relative sensitivity to different parameters at a given frequency. It would be useful also to show the unnormalized figures.

Figure 4 is unnormalized with sensitivities that are plotted with logarithmic scale.

Figure 4: I do not understand this figure. What is the cause of the sharp spectral feature in the SST sensitivity around 15 GHz? This makes no physical sense to me. Please explain.

This is because the results are presented with a logarithmic scale, because the sensitivities are very different depending on the parameters. Around 15 GHz the sensitivity to SST becomes zero, this is why we have this shape.

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P8 L145-155 I am struggling to see the point in this analysis. You have a multi-channel instrument, you have already stated that the multi-channel aspect is important, its clear that single channel frequency retrievals are useless (even ignoring sensitivity to other parameters). I am not sure how this analysis gives new insights? It seems far less useful than a multi-channel information content study.

The multi-channel analysis has been done in Kilic et al., 2018. In this study, our goal is to present, in a convenient way for the users, the CIMR channels individually and their advantages in terms of sensitivity.

P9 L164-166 This does not seem to be a new finding, yet it reads like it is.

It is a confirmation and it summarizes to the community why these channels have been selected for CIMR.

P9 L169-171 I agree the major aspect of CIMR is to use these frequencies together but this is really not explored in this short paper. The paper only repeats rather well known aspects of the sensitivity of individual frequencies. It is not difficult with the calculated gradients to explore the multi-channel aspects using linear information content theory. Its not new, but it would give more insight into the use of these channels together.

As mentionned above, this multi-channel analysis has been presented in Kilic et al., 2018. Kilic, L., Prigent, C., Aires, F., Boutin, J., Heygster, G., Tonboe, R. T., ... & Donlon, C. (2018). Expected performances of the Copernicus Imaging Microwave Radiometer (CIMR) for an all-weather and high spatial resolution estimation of ocean and sea ice parameters. *Journal of Geophysical Research: Oceans*, 123(10), 7564-7580.

Please also note the supplement to this comment:

<https://os.copernicus.org/preprints/os-2020-92/os-2020-92-AC1-supplement.pdf>

Interactive comment on Ocean Sci. Discuss., <https://doi.org/10.5194/os-2020-92>, 2020.

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