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Interactive comment on "A 3D map of englacial attenuation rate from radar reflections at Law Dome, East Antarctica" *by* Syed Abdul Salam et al.

Anonymous Referee #1

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Review of "A 3D map of englacial attenuation rate from radar reflections at Law Dome, East Antarctica" by S. Abdul Salam et al.

8 November 2020

This manuscript describes the development and application of a new method for measuring englacial radar attenuation using an extensive radar dataset from Law Dome on the East Antarctic coast. The rationale for a new method is explained and multiple parameter decisions are considered in the new algorithm, which possesses the advantage of not requiring direct tracing of internal radar reflections. The resulting dataset

Explored within this manuscript is an intriguing idea that I first considered upon reading Hills et al. (2020, doi:10.1017/aog.2020.32): automatically classifying the "scope"

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Discussion paper

vertical traces of a radargram in terms of internal reflections to quickly trace prominent reflections and then derive attenuation rates. This manuscript aggressively pursues this idea using an appropriate dataset (they're forgiven for not knowing Hills et al. first presented part of it). However, in my view the manuscript overreaches and in a way that is beyond the scope of ESSD as I understand it. IMO, new methods should be previously validated for a journal focused on datasets, and also the presented datasets should have clear potential for wider use. I cannot presently assume that is likely of this attenuation-rate dataset, as I am still trying to fully unpack the method and the algorithmic uncertainties alone seem large. As such, I do not consider my review exhaustive as there are multiple major issues to be addressed and iterated on.

Major concerns

1. It's unclear whether the returned power peaks the algorithm detects are credibly interpreted as internal reflections rather than instrument or environmental noise. As best as I can tell, there is no assessment of the horizontal continuity of the candidate reflections between traces, which seems essential to making that case. One might instead argue that the present approach is a parsimonious way forward, but that specific argument is not made and the reader is left to assume that continuity is not important in this case. Perhaps the cross-over errors on the resulting attenuation rates support the argument that the signals are real enough? But that's not a great validation. If the candidate reflections don't meet some minimum threshold of horizontal continuity (10 km?), then I think they should be dropped. The 60-m depth interval for reflections seems reasonable, but no clear physical basis is provided for it (e.g., multiple of range resolution of radar system, qualitative examination of radargrams).

2. After reading the manuscript twice, it's still unclear to me what the "3-D" attenuation-rate field is, and I'm concerned that it's simply the gridded interval attenuation rate between each candidate reflection. If so, that would appear to explain the large range in attenuation rates discussed and the need for the N>0 constraint, including very low values, because this approach would place unjustified confidence in the assumption

of vertically uniform reflectivity. Layer reflectivities are noisy, and the vertically uniform assumption is the only present path toward an estimate of the *depth-averaged* attenuation rate across multiple (more than two) reflections (e.g., Matsuoka et al., 2010; MacGregor et al. 2015). Interval attenuation rates should only be calculated if the layer reflectivities are much better constrained than they currently are, and limited efforts on this topic have not yet demonstrated calculating interval attenuation rates are sufficiently fruitful to apply at the scale considered here (Holschuh et al., 2016, 10.1002/2016jf003942; Hills et al., 2020).

3. Is there any relationship between the attenuation-rate field and ice flow across Law Dome? This is not discussed at all, and so it's hard to know if the many potential uses of this dataset discussed in the introduction are actually relevant if there is no evaluation of the final product's potential utility?

Minor concerns

Abstract. I assume that the wind speeds are mentioned because of the potential for unconformities, but lots of places on both ice sheets are windy and don't have unconformities, nor is their effect upon attenuation rates known. So is this really necessary to mention in the abstract given his unclear relevance at best? The attenuation rate values are never mentioned in the abstract, which is odd given its centrality to the study.

16: The EAIS volume is better described as *potential* sea-level rise.

22: Easterbrook (1999) is an odd general reference for the statements made and a more specific one should be used.

26/7: Has any study actually used attenuation/temperature from radar to constrain GHF directly? I don't believe there are.

36: Those studies assumed the *attenuation* was proportional to ice thickness (not rate), so that a depth-averaged attenuation rate can be estimated.

71: “Mapping function” is an unusual term. Is this an inverse model? I realize these may be effectively synonyms, but IMO “mapping function” does a lot of work here for a task (temperature from attenuation in 3-D) that has not yet been demonstrated.

101: What is this wind speed? The mean annual value? Without further specificity it’s meaningless.

110: Are these data SAR focused?

116: Specify at least once that this is the “real part of the relatively permittivity”. “Permittivity” as it stands is vague.

117: Only one of the properties listed (conductivity) is strongly temperature-dependent.

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