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Interactive comment on "In situ measurement of low-frequency sea-ice dielectric properties and implications for tracking seasonal evolution of microstructure" *by* M. O'Sadnick et al.

Anonymous Referee #1

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In this manuscript, Megan O'Sadnick and her co-authors present measurements of sea-ice dielectric properties in the frequency range below 100 kHz, which they correlate with independent measurements of sea-ice microstructure.

There are only very few carefully documented, published measurements of low-frequency sea-ice dielectric properties available, which is why this contribution is in principle a welcome addition to the sea-ice literature that can certainly be published subject to some revision.

How much revision is required depends primarily on the intended purpose of this contribution, which is not fully clear to me: if this paper is primarily meant as a paper that describes this particular data set clearly and thus makes it possible for others to make

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Discussion paper

use of this data, this is a nice contribution that can be published after removal of much of the correlation analysis of section 3

However, if the primary purpose of this contribution is an improved understanding of the relationship between the microstructure of sea ice and its dielectric properties, a major revision is needed that will require further analysis.

Since I do not want to judge the ideal purpose of this paper, I will leave this decision to the authors.

If they were to aim for a paper that provides new understanding, I believe that large parts of section 3 will have to be re-written, for which additional analysis is needed. This is because in my opinion, a focus on correlations is insufficient to provide new understanding on this particular topic. This is because much of the bulk behaviour is well understood (i.e. the impact of T on the dielectric properties of either liquid or solid), and the present analysis remains too superficial to test the robustness of this existing understanding.

For example, sticking to the dependence of epsilon on T, it is already known beforehand that this correlation will be based to a substantial degree on the correlation between the temperature and the brine volume. Hence, a mere correlation with T across all possible measurements of brine volume will be dominated by the changes in brine volume, rather than providing any insights in the role of T for epsilon.

While the authors partly address this issue through their cross-correlation matrix shown in Table 2, this table primarily reflects our existing understanding (high T correlates well with high brine fraction), but does not provide many new insights.

In addition, the extensive work of Buchanan has addressed many of the questions discussed here in more detail, and it remains unclear to me where this work truly goes beyond his existing work.

Hence, in summary, for section 3 it'd be very helpful to have a more concrete overview of what we know beforehand, whether or not we can test this previous knowledge with the data presented here, and how such tests then provides possibly new insights. For example, sticking to the temperature example, if our current understanding suggests that epsilon' increases with T in pure ice and decreases with T in brine, then it'd be interesting to compare measurements at different T for both the data points with high brine fraction (where epsilon' then should decrease with T if our understanding is correct) and then for the data points with low brine fraction (where epsilon then should increase with T if our understanding is correct). Such more in-depth analysis would then allow us to test more robustly if our current understanding is correct or now.

More detailed comments:

- I really enjoyed reading the intro, background and methods, they were very clear, and extremely well written, I find.

p.4, l.33: How was the gas content of the ice estimated?

p.6, l.8: Only data from March onwards are shown, it seems. It might be good, however, to indeed show data from January to allow one to appreciate the temporal evolution before the first data set.

section 3.2: Error estimates are missing entirely from this section. I doubt, for example, that brine-volume fractions are accurate enough to allow a qualitative statement such as the one given in line 24 on page 6

p.6, l.26-35: (and other places): the reference of comparisons is sometimes not clear. For example, line 30 seems to refer to a spatial increase within the top metre within May (?), while the sentence just before describes a temporal change from March to May. The next sentence then compares the complete uppermost metre in May (?) 2014 to the ice below, rather than the change within the top-most metre. These different comparisons make it sometimes difficult to follow the description.

The same holds, for example, on page 7, l.7-8: First, a temporal comparison between

2013 and 2014 is presented, but the term "similar trends" in the following sentence appears to refer to changes within a single year, which is a bit confusing. It'd be good to check the entire results section for these kind of inconsistencies.

p.6, l.38: I recommend to drop "linear"

p.7, l.14: is this March 2014?

p.7, l.22: I don't think it's necessary to explain the meaning of a significance level

p.7, l.34: "understood" seems too strong, as the following doesn't provide understanding but only the source of the correlation

p.8, l.6: I suggest to start a new paragraph before "A significant"

p.8, l.21: To leading order, the interrelation between T, S and brine volume is not complicated at all, as brine volume is simply given as const*S/T.

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