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The Cryosphere Discussions

Interactive comment on "Critical analysis of the relations between the velocities of elastic waves and effective anisotropy of ice polycrystals" *by* A. Maurel et al.

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Maurel et al. compare two different methods for the calculation of seismic velocities in anisotropic ice, the velocity averaging method (or slowness averaging method) and the effective medium method. As example they calculate the P-, SH-, SV-wave velocities of vertical transversely isotropic (VTI) media for vertical incidence. The velocity averaging method results in different SH- and SV-wave velocity for vertical incidence in VTI media. For vertical incidence in VTI media, both SH- and SV- wave are polarized in the isotropy plane and should therefore be equal. Maurel et al. therefore conclude that the velocity averaging method gives unphysical results. Hence, the effective medium method should be preferred for the calculation of seismic velocities in anisotropic ice.

Finally, they comment on the calculation of velocities following the method by Bennett (1968). Bennett derived seismic velocities for ice cluster (or cone) and small circle girdle fabrics by approximating the slowness surface. The manuscript focuses on an important point, that the velocity averaging method does not lead to correct velocities for anisotropic ice. However, the paper in its current form is difficult to follow, has some technical errors and the structure is partly confusing. Repetitions make it difficult to follow too and a critical discussion is missing. The work presented here is very similar to the paper of Maurel et al (2015). See for example Figure 10 in Maurel et al (2015) that already points out the unphysical result of the velocity averaging method. Further this Figure includes a graph for the Bennett equations, which this paper is missing. The manuscript needs some more work, a solid and more in depth discussion of the results and a better focus on the main subject to be more accessible to the larger audience.

General comments:

Title: To me the title does not really reflect the work that is done in this paper. You do not analyze the relation between velocities and anisotropy. It is rather a critical investigation of different calculation methods for velocities in anisotropic ice. Please consider changing the title so it better reflects the content of the paper.

English: I think the paper and the understandability of the content would highly benefit if the paper would be read and corrected by an English native speaker. Some of the words (especially verbs) used seem not quite appropriate in the context of a scientific paper and many sentences could be a lot shorter and thus better understandable if the structure of the sentence would be reworked. A lot of filling words are used (basically, usual, just . . .) they are unnecessary. I highlighted some of those in the attached PDF.

Terminology: Some of the terms in the paper are not quite correct, or not appropriate. Times of flight: The term used in geophysics and glaciology is traveltime. Vibrations of the waves: It is the polarization direction of the wave. Sound waves in the context of S-waves: shear waves are elastic waves a P-wave is an acoustic wave. Shear velocity:

It is a shear wave velocity. Pitfalls: Why do you not just use the word errors? Pitfalls sounds a bit like slang.

Structure: You are constantly jumping between the notation cijkl and the Voigt notation CIJ. For a reader that is not familiar with that, this makes it very hard to follow. Stick with Voigt notation once it is introduced. I would recommend swapping chapter 2.1 and 2.2. because 2.1 is really the derivation of seismic velocities and it is not especially for the ice polycrystal case. Further you could then introduce the Voigt notation and stick with it, instead of jumping back and forth all the time.

Introduction: The first paragraph has some errors and is not very selective in its choice of references. The introduction needs a paragraph with clearly distinguishing between the two compared methods. It does not become very clear in the introduction so far. The paragraph line 39-48 is very confusing and needs to be rewritten. Some sentences like this would maybe help to follow: 'In this paper we compare to different methods for the calculation of seismic velocities in anisotropic polycrystalline ice, the velocity averaging method (or slowness averaging method; slowness is the inverse of the velocity) and the effective medium method. For the velocity averaging method the seismic velocity is calculated for a single anisotropic crystal. The velocity of the bulk media is then derived by averaging velocities for different crystal orientations. In contrast, for the effective medium method the elasticity tensor for different crystal orientations is averaged resulting in an elasticity tensor for the bulk medium. Form this the seismic velocities are calculated. We will show, that the velocity averaging method has some errors in its fundamental assumption and will lead to unphysical results.'

Variables: Variables MUST be explained where they first appear. This manuscript is full of variables that are not explained at all or pages later. I pointed out a lot of them but maybe not all. Figures: Figures MUST appear in the paper in the order in which they are mentioned in the text. Your order: Fig 1, Fig 3, Fig. 2, Fig. 5, Fig. 4, Fig 6, Fig. 7.

Repetitions: You repeat yourself over and over again. I think that could become significantly better by improving the structure of the paper. For example: You explain three times what a VTI media is. I don't know how often you mention that the velocity averaging method is unphysical. Those things make the paper longer and more difficult to follow.

Repetition of equations: Some equations are shown with very little difference. I do not think it is necessary to show equations 1 and 2 and touch on perturbation theory. You do have equation 11 and for the scope of the paper it would be absolutely sufficient to start off with this equation.

The angle θ: It is very confusing that you use theta as the angle between the vertical axes and the c-axis and for the angle between the wavevektor k and the c-axis. In this context here it is of course the same angle because you only consider wave propagation along e3, but it is not true in the general case. You have to make clear, that due to this special geometry you consider here these two angels are equal. Figure 2 in my opinion is not necessary, but if you decide to keep it needs to include the e3 axis otherwise it is wrong.

The example Zinc: I don't see the point in showing the example of Zinc here. Your target audience of TC are glaciologist. This paper is really about the comparison of two different methods for the calculation of velocities in ice. You do not discuss the Zinc example. There is no need of including it. It would help more if you would really discuss the ice example crytically instead of showing Zinc. If you want to point out, that the discrepancy between velocity averaging and effective medium method can be larger for the P-wave for stronger anisotropic crystals one sentence would be enough, giving the discrepancy in percent for the example zinc.

Comments to Diez and Eisen: You comment on the paper of Diez and Eisen, saying that the velocity averaging method is used and speculating that they see the same S-wave velocity for zero offset because they would average the S-waves. Further you speculate that eq. 12 and 13 are wrong. None of these accusations and speculations are correct. In Diez and Eisen the elasticity tensor is averaged. Very similar to the method you use, the opening angle is derived from the eigenvalues. As such SH- and SV- wave velocity are equal for zero offset. Hence, there is no averaging done following Midday. Further, the definition of the distribution function is different than in your case and equations 12 and 13 are correct. In fact, the effective medium method (as you call it) has not only been shown in Maurel et al, 2015, but before that in Nanthikesan and Sunder, 1994 (Cold Reg. Sci. Technol., 22:149-169) and Diez and Eisen, 2015 (TC, Part1) and calculations have been compared to vertical seismic profiling data in Diez et al., 2015 (TC, Part2). Next to citing your own paper that should probably be part of your introduction.

Discussion: From line 220 it should be a new chapter and this chapter should really be a discussion of the results and explain somehow why the velocity averaging method leads to wrong results. It should also include the results of Bennett and as such be the chapter before the conclusion. Important is also to discuss these variations in the contest of seismic data. Velocities derived from seismic data and sonic logging do have errors. At least in the case of the P-wave, these errors will be larger than the errors made by the velocity averaging method. As such, this method due to its simplicity can very well be used. A critical discussion should follow that this might not be the case for the S-waves. Also could you give percentage values how large the variations are, especially between the result using Midday (red dashed line) and the effective medium method. You cite Gusmeroli (2012) a few times, which is really the paper that uses sonic logging to estimate anisotropy. They use the velocity averaging method. So can there results for the SV-wave still be regarded as correct within the limit of the given errors?

Bennett: The equations given by Bennett are semi-empirical and as such they do not have to follow a rigorous mathematical derivation. The question is if they do represent a good approximation of seismic velocities in anisotropic ice. Like mentioned before

seismic velocities from real measurements do have errors, that might exceed the errors made due to the approximation done in the equations of Bennett. The equations given by Bennett are compact, easy to handle equations, that are, even though they are semi-empirical, very valuable for the application of seismics on glaciers and ice sheets. Just criticizing them does put a wrong light on the value of these equations for glaciology. I think it is nice to reflect the equations here and to point out the empirical and approximate nature of Bennett's equations, but I do not think it is correct to claim they are wrong. The authors discuss seismic anisotropy from a theoretical standpoint. However, for applications empirical and approximate equations are often a good starting point.

Specific comments: Please see attached pdf.

Please also note the supplement to this comment:
http://www.the-cryosphere-discuss.net/tc-2016-23/tc-2016-23-RC1-supplement.pdf

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