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Discussions

Interactive comment on "Thin-layer effects in glaciological seismic amplitude-versus-angle (AVA) analysis: implications for characterising a subglacial till unit, Russell Glacier, West Greenland" *by* A. D. Booth et al.

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GENERAL COMMENTS The overall quality of this paper is good. It reports a very thorough and detailed piece of work. It addresses an aspect of seismic interpretation that has seen little previous attention in glaciology and it makes substantial progress. The primary conclusion that thin-bed effects should also be borne in mind when interpreting AVA data, has wide relevance within this field of research. Overall this is a good piece of research which will be well suited to publication in The Cryosphere.

SPECIFIC COMMENTS

The overall justification for the work is an eventual improvement in the way the subglacial basal boundary is incorporated into predictive models of ice dynamics. The paper aims to achieve this by the development of new approaches to interpreting seismic AVA data; by including previously ignored thin-bed effects, its ambition is to lead to more accurate and precise interpretations of the material beneath the ice.

This paper considers the effects of thin layers on the analysis and interpretation of subglacial conditions from seismic AVA data. Seismically, a thin-bed is often defined as having a thickness of a quarter-wavelength, or less. The effects of the superposition of a reflection from the top of a thin layer with others arriving slightly later from its base for example, determine the resulting composite reflection. Under many circumstances, these effects need to be considered during the quantitative interpretation of material properties from this composite reflection. This paper concentrates on amplitude-versus-angle (AVA) data; this is also commonly known in seismology as amplitude-versus-offset (AVO), the authors here use the term AVA to emphasis the dependence on incident angle, rather than on simple lateral offset from the seismic source. The paper's main approach is to consider the theoretical seismic AVA response to a thin subglacial layer. This is achieved by an increasingly rigorous series of methods; first a simple travel-time analysis to show which arrivals could contribute, second by determining which of these are expected to have amplitudes sufficient to be significant, and third to consider full-waveform forward modelling including the effect of anelastic attenuation of the seismic energy. Finally, the implications of the theoretical models for the interpretation of some real seismic AVA data is included using two examples, one is new seismic data from a Greenland glacier, the other proposes an alternative interpretation for a previously published study.

Many (although not all) of my comments that follow are negative but I emphasise that overall I think this paper presents a good, substantial and relevant piece of work which deserves publishing after some revision.

Attention to detail. The paper needs a very thorough check for typos, citations, references, parameters, figure call-outs and errors throughout. All details in the paper need checking rigorously. Quantities used from cited papers (eg Peters et al 2007, 2008, Vaughan et al 2003) are sometimes not used or inappropriate values apparently are chosen; citations and references contain mistakes and are sometimes not the appropriate ones; varying symbols used (e.g. σ and υ for Poisson's ratio) or perhaps wrong ones (e.g. should $\leq$ on page 770, line 15 and 772, line 6 actually be $\geq$?) need correcting; figures need checking for inconsistencies etc (e.g. green dots/dashes in fig 4, 2m, QL). I admit this will need a lot of work but it is needed.

Analysis details The paper presents the results of a considerable amount of work. The writing is mostly clear and concise but in a number of places it is probably a bit too brief. This is particularly so where a number of analysis steps are being considered. Four examples are the determination of mean quality factor for the ice column, the zero-offset reflection coefficient for the Greenland data, decomposition of effective reflectivity and measuring the amplitudes of the modelled AVA responses. None of these are trivial but few details are given. These deserve more comprehensive explanation, evaluation and consideration.

Wavelet amplitudes I think the picking of wavelet amplitudes perhaps needs further consideration and/or discussion. As described in section 3.4, the amplitudes of the synthetic wavelets are picked at the maximum point of the first "identifiable" half cycle. The locations of these points are shown in Fig 4 and these amplitude values are then used to derive the AVA responses shown in Fig 5. In theory the direction of the first break at normal incidence should not be changed, irrespective of the thickness or properties of the thin layer. I am not aware of a mechanism that can alter this requirement. What is possible, e.g. for an ultra-thin layer with weak R1(0) and strong R2(0), is that the first break becomes so weak that it is no longer identifiable. (With real data, this then means its amplitude falls below the S/N ratio.) Then the first "identifiable" half cycle becomes that dominated by the second, opposite polarity half-cycle, rather than

that associated with the true first break.

Assuming that the full-waveform forward modelling used an impedance in the dilatant-till thin layer less than that in the ice (which would be most appropriate and does appear to be the case, although there is some confusion e.g. page 773, line 5 and perhaps also the "reference" PP in Fig 6c), then in the perfect situation the normal-incidence first break should always be reversed and R1(0) <0. For the thinnest layers in Fig 5 a,b, this is not the case, presumably because the first half cycle was too weak to be identifiable; presumably therefore there must be some judgement used to identify the first half cycle. If this is correct, then I think it needs further consideration. Is the amplitude of this first identifiable half-cycle the most appropriate, or should a broader part of the composite wavelet be used?

Introduction The overall glaciological motivation for the work is dealt with in the first three lines (including citations). This does seem unnecessarily brief.

Page 764 L 1-14: Use of the best-fit Shuey terms is a good way of visualising the results and getting the implications over to the reader. The limitations are well recognised.

Page 765 L 7: Quantify "a small number" i.e. 2-3 m? L 27: Explain "progressive extension of the wavelet period" resulting from finite Q.

Page 766 L 16: What is meant by "extend"? Is "iterate" more appropriate?

Page 767 L 8: I think the fact that PPPP arrives at a time lag just outside that required to interfere with PP is actually dictated by the input model parameters, hence it is not really supporting the theoretical assumption, and it is actually dictated by the theory used. L 14 and elsewhere: The term "effective reflectivity" seems to be used confusingly. As described here (Line 14) it seems appropriate – i.e. the result of summing all the arrivals together to determine the final composite wavelet. But labelling the ordinate axis in Fig 3b as "effective reflection coefficient" seems inappropriate – what's plotted there is the actual reflection coefficient for each individual component. L 27: The destructive interference resulting from polarity reversals for the shear wave reflections is a good point. However, surely this is also the case for the P-waves too? Widess, 1973, Fig 2c illustrates this clearly; it is a general principle, not restricted to the shear waves.

Page 768 L 1-3: This summary, clarifying the main components that contribute to the AVA thin-layer effects is a good point to make. L 16-20: Need to note that these parameters are for the ice. L 15-27: I think this approach to the quality factor is a good one. In the absence of any real numbers to use, it is justifiable and works well. I think using the sea-bed sediments as an analogue is sound under the circumstances. There are few analogous environments to subglacial conditions, but the saturated, high overburden, high porosity, low effective pressure conditions at the deep ocean floors are probably the best we have.

Page 769. I think Figure 5 is far more illustrative of the points being made here than the one cited (Fig 4).

Page 770 L 3-6: My comments on attention to detail and wavelet amplitudes are particularly relevant here. It is not clear why a very low impedance contrast has been specified. It also bears no resemblance to the cited Peters et al 2008 paper, which gives an explicit impedance contrast (i.e. not a range) between ice and dilatant till of 0.4x10ˆ6, not 0.08x10ˆ6. (I also note that Peters et al 2007 is also a relevant citation and that gives 0.6x10ˆ6. Atre & Bentley 1993 (J Glac, 36, p507-514) is also a relevant reference for quantitative estimates of impedance for subglacial dilated till.)

Note: my comments on wavelet amplitudes and Poisson's ratio are particularly relevant throughout sections 4 and 5.

Page 771 L 18-20: It is not clear how this statement arises. I think it is the end goal of the paper, but at this stage it has not been shown. So far, some thin-bed AVA responses have been determined, but it hasn't yet been shown that these could be misinterpreted as single interface responses. Or is it simply assumed that could be the case, and

this statement is presented just as the introduction to this section? L 26-onwards: It might be instructive to see the fit analyses here. This is where Zoeppritz for a single interface is being matched to curves derived from Zoeppritz for a thin-layer. How close this match can be made could be a good indication of how likely it is that real AVA data could be misinterpreted in terms of a single interface.

Page 772, L22 – page 773. I admit I haven't yet been able to fully grasp the physical basis of this section, but it does produce some interesting results. This section would benefit from some clarification. Intuitively, at normal-incidence at least, it is reasonable to expect the effect of the deeper reflection to decrease as the layer attenuation increases and also for the effective reflection coefficient to converge with some simple combination of the two actual values as the layer becomes more ultra-thin. It could be my own ignorance, but I cannot see the basis of the equation (eqn. 4) for decomposing Rapp(0) into its components. (Unless it is simply substituting and rearranging transmission and reflection coefficients and ignoring layer travel time?) Perhaps this just needs a reference to a fundamental equation? The quoted dilatant till impedance value (3.9x10ˆ6) is too high. Is this a typo or was it actually used?

Section 4.2 I agree that Poisson's ratio on the low Q models matches the dilatant till better than the others. However, looking throughout Table 3 it appears that σapp is actually rather insensitive to any parameter variations. This makes me wonder if this good fit is simply coincidental. Is there any way of assessing if it is significant or not? I also note that, whilst not a perfect fit, Zapp corresponds much more closely to dilatant till, than lodged. I agree that the increasingly small values with increasing thickness are intriguing. It might be important to understand why. Is it known that the physics of Q-based reflectivity is incorporated in the SKB2 software? If not, then the suggestion that this is a possible cause would probably not be justified. The need for better data on attenuation of subglacial material to allow further progress is well made. I also note the relevant work in the cited Nolan & Echelmeyer paper, as well as in Nolan 1997 (PhD thesis, University of Alaska, Fairbanks) and the Bourbie references therein.

Page 775 L 1-2: Has the Qp for ice been measured in this study? If so, it is a significant result that deserves full description. Neither of the cited references determine a mean Qp for the ice column. (They also use direct/primary spectral ratios, not primary/multiple.) L21 onwards: Along with Fig 6, this illustrates the main thrust of the paper; if the best-fit to observations is also used to determine Poisson's ratio, as well as acoustic impedance, this can indicate the presence of thin-bed effects which wouldn't be apparent from impedance alone. Although as noted above, I'm not sure that the derived value of Poisson's ratio is shown to be diagnostic, the fact that it simply doesn't agree with the impedance is a good indication that a single-interface interpretation isn't sufficient.

Page 776 L 4-16: I disagree that there is a striking resemblance between the real-data AVA response and those of the low attenuation ultra-thin layers. Plotting it on e.g. Fig 5a shows that the real data zero-offset reflection coefficient is much greater ($\sim$0.1 rather than $\sim$0.05) and the gradient with increasing angle is much steeper. I therefore think that, without presenting alternative models which match the real-data more quantitatively, the analysis proposed in the rest of this paragraph probably isn't justified.

Page 777 L 2-3: This is a good point and highlights the main thrust of the paper, that thin-layer AVA effects need at least to be considered during interpretations of real data. L20-onwards: The re-evaluation of Peters 2008 is not really justified. The authors are proposing that the stiff-till bed in that paper could alternatively be interpreted as stiff-till with an overlying thin layer of soft till. The authors present nothing in this paper that compares diagnostically with the AVA data and models presented by Peters et al for the stiff till areas, namely negative normal incidence reflection coefficient and negative gradient. An alternative thin-bed AVA response, showing these characteristics arising from a plausible thin-layer model would probably be required to justify questioning the original interpretation. However, I do agree with the final sentence that improved reconciliations between geophysical interpretations and field observations is a worthwhile

goal.

Table 2 What are the sources for the chosen parameters? The derivation of Qice doesn't seem to be given in Section 5 as claimed. Vp and density for dilatant till (and perhaps lodged till as well) seem too high.

Table 3 I don't think the term "error" is appropriate here (nor in many similar occasions in the paper). What are given are differences between models, and numbers derived from models (not observations). These are excellent for illustrating the behaviour of the models but are not really an error. Qualitatively, this table is very useful, even if it has limitations quantitatively. For acoustic impedance, the low attenuation columns with the thinnest layers "look" much like the deeper lodged till; but as the layer thickens you begin to see its influence indicated in the impedance. With low Q, the attenuation is so great that you hardly see the effect of the deeper reflection at all. The Poisson's ratio estimates appear to indicate just how insensitive that response is to variations. It is useful as an indicator that single-interface interpretation is not sufficient, but not a reliable indicator of what the true Poisson's ratios might be.

Figure 4. Grey band in the control column presumably isn't required? I don't understand the ordinate axis label for the synthetic seismograms. In its simplest case, shouldn't the control panel in a) (ice overlying low-attenuation dilatant till half-space, nmo corrected) show no remaining moveout with offset?

Figure 6 I am confused by some of this figure. The amplitude of the primary reflection in a) quite clearly seems to decrease with offset, yet the resulting reflection coefficient data points in b) increase. The AVA model fit also looks particularly poor. Is it constrained by the normal-incidence reflectivity determined from primary/multiple ratios (I presume this is what is meant by reference to King et al 2003, although the data and analysis aren't presented)? If so, is that a realistic restriction (what are the errors on it)? Could a better fit be found by ignoring or relaxing it (if the error is high), or allowing a fit more like the hd=1.5 and 2.0 m responses in Fig 5a,b, i.e. where reflectivity decreases over the first $\sim 10°$?

TECHNICAL COMMENTS A few technical corrections have been included above. However, in general I thought there were many throughout the paper and I have not identified them all. I think the authors need to work through the paper thoroughly to identify and correct them.

Interactive comment on The Cryosphere Discuss., 6, 759, 2012.