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The Cryosphere
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.

O. Eisen (Referee)

olaf.eisen@awi.de

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General

The authors investigate the seismic response of thinly-stratified till units underlying an ice sheet for varying angles of incidence. Using different modelling approaches they first establish synthetic data sets to understand the principle relation between layer properties and the AVA-signature of seismic signals. The established analysis procedure is then applied to a measured data set from the Greenland ice sheet. A conven-

tional AVA-analysis yields contradicting values for the impedance and Poisson's ratio of the ice-underlying till. In contrast, with the application of their AVA-thin-layer-analylsis they obtain consistent properties, indicating the presence of a water-rich lodged till unit. For a second, already published AVA analysis from Antarctica, they explore alternative scenarios for data interpretation.

Given the increasing importance of processes at the basal interface of ice masses, involving ice, the underlying material and liquid water, the manuscript presents a considerable methodological contribution to deducing the bed properties from seismic AVA surveys and analyses. Moreover, the immediate application to Russel Glacier in Greenland convincingly demonstrates the advantages in the approach taken by Booth et al. to reduce the risk of interpretation ambiguities and data misinterpretation.

The paper is well written and structured, the overall approach can be followed. 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)). 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. More major and minor issues are listed below as well as in the annotated manuscript.

Major Issues

The main issues that concers 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 raypaths 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.

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.

Other Issues

- 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, ν, Z, ρ, Q, etc.
- Both, ν and σ are Poisson's ratio. Please make your choice.
- In some cases values are assigned to variables in a strange way, e.g. p766l15 or p773l4/5: $Z_{dilatant}$, here=3.90×10^6 kgm^{-2} s^{-1}, instead of "here, $Z = \ldots$". Value-assigning respective lists such as p775l24/25 do not improve readibility either.

- The authors switch between ablosute values for the layer thickness and fractional values of λ. I suggest to use absolute values throughout, as despite defining $\lambda = 12$m other values for the wavelength are mentioned as well in the text. Where necessary, the absolute values could be extended by the fractional value, e.g. p767l1: "for $h_d = \lambda/4 = 3.0$m" or p770l2.
- Structure:
  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.
- Least significant digits:
  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 exagerated, especially in the latter case, where the Poisson ration hardly varies. Here, the authors should revisit their results and maybe reduce the decimal place.

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.

p762

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 made clear that the reflection coefficient used here is the amplitude reflection coefficient.

p763

l5: R(q): what's q, slowness? It's not used before or afterwards. R(q) is an excact solution.

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 discription 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**."

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?

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.

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 Z_j to calculate the apparent acoustic impedance . . . . "
Write down equation for Z_{app}.

l27: grid-search and best-fit: elaborate some more what you mean by this.

p772 first paragraph: provide reference to table/figure.

l6: check <=2.5m. Either wrong or provide lower bound.

l24: Which model is meant here?

p773 Provide reference/explanation, where eq. (4) is coming from.

l16: Not errors, but differences. Also elsewhere in the text.

p 774 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)?

p775 first paragraph: provide reference to figure.

l24: elaborate on grid search.

l22: Fig 10c??? -> Fig 6c?

l26/27: The uncertainties on Z and ν seem very low, given the AVA distribution.

p776

l20: numerical inversion of what for what?

p777

l5: cepstral: explain to TC-readers.

p778

l16 : Not only for predictive, but also for diagnostic modeling or for paeleo-reconstructions.

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

- **Table 3:**
  Include values for Z and ν model values in caption, as they serve as reference values for the deviations dZ and $d\nu$. The small variation of ν/σ for all Q-models and thicknesses should be emphasized and discussed more in the main text.
- **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 h_d in the caption.
- **Figure 4:**
  Would be nice to scale the amplitudes in a way that it is able to see the first arrivel of the wavlet, is that at 0 ms? I might have gotten something wrong, but the data have been nmo-corrected. Whey to the picks still have considerble curvature as $f(\Theta)$?
- **Figure5+6:**
  Grey hairlines too thin, difficult to see when printed.

Writing and Annotations

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

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

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.

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?

Please also note the supplement to this comment:
http://www.the-cryosphere-discuss.net/6/C425/2012/tcd-6-C425-2012-supplement.pdf

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