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Interactive comment on "Monitoring the seasonal changes of an englacial conduit network using repeated ground penetrating radar measurements" *by* Gregory Church et al.

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General Comments

This manuscript reports on radar imaging of an englacial conduit located at ca. 100m below the surface of Rhongletscher. The authors image this conduit repeatedly over several years with a GPR using antennas with 25MHz center frequency. The radar data constrain the seasonal and interannual evolution of this conduit. The authors use the radar data to put constraints on conduit thickness, reflectivity, and width. They use full-waveform simulations to tackle the fact that the conduit represents a thin layer, which is at times near/below the vertical resolution of the radar data.

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I found this manuscript to be interesting and insightful. For the most part, it is methodologically sound. I am particularly impressed with the fact that the authors recognized the need to tackle the thin-layer problem in a rigorous way. The investigation of the dependence of conduit reflectivity on conduit thickness, which is near the resolution limit, represents a valuable contribution to radioglaciology.

There are, however, several aspects of this manuscript that, in my opinion, can be improved:

(1) For instance, I am uneasy about the fact that critical quantities such as the conduit reflectivity and thickness are reported without error bars, even though they are estimated from radar data that, naturally, contain noise. In all instances, uncertainties in quantities derived from data should be reported.

(2) I find the repeated references to 'dry' and 'wet' glaciological environments to be not salient to the interpretation of radar results presented in the manuscript. It is a somewhat artificial distinction that worked in Baelum and Benn, 2011, but seems to add little here. I think that the manuscript will improve if the use of this distinction will be limited or eliminated.

(3) Whereas I appreciate the insights from the full-waveform modeling of the thin-layer problem, I do not think that the model output can be used to fully quantify uncertainties on conduit thickness and reflectivity derived from GPR data. Their simulations do not include any noise, but the real GPR data do contain noise. If it is possible, the authors should run additional simulations with a realistic level of noise included in the model setup. If this is not feasible, at least a discussion of this limitation should be provided.

(4) The authors invested a lot of effort into considering the impact of vertical resolution (range resolution) on their data interpretation. However, there is no consideration given to the effect of horizontal GPR resolution on their results. This impacts the mapping of the conduit width and shape in particular. But it is also possible that the calculated reflectivities result from the radar signal reflecting from a large area (the Fresnel zone)

with a mixture of different real reflectivities. It is relatively straightforward to calculate the diameter of the Fresnel zone at any given depth. My quick estimate indicates that, in this case, the Fresnel zone may be almost as wide as the width of the conduit detected in this study. So, this could be a significant complication.

Specific Comments

Line 52 – 'in cold ice' rather than 'on cold ice.'

Line 60 – By definition of the word 'annual' (occurring once a year), the sequence 2012, 2016, 2017 is not a sequence of annual measurements.

Line 121 – geometrical is misspelled.

Lines 130-132 – When expressed in terms of energy ratio, a reflection coefficient cannot have negative values. This is why the reflection coefficient defined as the ratio of the reflected wave energy to the incident wave energy is equal to the second power of the amplitude reflection coefficient. The latter is defined as the amplitude ratio of the reflected and the incident wave. As written currently, this passage violates energy conservation because reflection cannot result in negative energy. In reality, the authors are talking about the amplitude reflection coefficient, which can have negative values because of phase reversal. But they misrepresent this as the energy reflection coefficient.

Line 132 – Strictly speaking, the way that the reflection coefficient is treated here (e.g., Table 3), it is only a function of the dielectric permittivity, not electrical properties such as electrical conductivity or electrical resistivity.

Table 3 – I appreciate that the authors are copying this table from a prior publication, but some specific assumptions underly calculations of these reflection coefficients by the original authors (Baelum and Benn, 2011). These values of the reflection coefficient have been calculated with specific assumptions regarding the porosity of these materials (e.g., granite and sand). These values are not some generally applicable values

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for all granites or wet sand. The footnotes to this table should state the assumptions underlying these values. Also, it would be much more realistic to re-calculate a range of values for granite and wet sand making assumptions that span some reasonable range of porosities for these two materials.

Line 152 – It does seem that there is at least a faint reflector on the right-hand side of the image for September 2012. The strength of these faint reflectors from September 2012 data appears about equal to the strength of reflectors from figure 3b.

Line 155 – I am not sure that this conclusion is well justified by observations (see my comment above for Line 152).

Line 187 – I must admit that I do not understand the logic of picking -0.11 as 'the boundary between dry and wet glacial reflection environment' in this study. Do the authors really expect that there may be 'granite' in the middle of the ice column? Undoubtedly, the only plausible materials in the conduit are either glacial meltwater, sediments, or air. The only 'dry' material that could be there is, then, air? This part of the discussion must be improved. The entire distinction of dry vs. wet seems artificial and unnecessary in the context of this paper. The authors are allowing themselves to be too influenced by Baelum and Benn, 2011.

Line 190 - The reflectivity of zero means that there is no reflection. If the channel was dry (full of air), then according to Table 3 the reflection coefficient should be +0.28. Hence, the logical interpretation of reflection coefficient being zero is that the conduits either close entirely or become so thin that the radar wave passes through them without detectable reflection. This should be the conclusion from the observation of near zero reflection, not the relatively vague statement that 'the channel is not a wet environment.'

Table 4 - I find this table confusing. For one, conductivity and magnetic loss are given the same symbol (Greek sigma). Are they the same thing? Rather not since they do not have the same values. They may be interrelated since I do not recall that magnetic loss is part of Maxwell's equations, whereas the three other parameters are

(permittivity, conductivity, permeability). Since the governing equations for the related calculations are not shown, it is difficult to really envision how these parameters are used in the forward modeling. Finally, I am always bothered when such parameter values are given without uncertainty ranges. We rarely know, or can assume, specific parameter values without considering what range they can really have.

Line 234 - Whereas I am impressed by the full-waveform modeling used to get around the thin-layer problem, the approach taken here has at least one shortcoming. Unlike synthetic data, real data are noisy. The authors contend that they can use their real, noisy data to determine the conduit thickness to within +-0.15 based on idealized, noiseless simulations. They make a similar claim about the reflectivity being within +-0.1. The noise-free simulations represent a useful end-member but they cannot be used to determine the uncertainty on conduit thickness and reflectivity based on real data. The authors should analyze the power spectrum of noise in their GPR data and then add this noise to their forward simulations. This approach will enable them to quantify more realistic ranges of uncertainties on conduit thickness and reflectivity.

Figure 9 and its discussion in text - I got utterly lost in keeping track of what thickness and what reflectivity the authors call 'true', 'apparent' 'calculated' and 'observed'. This needs to get streamlined. I think that the best approach is to use only two of these terms. 'Apparent' should be used when talking about conduit thickness and reflectivity derived from GPR data. 'True' should be used to describe the equivalent quantities obtained from forward simulations.

Line 287 - delete the unnecessary verb 'are'

Line 292 - 'Remnants' 'are' (not 'is')

Line 299 - This artificial distinction between 'wet' and dry' glacial environment is really an unnecessary oversimplification.

Line 319 - 'appears as a single specular reflection'

Line 325 - ’ plotting the reflection normalized by the amplitude of the basal reflector’

Section 5.3 and Figures 10 and 11 - When discussing and plotting conduit shape, it would be useful for the authors to calculate the Fresnel zone’s size for their radar at a depth of the englacial conduit. I made a quick and dirty calculation and I’m getting some dozens of meters. So, the Fresnel zone for this 25 MHz radar at 100m depth below the surface can be comparable to the conduit’s width. Perhaps the conduit width appears artificially larger than it is because of this limit in horizontal radar resolution? In any event, the size of the Fresnel zone should be considered here. The authors have invested a lot of effort to investigate the limits of vertical resolution, but there is no effort to quantify the horizontal resolution.

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