<u>Responses to comments from Andy Smith (RC C269)</u>

Andy Smith: *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.*

Response: We apologise for the lack of thorough proof-reading in the paper, and will make all necessary changes in the revised manuscript. We will verify all references, and will use the *s* annotation throughout the manuscript for Poisson's ratio.

Andy Smith: *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.*

Response: Our main goal in this paper was to highlight the problematic impacts of thin-layer AVA effects rather than describing related processing steps that have been established elsewhere. We therefore realise that some of the concepts in the paper are only described in brief such that they do not become its 'take-home' message. However, we will give more detail about these in a revised manuscript. We expand on these points in the related, forthcoming, issues.

Andy Smith: *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.*

Response: Yes, the intrinsic reflectivity at the bounding interfaces of the thin layer is never changed, by the character of the composite (i.e., whether it has a positive or negative first break, and whether it is dominated by the reflectivity of the upper or lower interface) is influenced both by reflectivity contrasts and the thickness of the thin-layer. As layer thickness approaches the quarter-wavelength criteria for resolution, we see in Figure 4 that the first half-cycles of the composite response are very similar to that of the 'control response', where the underlying half-space is absent. However, for ultra-thin layers with closely-spaced arrivals, the character of the first half-cycles are distorted.

For our model set up, we take the point that our first half-cycles have a low-amplitude and probably fall below some critical S/N ratio in real data. We will therefore base observations in the resubmission on picks made for the second half-cycle, which always has higher S/N ratio but retains the character of the composite (as shown in an initial investigation that we have now performed).

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

Response: Here we also relied on references to establish the glaciological motivation: we will expand this in a redraft.

Andy Smith: *P765 L7: Quantify "a small number" i.e. 2-3 m? L 27: Explain "progressive extension of the wavelet period" resulting from finite Q.*

Response: We'll add in that quantification – yes, 2-3 is appropriate. Finite *Q* causes the seismic waveform to change with propagation as higher frequencies are attenuated. The longer the propagation distance in a finite-*Q* layer, the greater the extension to wavelet period. Hence, as offset increases, a wavelet spends longer in the finite-*Q* layer and hence we observe a progressive extension to wavelet period as the offset in the CMP gather increases.

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

Response: Maybe simply saying 'increase' is more appropriate. We'll correct this.

Andy Smith: *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.*

Response: Yes, that's probably true (and is related to Olaf Eisen's comment regarding resolution vs. interference): it's as much about the waveform we actually use, as it is theoretical considerations of interference. We do emphasise that this is for a 150 Hz pulse propagating at the speed assigned to the thin layer, but we will state more explicitly that it also depends on the waveform that we use and how many cycles are associated with it. Addressing this issue should also go some way to handling Olaf's concerns.

Andy Smith: *P767 L14 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.*

Response: We're confident that we're using the term 'effective reflectivity' properly, particularly in the context of Fig 3b. Effective reflectivity is the property that is measured from seismic data, where the transmission losses from other interfaces are not taken into account. For a reflection from a single interface (i.e., that described by ***PP***), reflectivity and effective reflectivity are identical; thereafter, all reflectivity is 'effective' until losses across overlying interfaces are compensated. Fig 3b therefore shows the reflectivity that is measured without applying and transmission loss correction. The apparent reflectivity of the composite is therefore derived from the summation of effective reflectivities, rather than the individual reflectivity of each interface. We will adjust the text to introduce the term 'effective reflectivity" more clearly in the rewrite.

Andy Smith: *P767 L27: 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.*

Response: We agree entirely with this statement, and did not intend to imply that destructive interference would be S-wave specific. Instead, we wanted to emphasise that the process is more effective for S-waves in our model cases since the reflectivity of the S-wave multiples is stronger than for P-waves; however, we acknowledge that P-wave multiples will still interfere destructively, albeit to a lesser degree, and will modify the text accordingly.

Andy Smith: *P768 L 16-20: Need to note that these parameters are for the ice.*

Response: We'll see to this.

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

Response: We will therefore introduce Figure 5 at this stage of the manuscript.

Andy Smith: *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.)*

Response: We're not sure how this slipped through the net – a combination of typos and a need for more thorough checking... We will ensure this section is consistent in the resubmission.

Andy Smith: *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?*

Response: This statement is intended to introduce the next section, namely *what's the effect of interpreting the composite thin-layer response as a single reflective horizon?* We will rewrite this sentence such that it does not imply previous analyses in the paper.

Andy Smith: *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.*

Response: The best 'domain' to describe such a fit is with the Shuey linearisation of Zoeppritz equation: despite the limited suitability of this linearisation for glaciological situations, the better the linear fit to a given set of AVA observations, the more closely those observations define a plausible AVA curve. As such, this fit analysis is already performed, and is represented in the uncertainty bars on Figure 5d. There are large uncertainties in *A* and *B* for the 'mid-thickness' layers, given their odd-looking AVA curve (decreasing up to 10 degrees, then increasing again), suggesting that these curves would never be misdiagnosed; however, there are very small uncertainties in both thinner and thicker layers, suggesting that these composite curves do resemble plausible single-layer AVA responses and, as such, the potential for misdiagnosis is high.

This is an interesting use of the AVA cross-plot, and one that we hadn't fully explained. We will make sure it is better-explained in the revised manuscript.

Andy Smith: *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?*

Response: This approach does ignore the effect of travel-time through the layer, and treats the observed arrival as if it was a reflection from a single interface, having composite properties of the series of layers. The equation is therefore an expression for the summed reflectivity of the composite interface: the reflectivity of the upper interface plus the effective reflectivity of the lower interface. In this sense, it is just a rearrangement of the zero-offset reflection coefficient equation. We'll make this clearer in the resubmission. The quoted dilatant till value *is* a typing error, and we used the value specified in Table 2.

Andy Smith: *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* σ_{app} *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, Z_{app} 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.*

Response: Poisson's ratio does seem to suggest dilatant till in all cases, although we've shown in Figure 1 that variations in the quantity do significantly alter the output AVA curve and the position of points on the AVA cross-plot. When σ and Z are interpreted together (at least for the infinite and high-Q models), we see that Poisson's ratio for the dilatant till is best-represented where there is also the best match to acoustic impedance – specifically, for the thickest-layer models where the reflections are almost resolvable. Consequently, we don't think that that the good fit is coincidental.

For low-Q models, yes, acoustic impedance is always more representative of dilatant till since the reflection from the deeper interface is greatly attenuated and therefore contributes less to the composite. We feel that the stronger negative reflectivity is related to contrasts in Q although, as explained in the next response, we can only comment at this stage on the qualitative rather than quantitative impacts of this.

Andy Smith: *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.*

Response: Q-based reflectivity is included into SKB2 (we modelled layers with no acoustic impedance contrast, but contrasts in Q, and these produced reflections), but whether those outputs concur quantitatively with recent descriptions of it is still unclear. It's likely in SKB2 that a specified velocity is applied to the chosen wavelet frequency, so a transition into a strongly dispersive material gives rise to significant velocity contrasts even if a nominal impedance contrast is absent. Therefore, SKB2 includes the equivalent phenomena as described by Quintal and Morozov, but we have yet to confirm that this is quantitatively equivalent. Consequently, we do not conduct more quantitative analysis on the low-Q models.

Andy Smith: *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.*

Response: Thanks for these extra references – we will check and include them.

Andy Smith: *Page 775 L 1-2: Has the* Q_p *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* Q_p *for the ice column. (They also use direct/primary spectral ratios, not primary/multiple.)*

Response: Q_P was measured as part of this study. We didn't make more of it in the text since our approach uses established methods (i.e., the spectral ratio – which was the specific 'target' of the references, rather than the actual wavelets used) and it is not directly related to thin-layer problems. We could expand somewhat on the spectral ratio method, but this has now been established for some time. We could instead discuss the benefits of measuring Q_P for the whole ice column? By comparing primary and multiple wavelets, we have two events that have traced more-or-less the exact same travel path; the wavelets involved in a direct/primary spectral ratio calculation have experienced completely different parts of the ice column. Consequently, the Q_P value output from a primary/multiple spectral ratio is the most appropriate description of the bulk attenuation suffered through the whole ice thickness.

Andy Smith: *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.*

Response: Given the observations in Figure 1, we feel that if both the thin-layer and the underlying material had low Poisson's ratios, the composite AVA curve would certainly be changed hence the analysis is not entirely insensitive to variations in the quantity. However, our main point is indeed that the incompatibility of the two derivative quantities is indicative of a breakdown of a single-layer assumption.

Andy Smith: *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 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.*

Response: We think this is a problem of terminology, as we meant to imply that the model and real AVA responses have qualitatively the same character, and maybe describing it as a 'striking' similarity was maybe overdoing it somewhat. We instead intended to draw attention to the fact that the characteristics of the real AVA response had been observed in the simulated one, and only by invoking a thin-layer argument can positive reflection coefficient and AVA gradients be reconciled. Whilst our models do not replicate the same impedance contrasts as eventually observed in the real data, we still think it is worth applying observations made for synthetic data to the real data, even if it serves as an illustration of how reflectivity can be decomposed.

Andy Smith: *Page 777 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.*

Response: We certainly acknowledge that we don't have the modelling capacity to provide a quantitative reinterpretation of the Peters et al data. However, our reinterpretation concerns the soft-till areas, rather than the stiff-till that is mentioned in the comment. The soft-till AVA response (Figure 3B of the Peters paper) features a negative reflection coefficient and a *positive* gradient, which we do observe for our low-Q models and, consequently, Peters' observed AVA response certainly falls into the range of composite responses that we model.

We cannot (and do not) say the same for the stiff-till areas (Figure 3A) and it is possible – at least considering the range of models that we present – that these are indeed single-layer responses. However, it is possible that the soft-till areas are closely-underlain by that stiff-till and the observed AVA curve in Figure 3B is a composite thin-layer response (variations in which reflect the thickness of a laterally variable soft till thin-layer). Although this geometry may be more consistent with recent thoughts on thin till layers, we fully acknowledge that arriving at the true geometry requires more extensive modelling and perhaps a reinterpretation of the seismic data – and it certainly wasn't our intention to suggest that our reinterpretation is the correct one.

We agree with Olaf Eisen that the suggestion of an alternate interpretation is nonetheless valuable (but stop short of his recommendation that the discussion is allocated its own subsection), and we feel strongly that this discussion should remain in the paper. We will, however, emphasise that we are just suggesting the alternative possibility of a different reflector geometry.

Andy Smith: *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.*

Response: The parameters are based on figures tabulated in Peters *et al.* (2008), and acoustic impedances given for high-porosity saturated till in Vaughan *et al.* (2003). Vp and density are slightly higher than listed in Peters *et al.* (2008) although only each by 100 m/s and 100 kgm^{-3} and these are probably small compared to potential natural variation in the subglacial environment. The implied acoustic impedance of 3.4 $x10^{6}$ $kgm^{-2}s^{-1}$ is within the range for dilatant till mentioned by Vaughan *et al.* (2003) (referencing Atre and Bentley, 1993).

Andy Smith: *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.*

Response: Maybe the term 'mismatch', or suchlike, is more appropriate – we'll also revise 'error' throughout. We will emphasise the points made in this comment in the main text.

Andy Smith: *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?*

Response: It's true that there is residual moveout on the gathers, and we confess that we're not entirely sure where this arises from. However, the non-stretch NMO correction has been very effective – originally, the moveout on the reflection is 800 ms, so residual moveout of less than 4 ms is negligible (0.5 %). Furthermore, we would prefer just to acknowledge that the non-stretch NMO does leave some residual moveout than either distort wavelet amplitudes with a conventional NMO correction (via stretch), or apply a harsh stretch mute.

Andy Smith: *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* h_d *=1.5 and 2.0 m responses in Fig 5a,b, i.e. where reflectivity decreases over the first 10°?*

Response: We picked a shot gather to display, and maybe this is not the most appropriate record. We will present a more-representative gather in the resubmission. We did force the fitting operation to pass within the error bounds of the zero-offset reflectivity, because it was otherwise difficult to assess the quality of fit. However, we've been consulting with a geostatistics guru at Imperial College and we are confident that we can repeat this analysis and come up with a more appropriate fitting method – that will either improve the degree of fit, or at least have more-representative uncertainties (see Olaf Eisen's comment regarding the apparent small size of our uncertainty, given the quality of the fit). This new analysis will feature in the resubmission.

We agree that there may be a good match between the composite response and the intermediate-thickness responses, but these curves are difficult to define analytically and hence assessing a fit might prove difficult. It might be possible to explore a fitting operation that 'fixes' the position of the polarity reversal and defines AVA curves around this, but it might take some time to develop. We'd therefore prefer to stick with an assumption of an ultra-thin layer, and see how the geostatistical analysis develops.

<u>Responses to comments from Olaf Eisen (RC C425)</u>

Olaf Eisen: *Nevertheless, some issues should be elaborated more and in a more explicit manner. Among others, this concerns the concept of effective reflectivity (sec. 3.2) as well as relative reflectivity (eq. (4)).*

Response: As explained in previous comments to Andy Smith, effective reflectivity will be better-introduced, and our 'arrival' at Equation 4 will be clarified.

Olaf Eisen: *The dependencies of the reflection coefficent is rather special. An explanation how the reflection coefficient depends on Q would by nice, as the different Q-values are discussed later.*

Response: As in the reply to Andy Smith, we'll feature more of an explanation of Q-based reflectivity. It arises because of differences in the dispersive (frequency-dependant velocity) character across an interface: different frequency components of a wavelet can be reflected because of dispersion-based velocity contrasts, rather than any inherent contrast in the acoustic impedance (which is usually defined using a single velocity). A second issue is the concept of a 'lag time'. As energy passes into a low-Q material, it takes a certain amount of time for the layer to respond and reflect the energy back to the surface.

Olaf Eisen: *The main issues that concerns me is the concept of resolution vs. interference. The authors use both synonymously. However, there is an important difference which has to be made clear. Resolution is the ability to distinguish two signals. Commonly, criterions like those from Ricker or Rayleigh are used to pin down the limit mathematically. Resolution does not provide any quantitative statement about the ability of two signals to interfere and thus result in a superposition of amplitudes. However, this is exactly what the authors do, as stated on P767L4-7 ("At this theoretical limit of resolution, the two-way travel-time of a P-wave through the dilatant till layer is 3.4 ms, hence only ray-paths that lag PP by less than this can interfere and therefore contribute . . . ". Whether the first-break signals analysed in an AVA interfere or not depends very much on the shape of the source wavelet (i.e. whether it is only one wavelength long or more) and on the SNR. For minimum-phase signals, it is likely not of importance. However, for mixed-phase signals, less dominant parts of the later wavelet can interfere with the main part of the primary signal. Although the authors could show that in their synthetic and field cases the interference does not play a role if resolution is possible, the two concepts should not be mingled. The statement p770l4/5 and the synthetic data in Fig.4 imply that the wavelet is longer than one wavelength and not minimum phase, so that some degree of superposition takes place even for resolvable signals.*

Response: Yes, our source wavelet is mixed-phase and its main half-cycle (the second, in the control cases) is preceded by a small initial arrival. We thank Olaf for this comment, and we will ensure that the concepts of resolution and interference are clearly separated in our resubmission. However, we don't think that our observations are vulnerable to this confusion, since we pick the first half-cycle of composite wavelet (and in the resubmission, will be picking the second half-cycle) although we will relax the 3.4 ms criterion since this applies for an ‘ideal’ wavelet and not our mixed-phase pulse. Instead, we will look at the duration of the wavelet (typically around 8 ms) and use this as a criterion for the potential for interference instead.

Olaf Eisen: *A second greater issue is the usage of the Shuey-term cross-plot. Although the authors state the limitations of the Shuey term because of the underlying assumptions, a less aware reader might nevertheless miss the limitations and consider the cross plot as "the" means to identify thin-layer effects. I therefore suggest to state unambiguously already in the paper outline in the Introduction as well as the Conclusions that the Shuey-term cross-plot is a useful tool, but neither sufficient nor necessary to identify thin-layer effects, depending on the elastic properties.*

Response: We’ll make this explicitly clear throughout the revised manuscript.

Olaf Eisen: *Variables used for medium properties are only distinguishable from the context, so I suggest to introduce variable superscripts like* ^{ice}, ^{till}, ^{lod}, ^{dil}, *or alike for variables such as v, Z, Q, etc.*

Response: Thanks for the suggestion – we’ll implement this.

Olaf Eisen: *Both,* σ *and* ν *are Poisson’s ratio. Please make your choice.*

Response: Apologies for the inconsistency: we choose σ.

Olaf Eisen: *In some cases values are assigned to variables in a strange way.*

Response: We’ll amend these, and check for consistency.

Olaf Eisen: *The authors switch between absolute values for the layer thickness and fractional values of λ. Where necessary, the absolute values could be extended by the fractional value.*

Response: We'll ensure consistency throughout in a revised manuscript.

Olaf Eisen: *I suggest to provide an own subsection to the Greenland case, e.g. 5.1, and move the discussion of Peters et al. results (p777l17ff) to a new subsection 5.2. (e.g. "Revisiting BIS"). The present discussion of Peters et al. of BIS needs to spread out more specifically. Wording should be chosen more carefully.*

Response: This comment disagrees quite considerably with Andy Smith's views, that the reinterpretation of the BIS is probably unjustified. We feel that our cautionary comments about the validity of the BIS interpretation are still valuable but, as mentioned in the response to the relevant comment from Andy (and in later ones to Rob Bingham), I will be 'toning-down' part of this section. It is therefore unlikely that the BIS discussion will be allocated its own subsection.

Olaf Eisen: *At several places the numerical accuracy of provided figures does not make sense physically. As such, it implies an unobtainable accuracy: p776l9: Reflection coefficient to the fourth digit does not make sense. The least significant digit for the Poisson ratio in Table 2 and Table 3 seem somewhat exaggerated, especially in the latter case, where the Poisson ratio hardly varies. Here, the authors should revisit their results and maybe reduce the decimal place.*

Response: Ok – we'll lose precision wherever possible.

Olaf Eisen: *p760: l6-8: rewrite. l9: Specify/include: We model "the seismic response" of dilatant "till" layers ... "with a forward model". l19: unclear what upper and lower refer to.*

Response: We will implement these changes. Upper and lower refer to layering within the stratification – we'll rephrase this.

Olaf Eisen: *Chapter 2 beginning: It is somewhat confusing the way the quality factor influences the reflection coefficient. It would be nice to make a clear difference here between reflection coefficient and reflection amplitude.*

Moreover, l11 states "fraction of energy", while eq. (1) uses and p770l10 refers to amplitude. It has to be made clear that the reflection coefficient used here is the amplitude reflection coefficient.

Response: We'll be including more discussion of Q-based reflectivity in light of other comments on the manuscript. We'll also be careful to clarify that we're talking amplitude rather than energy.

Olaf Eisen: *p763 l5: R(q): what's q, slowness? It's not used before or afterwards. R(q) is an exact solution.*

Response: 'q' is actually a typo... Change it to 'Symbol' font, and it's a θ!

Olaf Eisen: p766. *Is not 1. and 2. one approach of two-part travel-time and AVA, and 3. The reflectivity method the second approach? Approaches 1. and 2. are interpreted together at the end (p768, l1-3). l5: "1D full-waveform forward model" (otherwise I expect something different then the reflectivity method), full-waveform is not used afterwards as description for this model. l7: ray tracing: using which program? l23: I suggest to introduce the general form P. . .P here: "arrive at the surface as P-waves, denoted **P . . .P**."*

Response: We do interpret travel-times and effective reflectivity together, but we feel that it is important to understand them separately in the initial model introduction. Full-waveform is probably not appropriate here – reflectivity should have been used instead. Ray tracing was performed in Matlab, using our own script. We will investigate moving the descriptions of the raypaths to this section.

Olaf Eisen: *P767 last paragraph of 3.1.: very difficult to understand. Rewriting useful. l6: Theoretical limit: for vertical incidence? l22: meaning of "reflectivity within" unclear. l25: Interference with the PP reflection?*

Response: This entire paragraph will be clarified in light of the comments regarding resolution vs. interference.

Olaf Eisen: *p770 l4: PP is just reflectivity not "effective reflectivity", isn't it? l5: Grey triangles are on the absolute maximum? l8-l12: And what was done then, whats the point of this approach? l19: The energy of a P-wave is always in the radial direction. Only the ratio of vertical vs. horizontal energy/amplitudes depends on propagation angle. l23: "increasing reflectivity": specify in text for clarity in which way increasing, e.g. increasing magnitude, increasing negatively or positively. Difficult in general for values around 0 with switching signs.*

Response: Yes, the effective reflectivity of *PP* is identically similar to simple reflectivity since there are no preceding transmission losses. However, that doesn't invalidate this terminology and we can still plot the reflectivity of *PP* on an effective reflectivity axis.

Grey triangles will be shifted to the second (larger) half-cycle in the resubmission. However, it's true that they were positioned on the absolute maxima of their respective half-cycle.

The energy of a P-wave is either in the vertical or radial direction: indeed, at zero-incidence, it is always in the vertical direction, and more of it appears on the radial component with increasing incidence angle. Since we only look at the vertical component of the synthetic data, there is another amplitude decay that we do not account for.

Olaf Eisen: *p771 Rewrite: For acoustic impedance, ...: "To quantify this difference, we substitute R(0) values observed in the model and the known acoustic impedance for ice in Eq. (1) and rearrange for Zj to calculate the apparent acoustic impedance . . . . " Write down equation for Zapp. L27: grid-search and best-fit: elaborate some more what you mean by this.*

Response: Each point will be included in the revision.

Olaf Eisen: *p772: first paragraph: provide reference to table/figure. l6: check <=2.5m. Either wrong or provide lower bound. l24: Which model is meant here?*

Response: Reference will be provided. <= is a typing mistake. The 'model' is any model comprising an ultra-thin layer – we'll rephrase this.

Olaf Eisen: *p773: Provide reference/explanation, where eq. (4) is coming from. l16: Not errors, but differences. Also elsewhere in the text.*

Response: A consistent with Andy Smith's comment – we'll change error to either 'difference' or 'mismatch'. We'll also clarify the explanation about Equation 4.

Olaf Eisen: *p774: Real data: - How about the across-line geometry, e.g. from airborne radar? - What does the ice/snow surface look like, especially in respect to the ghost and its possible interference? - What is the minimum shot-geophone distance to calculate R(0)?*

Response: There are no airborne radar data in this area (as I found out to my disappointment when trying to design the survey!). A second seismic profile crosses this one orthogonally, and doesn't show any potential for out-of-plane events. There is no snow around in the area; the ice surface in places is very wet, other places very solid and dry. Although the source ghost may be interfering with our reflections, there is no systematic near-surface consistency for successive shots (80 m apart) hence ghosts are a possible source of random error in our AVA curve. The minimum source-receiver offset is 0 m.

Olaf Eisen: *p775: first paragraph: provide reference to figure. l24: elaborate on grid search. l22: Fig 10c??? -> Fig 6c? l26/27: The uncertainties on Z and □ seem very low, given the AVA distribution.*

Response: References to be made. Fig 10c is from an earlier draft – yes, it should be 6c. We'll be revising our fitting procedure, using a geostatistical algorithm, which should make the uncertainties in our derivative quantities more reasonable.

Olaf Eisen: *p776 l20: numerical inversion of what for what?*

Response: Inversion of the AVA curve for all component quantities; physical properties of each of ice and dilatant/lodged till.

Olaf Eisen: *p777 l5: cepstral: explain to TC-readers.*

Response: Will do. A cepstrum is the amplitude spectrum of an amplitude spectrum, used for looking at periodicity in the frequency domain.

Olaf Eisen: *p778 l16: Not only for predictive, but also for diagnostic modelling or for paeleo reconstructions.*

Response: We'll include this.

Olaf Eisen: *Figure 1: 2nd last line: best-fit Shuey terms.*

Response: We'll include this.

Olaf Eisen: *Table 3: Include values for Z and σ model values in caption, as they serve as reference values for the deviations dZ and dσ. The small variation of σ for all Q-models and thicknesses should be emphasized and discussed more in the main text.*

Response: Values to be included. The small variation of σ arises because the best-fit terms through our curves all deliver a positive AVA gradient. It's actually an insensitivity to S-wave speed that causes the apparent insensitivity to Poisson's ratio, and once v_S becomes small with respect to v_P, Poisson's ratio tends towards 0.5.

Olaf Eisen: *Figure 3a: Plotted over offset: what is the bed, which angle is the incident angle at the bed? Do you use the PP angle at the ice-dilatant till as the incident angle of the bed and over offset sort the other reflections of these angles? Include value for layer thickness hd in the caption.*

Response: I'm not sure I understand this comment, as the incident angle at the bed is included in the plot. However, yes, the angle we use is that which *PP* arrives at, as would be done if no thin-layer case was suspected and the wavelet was interpreted as having reflected from a single interface.

Olaf Eisen: *Figure 4: Would be nice to scale the amplitudes in a way that it is able to see the first arrival of the wavelet, is that at 0 ms? I might have gotten something wrong, but the data have been nmo-corrected. Why do the picks still have considerable curvature as f(□)?*

Response: We'll be updating our picks in light of Andy Smith's comments. We again acknowledge that there is residual moveout, but this is less than 0.5% of the moveout originally shown across the gather.

Olaf Eisen: *Figure5+6: Grey hairlines too thin, difficult to see when printed.*

Response: We'll thicken hairlines.

Olaf Eisen: *Annotated MS: Only highlighted parts (i.e. without comments) indicate potential to improve style and/or readability by partial rewriting.*

Response: All annotations will be considered and applied.

Olaf Eisen: *The authors overuse "notes". Often introduce as "(note:. . .)", the reading is disrupted, especially in those cases where two levels of parentheses occur (e.g. p763l3/4). In many cases the "notes" can be incorporated in the text as ordinary sentences.*

Response: Agreed. Bad writing style, that we'll address.

Olaf Eisen: *The authors seem to like to replace sentences with half-sentences ending by a ";", making reading more difficult. In nearly all cases this is not necessary and the half sentences can be replaced by ordinary sentences.*

Response: Another characteristic of mine that I'll try to suppress.

Olaf Eisen: *Consistency: Always "thin-layer" when used as adjective, e.g. thin-layer considerations, thin-layer responses, etc. Introduce variables the same way. Varies between e.g. velocity v, velocity, v, velocity (v). Figure in the text Fig. or Figure?*

Response: We'll ensure consistency throughout.

<u>Responses to comments from Rob Bingham (RC C591)</u>

Rob Bingham: *At the close of the introduction (top of P762) the authors state the manuscript will apply their strategy to (a) Russell Glacier and (b) reinterpretation of Peters et al. (2007). Later in the paper, a whole Section (5) is devoted to (a), which is entirely deserving given it is the authors' data. In effect however the promised part (b) forms only a small part of Section 6. I guess from the introduction I expected more equal weight to be given to these objectives. I'd be more inclined only to mention the Greenland objective in your introduction, as this is the major "real data" section of the paper. The Peters et al data reinterpretation is fair enough as one implication to be discussed.*

Response: We appreciate that maybe we over-sell the reinterpretation early on, so we'll downplay it at this stage of the introduction.

Rob Bingham: *Throughout the manuscript there is an overuse of "notes" given in bracketed sentences. These statements are not always notes in my opinion, but valid points that should not be understated in this way. In most cases take away the brackets and word "note" and just leave the sentence in.*

Response: As with the comment to Olaf Eisen, we will address this and improve the writing style.

Rob Bingham: *Inconsistency throughout manuscript concerning notation for Poisson's ratio (σ or ν?). Make sure Knott- Zoeppritz is always spelt with a hyphen.*

Response: Again, we will ensure consistency throughout.

Rob Bingham: *P762, Eq 1 and line 19: Should use either capitals or not for z.*
P763, Eq 2: Why use small p and capital S?
P763, line 5: What is q in R(q)?

P763, line 15, Fig. 1a, Table 1: There is something wrong here. In Fig. 1a, acoustic impedance Z (or z, you decide!) decreases from curve i to curve v BUT in Table 1 acoustic impedance INCREASES from curve i to curve v. I think it's just that Table 1 column for z needs reordering.

P763, line 24: Should be Shuey (1985)?

P764, line 13: sp. Poisson's

P765, line 11: Missing word "till"

P765, line 26: remove hyphen between "more" and "rapidly"

P767, line 17: It would be more correct to say if it "were" resolved.

P771, Line 23: sp. within

Response: All typographic-type errors will be rectified.

Rob Bingham: ***Fig. 1:*** *The first time I referred to this figure (from p.763) I had no idea what A and B were, so I think it's worth adding these into the caption for this figure. Also in this caption, "Poisson's" should be written with an apostrophe, and "specific" should be "specified".* ***Fig. 6:*** *Remove comma after "data" and sp. Poisson's.*

Response: These will be improved in the revised manuscript.