

Interactive comment on “A Network Model for Characterizing Brine Channels in Sea Ice” by Ross M. Lieblappen et al.

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This is potentially a useful paper based on producing a network model of sea ice brine microstructure. However, I have found it extremely difficult to review. My main criticism being that based on network theory as it is, and with many of the references relating to this (e.g. Newman, 2011; Pierret et al. 2002; Delerue et al. 2003), much of the terminology and techniques will be completely unfamiliar to the general reader of The Cryosphere. I believe therefore that before full publication the manuscript needs to be restructures in a form that will make it much less opaque to readers who are not familiar with the methods presented. Without this I feel that any impact that it might have will be substantially diminished. I give below a series of specific comments which are intended to indicate what I see are some of the major issues and, in part, how they

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might be addressed.

The first three comments relate to what I believe should be a standard introduction to the sampling and presentation of the initial measurements.

1. Please describe the ice cores. In other words, in each core, what was the thickness of frazil ice at the top, the thickness of columnar ice beneath, and any indication of platelet ice at the base? This is important in terms of reference for the interpretation of the inferred microstructure.
2. Were the cores sampled on site or after transportation? If the latter then please give details of how the cores were treated between extraction and sampling.
3. In section 3, with reference to Figures 2 and 3, please explain what the different parameters plotted are, what you might expect them to show, and how they relate to each other. For example, what is expected to be the relationship between brine volume fraction and specific surface area (Spor). In investigating permeability in sandstone samples Zhang and Weller (2014)* have demonstrated that there is a relationship between fractal dimension and Spor. Would any such relationship be expected here? Explain the Euler number for those unfamiliar with it. How is the degree of anisotropy derived? Given that the lower parts of the cores are explained to have a significant degree of uncertainty associated with the measurements (quantify this?) does it make sense for the scales for Euler number and connectivity to be dictated by the lowermost samples?

*Zhang & Weller, 2014. Geophysics, 79, D377-D387.

The above comments are in fact relatively introductory and indicate the need for a clear explanation of the background to the study, how the initial sampling was carried out, and what the initial measurements show. Unfortunately I find that from this point onwards the manuscript becomes confusing and is not at all well explained or illustrated for the more general reader.

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4. I find section 4.2 extremely confusing, including the figures that accompany it. Figures 5 and 6 ostensibly show the 5 “largest” brine channels from each of the two cores. I assume that one row represents 1 brine channel shown as a sequence of samples through the core in depth order. However, the colour scale suggests that the throat size goes to zero at many points i.e. a pore terminates. I find this hard to understand in the context of the fact that a single row represents a “large” brine channel. Please clarify.

The discussion on branches, with reference to Figure 7, is similarly very confusing. In Figure 7 what is the horizontal scale for each separate line representing a brine channel? What is the significance of the fact that one brine channel appears to pass almost right through a sample but the others do not?

I cannot at all understand the significance of Figures 8 and 9.

5. Similar comments apply to sections 4.3, 4.4 and 4.5, and their associated Figures. An excellent example of network terminology that will be unfamiliar to most is the statement “. . .treated the network as a directed graph. . .” etc.

In essence therefore, although I believe that the work presented is ultimately publishable, to be so requires considerable restructuring of the manuscript and I urge the authors to do this. There needs to be a much clearer explanation of the techniques, probably a reduction in the number of figures including clear explanations of what they represent. A somewhat broader review of previous work on looking at the interconnectivity of brine channels in sea ice would also not be remiss.

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