Comment on tc-2021-220
Slawek Tulaczyk (Referee)

This manuscript reports on the results of a regional radar survey of the Devon ice cap. The data are interpreted in the context of prior work by the same team in which they proposed the presence of briny subglacial lakes in this study region. The results presented here broadly corroborate this prior contention. However, the authors also see evidence for extensive areas that are not lakes but likely brine-saturated sediments. Generally, the new dataset yields a detailed picture of the complex subglacial hydrology beneath the Devon ice cap and may be helpful in a range of glaciological and microbiological studies in the future. The manuscript includes an excellent discussion of the possible microbial habitats beneath the Devon ice cap and overviews their relevance to search for extraterrestrial life on icy planetary bodies.

Overall, I find this manuscript to be informative and worth publishing. I have several specific comments on the methods and results sections (see below). However, once I got to the discussion and results sections, they seemed to be very well crafted with proper caveats stated where they are necessary. The figures are loaded with information and clear. Citations are extensive and relevant. I did suggest a few additional papers that the authors may want to take into account. I do think that there are several places where more methodological rigor can be introduced and a few places where statements can be made in a more even-handed way.

Specific comments:

Line 57: It is much more relevant to this study to give the radar wavelength in ice, not in air.

Line 83: The significant difference between the median and the mean suggests that the cross-over errors are not normally distributed. I would be interested in including a histogram of the errors as a figure. Alternatively, the authors should at least point out the deviation of this distribution from the Gaussian distribution. This is important because it is enough for a standard normal distribution to give the mean and the standard deviation to describe the underlying probability distribution function fully. However, more complex distributions (e.g., multimodal, asymmetric, or skewed distributions) need to be described...
by more than just the two first moments. With non-Gaussian distributions, one also
cannot assume that 68% and 95% of data fall within one and two standard deviations
from the mean.

Line 94: This statement is not incorrect, but, in the context of this study, it sets up an
unrealistic juxtaposition of subglacial conditions. Why would the subglacial bedrock be dry
in the study area if there is so much water in lakes and sediments in the region? Ice over
bedrock saturated with brine could produce higher radar reflection strength than ice over
a subglacial lake filled with very fresh meltwater (e.g., Tulaczyk and Foley, 2020).

Line 113: It is entirely unclear how the authors choose where to draw the line designating
Nh. Why is it chosen to be at such a value of correlation coefficient (ca. 0.1)? Why not at
0.2 or 0.3? Is the correlation coefficient magnitude here R or R-squared. Estimation of the
attenuation coefficient is an important methodological step, and it needs to be fleshed out
more than it is here.

Figure 2: It is clear from the distribution shown in this figure that quite a range of quite
different lines, representing different attenuation coefficients, could be fit reasonably well
into this distribution. For one, there is a distinct peak to this distribution (at ca. -42dB and
480m), with the resulting linear fit being determined by extreme tales at low and high
frequencies. I'm sorry, but I do not buy the relatively narrow uncertainty of 3.1 dB/km
that the authors assign to their fit. Linear regression can be performed in a way that yields
the uncertainties in the two parameters that define the linear fit, one of which is the slope
of the line, from which attenuation is calculated. The authors need to report these
statistically-based uncertainties on their attenuation estimates. The validity of the method
used by Schroeder et al. (2016) may be dependent on the specific distribution of the bed
power vs. thickness. In this case, this distribution does not support a tight linear fit.

Lines 131-132: As in the dry bedrock example, the authors use an extreme end-member
here and create a false sense of the singular relationship between high specularity and ice-
on-water interfaces. In reality, ice overriding sediments can easily have a flat interface,
particularly if the sediments are weak. However, I have seen even deglaciated granitic
surfaces that are smooth enough on the scale of 1-10m that they would have high
specularity.

Line 156: The mean cross-over error of 17.6 m itself is associated with uncertainty. In
reality, you do not know if it is 17.6 m or some larger or smaller value. This is why the
concept of uncertainty propagation has been invented. Please, calculate the uncertainty of
the mean cross-over error and propagate it into your calculation of uncertainties of the
hydraulic head.

Line 157: Similarly, the uncertainty of 0.5 m for the ArcticDEM seems unrealistic from my
experience. The authors should look at independent estimates rather than just quote a
potentially optimistic estimate produced by those who generated ArcticDEM. For instance,
the analysis of Xing et al. (2020) suggests a 2-sigma error of about +-5 meters for the
Greenland ArcticDEM analyzed by them.

Lines 200-201: This statement is somewhat misleading. The paper by Peters et al. (2005)
uses complex permittivity, which incorporates the effects of real permittivity and electrical
conductivity on radar reflectivity of the bed. Yet, later (Line 203), the authors interpret the
observed spatial variations in the basal reflectivity as transitions between wet and dry
bedrock conditions. Whereas this simplified interpretation is commonly used, it is not
strictly correct, as pointed out in Tulaczyk and Foley (2020) and in the open discussion to
this TC paper. Spatial variations in relative bed reflectivity may be due to changes in the
salinity of subglacial water, which influences the imaginary (electrical conductivity)
component of complex permittivity. The relative amount of subglacial water (i.e., the wet
vs dry language used here), influences the real part of complex permittivity. In reality, the authors could be looking at subglacial materials with the same amount of water but seeing very different radar reflectivities because in some places, this water is fresh meltwater, and in others, it is hypersaline brine. The effect of electrical conductivity seems particularly pertinent to this study, given that the authors propose that very hypersaline brines (five times more saline than seawater) are present in some parts of their study region. The role of electrical conductivity in the radar reflectivity of glacier beds needs to be tackled in this manuscript head-on. The authors cannot say in some parts of this manuscript that there are very saline (conductive) brines in the region and then default to interpreting bed reflectivity as if only the real part of complex permittivity mattered in radar reflection.

Line 221: There is literature focused on estimating lake bathymetry from their size and the topography of lake surroundings (e.g., Heathcote et al., 2015). Perhaps the authors can lean on such prior research to get a better handle on lake depths?

Line 231: Can you quantify how 'flat' or 'rough' on the scale of the radar wavelength does the ice bed have to be to yield specularity of >0.4? Just from intuition, it seems that 0.4 can still correspond to pretty rough surfaces. I am not that comfortable associating specularity of such low magnitude with only being consistent with ice-water interfaces. Again, ice-on-weak-sediments can make pretty flat interfaces too. Moreover, such an interface can be highly reflective. Particularly when the subglacial fluid is hypersaline and/or if the subglacial material contains clays (both are electrically conductive materials). Furthermore, an extensive area of weak subglacial sediments would lead to the ice surface slope being shallow, which would also result in hydraulic flatness. All three criteria for subglacial lakes used here can also indicate wet, weak sediments.

Line 286: Can you elaborate why you do not think that there is a widespread area of wet conditions here?

Lines 297-298: What about the possibility that less saline fluids (e.g., fresh subglacial meltwaters) cause the low bed reflectivity in this high specularity area? This seems like an equally plausible explanation as compared to the 'dry bedrock' one.

Lines 454-455: Three papers have been published in GRL this summer, casting doubt on the interpretation of high reflectivity anomalies beneath the Martian SPLD as liquid brines (Bierson et al., 2021; Khuller and Plaut, 2021; Smith et al., 2021)