Review of: *Aerogeophysical characterization of an active subglacial lake system in the David Glacier catchment, Antarctica*
Submitted to: The Cryosphere
Reviewer: Nicholas Holschuh

General Comments:
This paper provides a review of data collected over David Glacier from a helicopter variant of the UTIG HiCARS radar system. The authors provide an honest interpretation of the data, acknowledging the limits of radar's ability to diagnose subglacial hydrologic characteristics, and add to the growing body of literature highlighting discrepancies between altimetry and radar delineated subglacial lakes. The work is well written and thorough. There is room to expand on the interpretations in a few key areas (mentioned below), but overall I found this to be a nice contribution to the literature and nearly ready for publication.

Technical Comments:
I appreciated the thorough discussion of attenuation in this work, but a slight reorganization can make the caveats of the applied method clearer. On page 12, line 25, you state that you assume that basal reflection coefficients are independent of ice thickness, but then spend much of the later sections articulating why that assumption is wrong, which requires you to restrict the data used in your attenuation fit. I think it would be more transparent to discuss the basis for assuming that both depth-averaged attenuation rate and basal reflectivity might be depth-correlated (presently stated at P.13, L3-5, and P.13, L15-16), and then proceed with the method in the face of those known caveats. This would provide a succinct description for future users of the method, and make it clearer to the reader why your final estimate is a lower bound.

It is worth mentioning, based on the results presented in figure 9, that your final returned-power distribution spans 70 dB. This represents a major challenge in radar interpretation more broadly, and one I have faced in my own data interpretation. As you point out, the range of reasonable reflectivity variation due to variation in dielectric properties of the substrate material is ~20-30 dB, not 70 dB. The fact that variance is so high means we are missing a critical (and potentially, dominant) control on the values. It might be worth pointing this out to the reader.

When discussing why altimetry derived lakes may not be obvious in radar data, I think it is important to keep in mind that both radar system characteristics and specific lake characteristics (such as water depth) will affect how the lakes present in radar data. Different radar systems will have different sensitivities to what is likely a double interface (ice-water, water-rock), with water column thickness either (a) falling under the range resolution of the system over the whole lake, or (b) pinching out to a water-layer thickness below the range resolution at the lake margins. You know from the altimetry data presented in figure 4 that the water layer thickness changes on the order of meters (not tens of meters), which means that thin film effects may provide an (at least partial) explanation the character of lakes in both this survey and those in the literature (see Christianson et al., 2016 for more details on thin film effects in radar). You would expect lakes across surveys to appear different, given the range the system characteristics for radars used in the papers cited:

- Impulse 3 MHz system: 28 m range resolution in ice, 8 m in water (Welch et al., 2009) (Langley et al., 2011)

- UTIG/SOAR data, HiCARS 4MHz Bandwidth: 21 m range resolution in ice, 6.25 m in water (Carter et al., 2009)(Langley et al., 2011)
- HiCARS 14MHz Bandwidth: 6 m range resolution in ice, 1.8 m in water) (Wright et al., 2014)

A discussion of a damped signal from thin lakes or the effect of water depth more generally would be a useful addition somewhere in this paper. This feeds back into roughness interpretation, as a thin lake lid may look quite rough (as mentioned in the very last line of the conclusions).

Finally, I think the discussion of lake D2 should be made clearer. In several places, you state that the behavior hasn't changed since the Smith et al. paper (e.g., P.14, L.27) but it is not clear how to interpret that statement given that part of the surface in that region is lowering and part of it is rising (rapidly). Perhaps there is a geolocation error in Smith et al., 2019, or the lake boundaries have changed since then? In general, I think your observations around D2 are fascinating, and warrant further discussion.

Line-Item Corrections:

Page #: 1-4 Line #:	I think you've done a really nice job of summarizing the literature here - this is a great resource to provide the community.
Page #: 9 Line #: 7-8	It would be helpful if you put numbers on the uncertainty evaluation here (along-track bed elevation variation [height over width] and cross-track beam width)
Page #: 8 Line #: 1 (fig 4)	Is the dz presented here normalized by the dt between observations? If not, it should be, so that you can reasonable compare observations that span 2003-2017 and those that span 2009-2017.
Page #: 8 Line #: 13	The phrasing of this sentence is a bit awkward - perhaps: "Due to the existence of side lobes in the transmit/receive beam the first return criteria may understeimate ice thickness in rough terrain."
Page #: 9 Line #: 5-8	Additional context would help the reader understand these errors. Define what it means to be "high bed slope"
Page #: 11 Line #: 1-3	It would be useful to show how these errors might affect hydrualic potential near the lake boundaries. One way to do that would be to present an error map.
Page #: 11 Line #: 7	The clause "cyclical active lakes are manifestations of feedback loops within this system" feels out of place, I suggest removing it.

Page #: 11 Line #: 24-26	10m of hydraulic head seems like a very low uncertainty given the bed-error estimates presented above. More justification is required before you quote that number, especially given the following sentence.
Page #: 12 Line #: 16-24	In this case, you have a known calibration surface in those data that span Drygalski Ice Tongue shown in figure 1. Using that surface would allow you to bolster your reflectivity analysis - if you are choosing not to use it in this study, you should justify that choice here.
Page #: 12 Line #: 25-28	Here is where it would be useful to introduce the known sources of errors in this method (instead of starting with the assumption that they don't exist). As you state, temperature is correlated with ice thickness, and water content is correlated with temperature, introducing two factors that will complicate your interpretation of attenuation rate using the data alone.
Page #: 8 Line #: 1	Figures 4-8 might make more sense in the results section than the methods section.
Page #: 13 Line #: 15-16	It might be useful to do a back of the envelope calculation to show that 1300 m is deep enough to expect a melted bed here.
Page #: 14 Line #: 8	Here, in your discussion of sources of variance in reflection coefficient, you should introduce the idea of thin films / the effect of water depth.
Page #: 14 Line #: 26-27	Here is an opportunity to clarify your interpretation of lake D2 dynamics. The statement that it hasn't switched from draining implies you think the surface elevation gain on the edge of the formerly defined lake (which corresponds with the hydropotential basin) is not a change in behavior. More text here to help the reader understand your position would be useful.
Page #: 15 Line #: 14-17	I find this statement confusing, and I think it is because I don't know what "the region of interest" is. Do you mean the lakes? The places where ice thicknesses are the highest don't seem to be over the lakes, but are where you would expect bed power to be the highest (as they fall well above the solid lie in Figure 8). It would help to clarify this point.
Page #: 17 Line #: 6-8	Do you provide a new outline for lake D2? That would be a useful outcome of this study. Also, this part of the text is an opportunity to clarify your thinking, is it draining or is it filling? Is this different from its behavior as stated by Smith et al.?

Page #: 17 Line #: 16-17	Again, do you think D2 is still draining? Despite the huge positive anomaly nearby? More explanation is required.
Page #: 18 Line #: 8-9	This statement is hard to defend -- can you imagine lifting the ice surface up 10 m through changes in water storage but not fully decoupling the ice from the rock to form a lake? You seem to argue this is analogous to what is observed at Thwaites, but I find that section of the text difficult to follow. If you want to keep this interpretation in, more discussion / justification is required.
Page #: 19 Line #: 21-22	This point is a really interesting one, and I think should come up in the discussion earlier so the reader is prepared for it in the conclusions. Perhaps introduce this idea up near P.14, L,8.