The Cryosphere Discuss.,
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The Cryosphere Discussions

Interactive comment on "The role of electrical conductivity in radarwave reflection" *by* Slawek M. Tulaczyk and Neil T. Foley

Anonymous Referee #1

Received and published: 25 February 2020

Dear authors,

Thank you for further clarifying some important points. I apologize that my review read more negatively than intended, and I think part of the confusion is due to how reviews are classified in TC.

I rejected and graded the article in its 'current form' as a full-length TC research article (as we have to do as reviewers) as I believed that the original content could be distilled to a few pages and $\sim$ 2 figures with subpanels. TC did not give the option to 'recommend resubmission as a brief communication' and, had this option been available, I would have chosen to do this. I thought this recommended action was evident in my review as I stated: 'Ultimately, I think the study would be better packaged as a brief communication/letter or needs to be significantly extended with more original material

to be a full-length TC article... I think the results are useful to the subfield and it would be good to see a substantially revised version of the study published in the future'.

From the statement above it should be also evident that I have no intention to block the underlying science or act unscientifically. I gave feedback with the hope of improving the final publication to better focus on points of novelty.

From your comments, I now appreciate that you consider there to be pedagogical value to presenting the complex wavenumber form of the reflection equation. As I was focused on demonstrating numerical equivalence (for assumed permittivity and conductivity for the different forms) I arguably overlooked this point. To my knowledge the complex wavenumber form has not been presented in the glaciology literature hence could justify a longer article to make the formulation accessible.

In which case my major comments could be revised/incorporated in a full-length article as follows:

1. Equivalence of two forms. It would be highly desirable, and consistent with the pedagogical flavor of the MS, to cross-reference between complex wavenumber and complex permittivity forms of the Fresnel equation (the latter form is what I believe most of the field are more familiar with which is why I recommend this as the starting point). This should also include a cross-reference to the loss tangent as it is already used implicitly as the control parameter on the x-axis of Fig. 1 to establish loss limits. (Note: for a lossy medium tanâĄą(delta)=ε"/ε' =σ/(ε'ω) which is what I used to demonstrate numerical equivalence of the 2 equations in my attached script). There are some radar analysis applications when the loss tangent arises as the natural variable to use to infer material properties; specifically when assessing losses through a layer of unknown permittivity.

2. Volume of original work. If we assume that communicating the complex wavenumber form to glaciologists has been justified as a contribution, then my point 2 could become redundant as sections 2 and 4 are now necessary. I had a read through to check I have

not overlooked anything, and I apologize that I potentially overlooked equation 12 as a new analytic form that isn't in the literature. If this result is new then it should be heavily emphasized, as the preceding formulae in Sect 2 and 4 are textbook results. Related to this point - graphically comparing equation 12 with the full/general formula would be useful (particularly in the context of Fig 4).

Re: So, I challenge RC1 to provide supporting evidence of her/his contention that our analysis is basically old news that is not worth publishing because the radioglaciology community already knows about it

If we are using citations as a metric then Peters et al. 2005 have 175 on google scholar and quotes the full complex form of the Fresnel equations (admittedly an extra step is required to convert loss tangents into conductivities). I took this paper as the reference point for what 'common knowledge' in our field is, as I know many use it as a mini textbook. To my eyes, the form in Peters also represents the logical starting point from a pedagogical perspective (though I appreciate this is subjective).

I did not suggest that the reflectivity parameter evaluation in Sect 5 was old news or not worth publishing. Instead, I gave evidence that it could be done using the permittivity formula in Peters et al. 2005 as a definition (hence significantly shortening the article to focus on the new content). I also agreed that the results in Fig. 4 were a valuable contribution: 'In particular; the result that conductive clay can be more reflective than freshwater for certain (lower) frequency ranges and conductivities stands out'.

I think my reference to Berry 1975 was used out of context by the authors in their response on this point (originality/common knowledge within subfield). I used it to make a subsidiary point about scattering loss that can occur from a 2-phase dielectric mixture (cross-referencing Peters 2005). This could (hypothetically at least) act to reduce reflection strength from saline till relative to freshwater.

Finally, Oswald 2008, 2018 and Jordan 2018 are all considering airborne systems (150 -195 MHz) which is why I thought it would be useful to briefly discuss the impact of

the study (if any) on higher frequency systems (and potentially the typical scientific questions addressed in airborne data analysis).

I hope these comments are useful and improve clarity.

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