Review to Tulaczyk and Foley, 2020

Summary

Tulaczyk and Foley provide an assessment of the role of electrical conductivity in radar reflection from subglacial/geological electromagnetic interfaces. They argue that conductivity has typically been neglected when using radar reflection to infer subglacial material properties, which instead has been reliant on the relative permittivity (i.e. real component of complex permittivity) when interpreting radar data. The new contributions are an assessment of the low-loss limit for subglacial materials (based on old and recent literature), and parameter evaluation for the amplitude and phase of the interfacial complex reflection coefficient. Using this framework, the authors then re-examine some results from literature (e.g. Christianson et al. 2012, MacGregor et al. 2011) that could be re-interpreted in terms of the conductivity of subglacial materials rather than the permittivity.

I do agree that the role of conductivity is underappreciated in radioglaciology and I think the results are of interest to that subfield and others interested in subglacial environments. In particular; the result that conductive clay can be more reflective than fresh water for certain (lower) frequency ranges and conductivities stands out. However, I have two major concerns that, in my opinion, prevent the MS from being publishable as a full-length TC research article (see below). 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. Despite my criticisms, 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.

Major comments

1. *Equivalence of Stratton formalism with commonly-used form in radio glaciology.*

In their study, the authors adopt a form of the Fresnel reflection equation from Stratton 1941 (eqs. 7 & 8 in their study), and a large portion of the manuscript is devoted to reviewing this formulation. On the other hand, the commonly-used from of the Fresnel reflection equation in radioglaciology is given in terms of the complex permittivity (e.g. see eq. 6 & 7 in Peters et. al 2005; a paper which is often used as a mini-text book). However, for non-magnetic media (which holds for all media considered in the study) it can be demonstrated that this commonly-used form is completely equivalent to the one given by Stratton 1941; the difference being that Stratton's formula is expressed in terms of the complex wavenumber rather than the complex permittivity.

I've demonstrated the numerical equivalence of the two (full) formulas in some attached MATLAB code. I do appreciate that plotting k_alpha and k_beta parameters gives insight to low/high loss regimes (Fig. 1). However, the parameter limits of high/low loss regimes in the Stratton formulation is also phrased equivalently in terms of the loss tangent; $\tan(\delta) = \sigma/(\varepsilon'\omega)$ in the permittivity form. Additionally, as the two formulae are equivalent, the low-loss limit and high-loss limits (eq. 11 and eq. 12) can also be derived from the complex permittivity version.

In summary, I do not see a clear advantage or novelty to the Stratton equation/formulation: it is simply an equivalent form of the Fresnel equation at normal incidence to what is currently used in the research field. I appreciate it is probably unintentional, but the way the MS is written makes it appear that use of the Stratton formula is novel (e.g. lines 47-51). Therefore, the context to the equation in Section 2 and 4 appears as redundant to me. One could take the complex permittivity form of the Fresnel equations as a definition for the parameter evaluation in Sect 5, quote that there also exists an alternative complex wavenumber form, and hence significantly reduce the length of the MS.

2. *Volume of new work*

An overarching concern is that the article does not demonstrate sufficient new results for a full-length TC research article. As I interpret it, the new contributions are:

(i) Section 3: literature review which shows that low-loss assumption becomes an increasingly poor approximation at the lower end of radar frequencies used in glaciology.
(ii) Section 5: parameter evaluation of interfacial (Fresnel) amplitude and phase relations for different conductivities and radar frequencies.
(iii) Section 5: re-examination of some results in the literature, where the radar return could be better explained by conductivity than permittivity contrasts.

In my opinion, Sections 2 and 4 both nicely summarize textbook material, but do not provide any new content. Also, due to my point 1 above, I think are they are largely redundant.

Whilst I appreciate the effort by the authors in their prior EM resistivity studies that constitute the literature review, there are no new experimental results in the MS. My impression is that the new work in Sections 3 and 5 could be completed in a week (I coded section 5 in an afternoon when checking the equivalence of the Stratton equation). The re-examination of older literature, whilst relatively compelling (MacGregor et al. 2011, Christianson et al. 2012) is also not necessarily definitive (e.g. rapid variation in radar attenuation/englacial temperature could also play a role).

Additionally, I think the entire MS could be compressed into 2 figures (the results in Section 5 could easily be compressed into a single multi-panel figure, with fig 1, 2 and 3 compressed into another figure).

I therefore recommend re-writing the MS a brief communication/letter, with the focus on sections 3 and 5.

Specific comments

Title. To my mind, some reference to subglacial and/or geologic interfaces would be useful in the title (it currently reads like a very general title that could apply to a non-glaciological theme).

Magnetic materials. I don't see the rationale for presenting the general case of magnetic materials throughout the derivations (e.g. equation 7a) and in the abstract. In all the later analysis it is assumed that the free-space value applies. I think it should just be stated in a single sentence that, the non-magnetic approximation holds for the majority geologic materials (and reference the general case for interested readers).

Premise that conductivity has been neglected. I agree with the authors that the role of conductivity is underappreciated in radioglaciology. However, I do not believe it is necessarily neglected. For, example one of the key textbooks from the field summarizes conductivity alongside permittivity (Bogorodsky 1985) for subglacial materials when calculating bed reflectivity, as does Table 1 in Peters et al. 2005 (although this is expressed in terms of the loss tangent). I think most people in the field assume that other factors are dominating their interpretation of bed-returned power (in particular, spatial variation in radar attenuation).

Radar attenuation uncertainty. Related to the paragraph above, I think a paragraph commenting on the role of radar attenuation uncertainty (and potentially other terms in the radar power equation/scattering losses) in the interpretation of the past literature is desirable. This seems particularly relevant for the discussion in Sect. 5 related to MacGregor et al. 2011. Also, in reference to Christianson et al. 2012, I assume a warm/temperate layer of ice above lake Whillans would provide an alternative explanation for the suppressed bed-returned power?

Radar frequency range. I think a brief comment on (slightly) higher radar center frequencies is desirable. Many airborne radar sounding systems operate in the 150-200 MHz range (e.g. radar systems operated by BAS and CReSIS) and the Auto Phase-Sensitive Radio Echo Sounder has center frequency 300 MHz.

Multi-frequency mapping of radar reflectivity. The sensitivity of the reflection coefficient to frequency is proposed in Sect. 5 as a useful constraint on the presence or absence of conductive materials beneath ice. However, both roughness scattering loss and radar attenuation loss have frequency-dependence which should also be discussed in conjunction.

Scattering loss from two-phase mixtures. Related to above comment, an underappreciated point is that two-phase dielectric mixtures used to describe glacial till (e.g. sand and water) can result in scattering losses which act to reduce the bed reflectivity (e.g. see Fig. 3 in Peters et al 2005, which is based on foundational work by Berry 1975). A brief comment on this would be desirable (as it could impact on reflectivity comparison between conductive subglacial till and fresh pure water).

`Permittivity-matching' of imaginary permittivity/conductivity in basal ice. I wonder if (in some scenarios at least) a conductive fluid in the basal layer (as mentioned in line 172) could be transported to the basal ice layer, hence acting to reduce the conductivity contrast across the interface and restoring permittivity as the dominant reflection source. It would be useful to comment on this (even if it is to rule out my comment as being physically implausible).

Minor/technical

Fig. 1/2. It should be stated in these figures and the text that the tuning parameter, (\sigma/(\epsilon\omega)), is equivalent to the loss tangent.

Fig. 6 – a second y-axis with dB units would be useful (as in Fig. 4).

Eq. 12. Should the second \alpha_2^2 be \beta_2^2 under the first square route? This seems inconsistent with equation 9

It would be desirable to add some color to the figures.

Extra references:

Bogorodsky, V., C. Bentley and P. Gudmandsen (1985), Radioglaciology, D. Reidel Publishing Co. Page 216.

Berry M. Theory of Radio Echoes From Glacier Beds, Journal of glaciology, 15, 73, 1975.