Response to comments: Monitoring the temperature dependent elastic and anelastic properties in isotropic polycrystalline ice using resonant ultrasound spectroscopy

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1 Introduction

The following provides responses to all comments received during the on-line discussion of this manuscript, including those from reviewers and other short comments. We would like to thank all reviewers and members of the academic community for their contributions, and for providing valuable feedback during the open discussion period. Additional annotations from attached supplements have been considered in the revised version of the manuscript.

2 Response To SC1: Leighton Watson

– Comment: "In line 8, page 3, it is stated that "iteration is terminated for values of chi-squared of between 12 and 14." Does this mean that the final value of chi-squared is between 12 and 14? These values seem large. Two possible explanations are (1) that the sample is not isotropic or (2) that the error estimates are overly optimistic. Do you have a sense about which one of these explanations might be more important? Did you try to fit an anisotropic model e.g. VTI? And see if that improved the fit or increased the value of chi-squared (the latter is more likely)?"

Response: Yes, these are the final χ^2 values. We attempted to minimise the χ^2 values for every inversion to a similar level in order to ensure all the results would be comparable. In most cases, the fit of the inversions ceased to improve beyond a certain number of iterations. For some sets of observed frequencies, additional iterations of the inversions may have resulted in better fits, but then theses inversions would not be comparable with those sets of peaks that would not converge any further. The resulting range of χ^2 values are those that represent a compromise between all the datasets. We have made some changes to the text to reflect this in more detail (see track-changes file page 5, lines 15 to 25).

Given what we know about the microstructure of standard ice, it is most likely that our estimate of σ_d, which was derived from a limited number of repeated measurements (compared to the work of Watson and van Wijk (2015), for example) is a lower bound.

We have expanded on our understanding of the (an)isotropy of standard ice from EBSD data, and our error estimates in the manuscript (see track-changes file page 4, lines 5 to 15). We have several EBSD analyses of samples made in this way (Prior et al. (2015), Vaughan et al. (2016, in prep), Qi et al. (2016, in prep), Seidemann, several unpublished data sets). All show a close to random CPO. When calculated (using a Voigt-Reuss-Hill approximation), average maximum Vp anisotropy is <2%. The orientation of this maximum in different samples is different, suggesting that the small anisotropy we see relates to a small sample volume (of a few thousand grains). The whole sample used in this paper contains of the order of 1×10^7 grains and these small magnitude local effects will be averaged as isotropic in the columnar samples by the resonance method. Any uncertainty in anisotropy in the case of these samples, lies in the distribution of porosity, which, while limited, is almost certainly non-uniform in its distribution, and not aligned in any form of symmetry.

– Comment: "In addition, I am surprised by the relative contribution of the first peak to the overall chi-squared value, as shown in Table 1. It is very small. In the work by Kasper and I we found substantial misfit between the measured and predicted frequency of the first peak. This is described as a 'curious property' of RUS measurements by Migliori and Sarro (1997). I am intrigued why you do not see similar behavior."

Response: It is interesting that our first peaks gets a good fit, but this 'poor fit' phenomena for the lower frequency peaks has never been understood or explained, as made clear by the quote from Migliori and Sarro (1997). It may be possible the lowest peak is a resonance that is part of the system that includes the transducers/stand? In this case we are using samples much larger than those typically used in the literature or with rock, which may affect the influence the system on the lower frequency excitations.

– These comments also included a supplement in the form of an annotated .pdf, based on which, some additional edits to the manuscript were made. Of particular note was the following comment: "Any reasoning for attributing the dominant mechanism to pre-melt water films developing on grain boundaries?"

In response, we have included the following modified paragraph (see page 7, line 17 to 25 of the track-changes file):

– "Quasi-liquid films can form on ice grain boundaries at temperatures above -30°C (Dash et al., 1995) (although the exact temperature associated with the onset of pre-melting is subject to some uncertainty, influenced by impurities, grain boundaries (McCarthy and Cooper, 2016), and the frequency of investigation), which leads to a dramatic increase in Q^{-1}, particularly above -20°C in pure ice (Kuroiwa, 1964). Here, we attribute pre-melt films developing on grain boundaries as the dominant mechanism for the changes in the values of the elastic properties and wave attenuation as a function of ice temperature. This is a more likely contributor than the dislocation damping mechanisms proposed to dominate at the highest temperatures (Cole et al., 1998; Cole, 1990; Cole and Durell, 1995, 2001), since these samples have not been subject to deformation. This has been observed previously by Spetzler and Anderson (1968) and Kuroiwa (1964) in laboratory resonant bar measurements, and in the field at seismic frequencies (Peters et al., 2012)."

3 Response To RC1: Anonymous Reviewer

- Comment: "The authors estimate averaged elasticity parameters (disregarding mode conversion), but it's really the temperature dependence, particularly for the anelastic properties, that is most useful for studies of real ice sheets. The authors explain that the Q values they found and the temperature dependence of those values exhibit bimodal distributions. I was expecting a plot of temperature dependence for the extensional modes at least, since these are relevant for seismic studies."

Response: We would like to direct the reviewers attention to Figure 6 in the manuscript, where we have included a plot of Q as a function of temperature and frequency for each mode, and identify whether the modes are extensional, torsional, or flexural excitations. Here, the bi-modal distribution in absolute values of Q is clear, as is the greater sensitivity of extensional modes to changes in temperature.

- Comment: "Why weren't measurements made below pre-melt temperatures, since as the text mentions this can cause an interesting mechanical transition? For pure ice this would be around -30 C, only 5 degrees colder than was measured (according to Peters et al. and references). Would it be possible to repeat measurements for other grain sizes or ice compositions, or speculate how grain size (curvature) and impurities affect pre-melting and Q? Perhaps this is planned in future work?"

Response:

The temperature for the onset of pre-melting in ice is dependent upon several conditions, such as impurities, pressure, or even the mode of investigation, and has been proposed to occur at much warmer temperatures. In the classic work of Kuroiwa (1964), a dramatic increase in internal friction of pure ice is observed closer to -15°C in resonant bar experiments, while this change is observed at lower temperatures in natural samples and NaCl-doped ice. In our experiments, we see the most dramatic changes in both our elastic constants and values of Q above -20°C. Arguably, the most drastic change in grain boundary mobility in deforming ice polycrystals occurs at -10°C (Duval et al., 1983), and is often associated with the proliferation of "a widened zone at grain boundaries with a liquid-like structure."

In any case, we were primarily limited by the minimum temperature of our freezer in this case (about -26°C).

- Comment: "The paper explains that an identical ice sample in the same part of the freezer was monitored by thermocouples frozen into its core, as the temperature was slowly increased. How slowly? Is it possible that conduction through the thermocouple leads biased the temperature measurements? The authors might want to discuss this."

Response: The temperature sweeps in these experiments took several hours to complete, and progressed approximately linearly at a rate of 4°C per hour. This has now been included in the text of the manuscript (see page 4, line 28 of the track-changes file). The thermal mass of the two thermocouples used to measure sample temperature is insignificant relative to the large ice sample, and should have very little influence on sample temperature.

- Comment: 4.2 line 20 typo: "...must exist within in an ice body..."

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Response: This has been corrected. Thanks for pointing it out.

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