

Response to Referee 1

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:

comments from Referees, *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.*

author's response and change, We have changed the title to " Critical investigation of calculation methods for the elastic velocities in anisotropic ice polycrystals".

comments from Referees, *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.

author's response and change, We have tried to improve the english. The modifications suggested in the PDF have been accounted for.

comments from Referees, *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.*

author's response and change, "Time of flight" has been changed to "traveltime", "vibration" to "polarization", "sound speed" by "elastic velocity". The term "pitfall" has been removed.

comments from Referees, *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.*

author's response and change, We have swapped chapters 2.1. and 2.2. We have tried, when possible, to stick with the Voigt notations. Note that this is not always possible; for instance, in Eq. (12) the Christoffel equation involves the effective elasticity tensor; thus we need to define this average tensor.

comments from Referees, *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.*

author's response and change, The introduction has been almost completely rewritten. Notably, the paragraph (line 26 to 39) referring to the en-

semble average calculation has been removed, and sentences have been removed according to the remarks of the referee in the PDF. This is the case for the lines 39-48.

comments from Referees, 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.*

author's response and change, We have indicated the meaning of each variable. This is notably the case for the new Eqs. (1), (7); \hat{c}^{eff} at line 100, and in Eq. (29).

comments from Referees, 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.*

author's response and change, We apologize for this. We have removed 2 figures. The references to the 5 figures in the revised version now appear in the right order.

comments from Referees, 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 dont know how often you mention that the velocity averaging method is unphysical. Those things make the paper longer and more difficult to follow.*

author's response and change, We have accounted for this general remark and the specific remarks in the attached PDF of the referee. Specifically, the VTI structure is explained once and for all in the introduction, line 23. It is also true that "unphysical" was used excessively; in the revised version, we mention that unphysical results can be found using the averaging velocity method. Then, the term "unphysical" is not used anymore associated to this method. We use unphysical again concerning the velocity in a single crystals (not in polycrystal) when referring to the expressions of the velocities derived in Bennett 1968. This is an important disagreement that we have with your analysis of Bennett's results; indeed, these expressions of the velocities in a single crystal cannot be considered as approximated expressions, as the Thomsen's expressions are, our Eq. 29. Thomsen's expressions are correct up to a small parameter being the small degree of anisotropy of the material, and the angular dependance is correct. To the contrary, Bennett's expressions introduce an extra dependence on φ which is wrong.

comments from Referees, 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.*

author's response and change, We removed Eqs. 1 and 2 in the revised

version. We agree that it is sufficient to start with Eq. (11), in the revised version Eq. (1)

comments from Referees, *The angle : 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.*

author's response and change, We removed the Figure 2. However, we stress that the derivation of the velocity in a single crystal using the angle θ is done without loss of generality. This is because θ can be first defined as the angle between \mathbf{k} and $\hat{\mathbf{c}}$ (without reference to any particular system of axes). Next, to make explicit the expression of the elasticity tensor, one has to define a system of axes, and it is possible to choose \mathbf{e}_3 along \mathbf{k} without loss of generality.

comments from Referees, *The example Zinc: I dont 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.*

author's response and change We removed the example of Zinc. The new Figures 4 include Bennett's results rather than the averages which were not discussed.

comments from Referees, *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.

author's response First of all, we indeed made a mistake when we said that Diez and Eisen used velocity averaging method, and we apologize for that. It is absolutely true that an average of the elasticity tensor is proposed. We remove this wrong statement. As a personal comment, we maintain that Eqs. 12-13 in Diez and Eisen, 2015 (TC, Part1), as they are written, are at least confusing: it is suggested that the average of the elasticity tensor can be performed by decomposing the average on the solid angle $d\Omega$ in three dimensions into successive averages in specific planes. If this is what is done in Diez and Eisen 2015, this is wrong. We assume that this is not the case, and as previously said, we removed this paragraph.

We agree with the fact that effective media theories were used much before Maurel et al 2015 (see reference to Keller and Karal in the 60s). When one refers to the anisotropy of a polycrystal, one refers to the effective anisotropy of an effective medium, and this fact has been known for many time but for some reason, it is not used when considering wave propagation in polycrystals. To be precise, it is Keller and Karal in the 60s who showed that the same average can be done in dynamics (on the wave equation) as in static.

author's change We remove the paragraph, lines 237-242.

comments from Referees, 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 context 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 Middy (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?*

author's response and changes The velocity averaging method leads to wrong results because unphysical results are wrong; this is already discussed in the chapter 3. In the case of Bennett's predictions, the discussion is different. Bennett started from modified expressions of the velocities in single ice crystal which are erroneous because he anticipated the velocity averaging for VTI structures. Thus he attributed fictitious weights thought to get a unique shear velocity after velocity average. Doing so, he obtained by construction a shear wave velocity close to the harmonic mean of the two unphysical velocities (which are obtained starting from the correct velocities in single ice crystal). The idea is clever and quite intuitive for VTI textures, but the unique reason why we

can say that it is clever is precisely that his expressions have been validated in practice. As such, it is not a predictive approach. Notably, it cannot be extended to other textures.

Next, you are asking us to compare the error due to the use of one of these wrong models with the uncertainties in the measurements of the velocity. Estimating the error in a model requires to have a reference model. Thus, we cannot do that except if we assume that the effective model is correct, and it is not the subject of the present paper to demonstrate its validity (this will be done by comparison with well controlled laboratory experiments). The subject of our paper is one step before this validation, and we think that it would be confusing to mix a discussion on the validity of existing models, from a theoretical point of view, and a discussion on the error due to the model used compared to the uncertainties in the measurements (see the following point).

We have rewritten the abstract, the introduction and the conclusion to better stress the goal of the present paper. The section devoted to the Bennett's calculations have been revised and Bennett's results have been reported together with the results from the velocity averaging method and from the effective medium theory.

comments from Referees, 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 Bennetts 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.*

author's response and changes As a first comment: In the supplementary PDF file, you mention that our sentence "Bennett did not publish his calculations. They can be found in his thesis but for the sake of completeness, we report below the main steps of these calculations." was insulting. It is a strange statement since our sentence was factual: a thesis is less easy to get than a published article. Thus, our intention was not to be insulting (nevertheless we suppress the sentence). We have the same feeling concerning your present comment. Highlighting an error in the calculations of a colleague cannot be considered in the scientific community as an insult or a reproach.

Next, Bennett's expressions are not approximate expressions but *ad hoc*

expressions (note also that these expressions are not more compact as the ones coming from the theory of effective media). Our goal is not to criticize Bennett's expressions for application to VTI structures. Nevertheless, they start from modified expressions of the velocities in single crystal which are not reliable as starting point for deriving averaged velocities for other textures; notably, they are factually wrong. We have modified the text in order to better explain this fact and the difficulties that his approach would present if one considers other textures than VTI textures. We have also reported in the new Figs. 4 the results coming from Bennett's expressions.

Finally, it is true that we discuss polycrystal ice anisotropy from a theoretical standpoint. This is because the inversion from the measured velocities to the ice anisotropy will require an accurate model. Thus, we analyze theoretical models based on two different methods to discriminate which one is the best candidate. The discussion on the uncertainties in the sonic measurements, or on the other sources of uncertainties, is a different discussion which makes obviously sense. Both discussions are meaningful and they can be conducted separately, since they are not related at all. Knowing the degree of precision which can be reached nowadays does not make acceptable an erroneous model. To the opposite, once one or several approximate (thus acceptable) models will be identified, the confrontation between the error due to the approximate model and the uncertainties due to the measurements will be necessary.

Response to Referee 2

Maurel et al. present a comparison of two averaging methods used to determine seismic velocities in anisotropic media, outlining the derivation of velocity-averaging and elasticity-tensor averaging for single grain and polycrystal ice exhibiting typical anisotropic fabric. The authors demonstrate the shortcomings of the velocity-averaging method which result in erroneous P-wave velocity estimates and unphysical S-wave velocities with subsequent erroneous average S-wave velocities. By contrast, the elasticity-tensor averaging method is shown to be robust in the cases presented. The authors go on to outline the shortcomings of the Bennett (1968) method. This paper is an extension of Maurel et al. (2015), with reproduction here of a number of derivations and equations reported previously. Maurel et al. (2015) includes a section 5(b) Comparison with previous work which is essentially a digest of, or prelude to, this paper, commenting that the differences will be discussed in detail in future work. At the end of this section they essentially report the findings presented here, but arguably in a more succinct manner: The agreement is excellent [referring to Bennett], although less good for the S-wave than for the P-wave, and this will be analysed in more detail in forthcoming work. The resulting agreement on the velocities is 0.07% for the P-wave and 0.7% for the S-wave, without any adjustment (figure 10) (red and black curves). For completeness, we also report in figure 10 the results obtained from slowness averaging as used in [24] [referring to Gusmeroli or slowness averaging as a method], omitting the T-average (equations (5.5)) (green curves). This latter case leads to two different S-wave velocities, which is unphysical as the wave propagates along the symmetry axis. Incidentally, there is a slightly more notable disagreement with Bennett [21], with 0.9% for both the P- and the S-wave velocities (when compared with the highest S-velocity).

General comments:

comments from Referees, *Although the result presented here are important, in that they highlight potential flaws in methodology, the significance in terms of the scale of errors introduced is not obvious: 1. The errors introduced using the velocity-averaging method for P-waves are very different to those of S-waves. As such, the significance to different experiments will vary.*

author's response Yes, you are right, for ice, the significance to different experiments will vary (if the experiments involve P- or S- wave propagation).

comments from Referees, *2. In general, seismic anisotropy is presented as a percentage of velocity which is more tangible and indicative of its significance when applied to data. This is not the case here. Presentation of results as percentages would allow readers to determine their significance much more readily and put the errors in context. At present, it is not possible to ascertain the scale*

of these errors when compared to observational errors, which are currently significant in seismic studies of in-situ ice. Percentages were used in Maurel et al (2015), as highlighted above. 3. It would appear from the plots presented that the errors introduced by the velocity- averaging method would be of the order of 1% for P-wave velocities and 1% for S-waves (again, as highlighted above). Errors at this level may be acceptable when comparing to field observations, although future studies will of course need to include this uncertainty.

author's response Indeed, in Maurel et al (2015), percentage were used. Nevertheless, the objective in this paper was to compare several models, and not to inspect their validity. This is the objective of the present paper, and as such, the focus is different. Entering in a discussion on the consequences (in percentage) of using a model which leads to wrong results would be confusing. What would be the message then ? Inspecting the validity of the velocity average method reveals that it conduces to non acceptable results (two different S- velocities). This would be different if two models, with different hypothesis, led to acceptable results; in this case, it would be necessary indeed to inspect whether or not the differences between the two results overcome the uncertainties in the measurements.

comments from Referees, 4. *The inclusion of the example of zinc is particularly confusing and of no interest to the general glaciological community. The authors include this example to emphasise the potential errors introduced but its inclusion over-complicates what is already a fairly inaccessible piece of work. More useful would be to present the errors introduced for all the likely anisotropic fabrics of ice (of which two are already presented, and all of which are already outlined in Maurel et al (2015)).*

author's response and changes, This point is related to the previous ones. We have removed the example of the zinc. We also added the velocities coming from the Bennett's calculations. We have changed the text in order to stress that the relative agreement between the observed values does not support the idea that erroneous models can be used. It only explains why the error in using such models for simple textures (cluster and girdles) has not been detected. Next, inspecting other fabrics would be useless since one cannot cover all the possible textures, thus we cannot guaranty that the error in using an erroneous model will be always not too important. Thus, we choose a texture which allows to demonstrate that the models are erroneous, with no need for comparison with experiments or with a reference model (a reference model would require to demonstrate that it is the best one, which is not possible in general).

comments from Referees, 5. *There is no glaciological context with regards the stress regime responsible for the fabrics presented, or why anisotropic fabric in ice is of interest etc. Again, this reduces the target audience. The manuscript is poorly written, difficult to follow, poorly structured and with an un- scientific style in places. As such, the main findings of the work are not clear and will be overlooked by the vast majority of readers. The structure needs attention and*

section headings need to be more specific and descriptive to improve the flow of the manuscript. Method and application should be in separate sections. The style, grammar and vocabulary also need attention: the paper is currently below the standard where a reviewer can be expected to correct all of the grammar and style issues.

author's response , There are two different points in this comment. First, the glaciological context with regard the stress regimes responsible for the fabrics presented. This is clearly outside the scope of the present paper, since our conclusions hold for other polycrystals, as soon as sonic logging measurements are concerned. The second point is : Why the anisotropic fabric of ice is of interest is another question, and clearly pertinent here. In fact, it motivated the present study. Because sonic loggers start to be used in the context of glaciology, and in this context, high accuracy is required because of the weak anisotropy of single ice crystal, it is particularly important to use accurate models. At this stage, we cannot claim that the presented model is sufficiently accurate (this needs comparison with results of well controlled experiments). We stress that the average velocity model and Bennett's results are erroneous and as such, it would be better not to use them. One could make an exception for the case of the clustered textures, because it seems the error to the erroneous model falls within the uncertainties of the measurements, but it is not so helpful. Indeed, in this case, the expressions found by the effective medium theory are also easy to use.

author's changes, We have modified the text and more specifically the introduction and the conclusion to make clearer the goal in the presented study (analysis of several models used to invert the elastic velocities to get the anisotropy of ice polycrystal); also the motivation coming from the use of sonic logging measurements in the context of glaciology. We have tried to improve the english.

comments from Referees, *Recommendation This is a useful and timely piece of work and adds to the growing body of papers investigating anisotropy in ice, highlighting pitfalls of a previously-applied methodology and assumptions made therein. The discrepancies introduced by using the velocity- averaging method are an important finding which must be heeded by future workers in the field.*

As such, the findings are suitable for publication in The Cryosphere. However, I have two main concerns and suggested improvements: 1. The significance of the findings to the glaciological community is not well presented, and possibly of only minor significance when compared to observational uncertainties, and as a result they may be overlooked. The glaciological context and significance needs more discussion.

author's response and changes, As previously said, we have tried to make clearer the goal of the present study and the significance of the findings to the glaciology community. With regard to the sonic logging measurements used recently in boreholes, accurate models have to be developed, able to describe the simple or more complex textures of ice polycrystals. A first step is to avoid erroneous models.

comments from Referees, 2. *One can regard this paper as an application of Maurel et al (2015). I would therefore recommend two strategies to improve the manuscript. If this paper is to be published in The Cryosphere this is critical to ensure accessibility to the likely readership: Option 1: Significantly more use could be made of referring to Maurel et al. (2015) with the removal of some of the repeated text and functions to improve the readability of this paper, e.g., parts of Section 2.1 and the Introduction. Alternatively, some of the content could be moved to an appendix or supplementary material and reported in more detail, as per Maurel et al (2015). At present, this manuscript is a poorly structured version of Maurel et al. (2015), which uses a much more coherent and well-structured presentation style. Option 2: This paper could be submitted as a Brief Communication, highlighting the results and discrepancies of the different methods and removing a significant part of the introduction and methodology by referring to Maurel et al (2015).*

author's response and changes, We judge that the option 1 is better. A brief communication removing the specific detailed calculations will not help the accessibility to the likely readership. We are not presenting the discrepancies between different methods (in which case we agree that removing the calculations would be incidental). We are explaining why two of them are erroneous and this requires to be specific in the description of these methods. With regard to the Option 1, we have significantly re-written the paper (notably by removing repeated text).

comments from Referees,

Specific Comments The manuscript is full of grammatical and linguistic errors, beyond what I regard as reasonable for a reviewer to highlight, and is in need of significant proof reading and editing prior to re-submission. Title: The title as it stands does not describe the manuscript. The paper is a comparison and evaluation of averaging methods. Abstract: The abstract does not fully describe the manuscript, only outlining the aims ??of the work and not the method, results or conclusions. Sections headings should be more concise and descriptive Section 2 As per RC1, this section could be re-structured to improve readability. Consider moving the first part of Section 3 (the two boxed sections, or perhaps all of 3.1) here to create a self-contained section of methodology followed by application only in Section 3. Section 3.2 This should become section 4 to improve readability. Section 4 The section discussing Bennett (1968) again builds on Maurel et al (2015). However, in the previous paper the errors introduced were described in terms of percentages. However, in this manuscript this section is poorly structured and difficult to follow. Figures 5 and 6 can be merged if zinc is dropped, and similar plots with percentage anisotropy included. The sections discussing previous work (Diez, Bennett, Gusmeroli) are poorly structured and lack focus or specifics (such as section or equation numbers in the previous work). The reader will therefore struggle to fully understand the issues with the previous work. Conclusions: As with the abstract, the conclusions do

not encapsulate the full body of work. Similarly, a separate discussion section is required, most likely a re-working of existing text will suffice.

author's response and changes, We have rewritten the abstract, the introduction and the conclusion.

Notably, the introduction has been changed in order to stress the difference in the focus of the present paper with respect to Maurel (2015). The section discussing Bennett has been shortened and we think that it better stresses the method used in Bennett 1968. The new Figs. (removing the example of Zinc) includes Bennett's predictions, rather than the averages which were not discussed