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 aver- aging 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 aver- aging 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 ensemble 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); $\hat{\mathbf{c}}^{\text{eff}}$ 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 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?*
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.