tc-2020-147 Reviewer 2 Response

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1 Introduction

Review of "Grounding zone subglacial properties from calibrated active source seismic methods" by H. Horgan et al Alex Brisbourne, August 2020.

The authors present an assessment of active seismic data analysis methods using measurements made at the grounding zone of Whillans Ice Stream. Data cover both grounded and floating ice and therefore present an opportunity to assess and calibrate existing and new data processing methods used to obtain absolute properties of the subsurface. To this end the paper is a useful addition to studies of this nature and builds upon the previous work of Holland and Anandakrishnan (2009) (from here referred to as HA2009). The manuscript is well written and structured. However, as outlined in my comments below there are a number of clarifications needed in order that the reader can ascertain exactly how the analysis is applied and how closely this fits with previous work. The methodology description in insufficient in places and clearer self-referencing would improve the readers' ability to follow the methodology.

General comments Section 2.6 - Estimating subglacial properties – Optimisation. It's not clear to me how this process is being carried out but as far as I can tell a single solution is being obtained for each shot. The problem is that within the measurement uncertainties and the uncertainties in the determination of A0 there will be a suite of solutions which fit the observations, and as with any inversion it is not acceptable to select only the best-fit solution. There appears to be no attempt to represent the suite of possible solutions.

In our submission we attempted to represent possible solutions in the following way:

- We assume the dominant source of uncertainty is from our estimate of source size.
- We approximate the uncertainty in our source size using the standard deviation of the source size distribution (Figure 4, P8 L15-16.)

- We estimate normal incidence basal reflectivity (R_{bInt}, R_{b10}) using the average value and the average ± one standard deviation of the source size. No inversion is required at this stage. We estimate a solution for each shot. No mixing, smoothing, or spatial normalisation is applied.

- To obtain more information about the substrate we invert the reflection amplitude versus offset for seismic properties (V_p, V_s, ρ) using the average source size, and the average source size ± one standard deviation of the source size (P11,1-2). These source sizes are all propagated through the inversion to retrieve seismic properties. We do this for each shot with no mixing, smoothing, or spatial normalisation.

- We plot the result for the mean source size with symmetrical error bars. The size of the error bars are the maximum difference from the mean retrieved seismic properties and those retrieved using the low (A_0-1σ) and high estimates of source size (A_0+1σ).

We agree that plotting negative velocities or densities is not helpful. We are also interested in better quantifying uncertainties through the inversion. Our existing method can produce very small uncertainties as inversion from a range of source sizes can result in a very narrow band of results, or the same result. This is exacerbated by our use of constrained nonlinear methods. One alternate approach would be instead of limiting ourselves to the mean and standard deviation to instead provide an ensemble of source sizes based on the observed distribution of sources and present the ensemble of results. Where this still results in too narrow a range of uncertainty a minimum value could be set, informed by our R_b results.

Temporal variation by tidal strengthening is mentioned (Walker 2013). Could this be contributing to some of the uncertainty/range, especially at the grounding zone?

[Walker and others, 2013] suggest a long-term temporal strengthening upstream of the grounding zone in their fixed-fulcrum model. In their simulations this strengthening would reach a maximum approximately 1.2 km upstream of the grounding zone for ice that is 1 km thick. While our results show overall stiffer till than that observed elsewhere beneath the Siple Coast ice streams, they do not show a clear pattern in stiffness. If the reviewer is suggesting the possibility of variable results at different stages of the tidal cycle we refer them to our response to Reviewer 1's comments where we explore the relationship between our results and the stage of the tide.

There is no mention of the free surface effect (see for example HA2009 - for a receiver on a free surface, at normal incidence the received amplitude is double that of a receiver far from the boundary). The amplitude ratio uses A12/A2 whereas the known ratio method uses A1/A0 (no square). If the free surface effect is not corrected for could this cause the doubling of A0 with the multiple *method as the square of A1 means this does not drop out as a ratio? Or does this fall out elsewhere?*

The free amplitude scalar referred to by [Holland and Anandakrishnan, 2009] applies to normal incidence rays in an isotropic medium with the receiver at the free surface. We currently account for this effect using the amplification approximation provided by [Shearer, 2009][Equation 6.19] that uses the square root of the impedance contrast between the source and receiver locations. Our geophones are buried at approximately 0.5 m depth and our shots are at approximately 27 m depth resulting in a correction to our path amplitude factors of approximately $\sqrt{10}$. A more complete treatment is provided by [Aki and Richards, 1980], and a comparison between the [Shearer, 2009] approximation and the [Aki and Richards, 1980] approach are planned as future work.

Specific comments P4L8 – Where does the -20C refer to? Floating ice? Base of firn? How is the velocity model for the sub-firn ice column determined and what is it?

-20C was chosen as a representative temperature for the ice column (see [Paterson, 1994] Fig. 10.6). Below the depth constrained by our shallow refraction model, our velocity model consists of a linear extrapolation to a V_p corresponding to -20C (3860 m s^{-1}). We keep this velocity constant to the base of the ice. [Kohnen, 1974] demonstrate an increase in V_p of 2.3 m s^{-1} per degree C, so we are fairly insensitive to our choice of temperature. Out ray tracing is however sensitive to the temperature structure and inversions in the temperature model would lead to diving rays that complicate the modelling of direct arrivals. This will require more investigation if ray tube effects for direct arrivals are to be accurately estimated.

P5L10 – Georod channel to channel variability greater than geophones – can you comment on why would this be?

We suspect the variability we observe results from less consistent coupling of the georods to the surrounding snow and firn. Our method was to bury the georod in a shallow (approximately 0.5 m deep) trench. (We used georods where the components were vertical when the unit was placed horizontally.) The geophones had short spikes that were inserted into the snow/firn, preferably into a hard layer, at a depth of approximately 0.5 m.

P5eq2/P15L26 – Correct me if I am wrong but it needs to be made clear that Eq. 2 is for a basal reflection, i.e. assumed vertical through the firn. The γ_d for diving waves referred to in Eq. 5 is in the firn and is more complicated as it must account for the ray tube energy loss (Medwin and Clay, 1998, eq 3.3.31). Presumably this is used somewhere for the direct-path pair method and should therefore be presented.

This is a good point. We ran our analysis for the Known Reflector method, and Primary–Multiple Ratio method with both the γ estimate we present in our submission and a more complete ray tube treatment. The results are largely similar and we chose to present the simpler 1/distance γ estimate for simplicity. The more complete treatment, which uses the square root of wavefront energy, does however result in a linear regression gradient between the two methods that is closer to 1:1 (1.6 versus 2.0). We have not run the direct–path methods with a ray tube approach and will endeavor to do so.

Eq3/Eq4/5 – be more explicit where equations are taken from in HA2009. It would be helpful to label the equations with the name used to reference them in the manuscript (amplitude ratio/direct arrival etc) and perhaps set the paper structure out with similar sub-headings to make it easier to follow.

Will do. We have changed some subscripts but will make it clear which equations are from [Holland and Anandakrishnan, 2009]. We will also label the equations with the names we use to refer to them, and look to make the structure similar to [Holland and Anandakrishnan, 2009] where possible. Thanks for these suggestions.

Eq. 4 – derived from HA2009 eq5 at normal incidence – where does the factor of 2 come from on γ_i^2*?*

This is a typo. The correct equation was implemented in our analysis. Thank you for pointing this out.

P7L27 – What does Fig 4C,D refer to? (no labels on Fig. 4)

These refer to an earlier draft. Apologies for the confusion.

Table 1 – please highlight consistent columns (e.g. all means one colour, all medians another colour ...). It is very difficult to read as it is presented.

We will follow this advice.

P8L4 – is this method essentially using HA2009 eq10 to determine A0? It would be useful to state this if so. As this is a new way of implementing the method I would like to see it explained with more clartity such that it can be reproduced.

Yes, we use [Holland and Anandakrishnan, 2009] equation 10 with the source amplitude obtained using the known reflector method. Our method is an optimisation of [Holland and Anandakrishnan, 2009] equation 10 where we minimise the misfit between our $R(\theta)$ estimated using our A_0 and the R_θ resulting form a range of possible acoustic properties.

P8L8 – I don't follow the argument that this is insensitive to attenuation as it is later used to calculate R (where exactly is this? Do you mean you use Zoeppritz and therefore the A0 isn't actually used?).

Our method of estimating source amplitude using a known reflector requires an estimate of attenuation. Our method of estimating basal reflectivity also requires an estimate of attenuation. Both these steps use [Holland and Anandakrishnan, 2009] Equation 10. As the same attenuation is used in both steps the resulting $R(\theta)$ is independent of the attenuation chosen.

You do a direct comparison of A0 in Table 1 which is sensitive to the choice of attenuation so it is important at that stage at least, and the result that this A0 is so different to that calculated by other methods is a significant result.

That is correct. Changing the attenuation does change the value of A_0, which is why we use the strength of the regression to assess the relationship between methods not the gradient. We chose to use reasonable values for attenuation instead of tweaking our attenuation to force a 1:1 gradient when comparing our A_0 estimates.

Figure 4 – Use consistent x-axis ranges as this is deceptive otherwise. I can't see ABCD labels as referred to in the text.

We will make these changes.

P10L6 – Please state the range of incidence angles at the reflector that picks are made out to. As you state later this is important in the range of forms the Zoeppritz curves will take.

We will quantify this for each line. It is dependent on ice thickness due to interference from the direct arrivals. Almost all shots have reflector picks out to 25 degrees with some having picks out to 30 degrees.

P10L9 –The use of the Zoeppritz equations will require basal ice velocities and density. What values are used or are these also allowed to vary within the optimisation? It needs to made clear in the text that these are assumed/fixed and at what values (if that is the case – are Table 2 values used on grounded ice too?).

Ice properties are set to the values in Table 2 for both grounded and floating ice. These were not allowed to vary in the inversion. The sensitivity of our results to these values could be done either by allowing them to vary in a constrained way in the inversion, or by forward modelling possible values.

Fig. 6 – How are the uncertainties calculated and what do they represent? Why are they so much greater on the ice shelf? They are very small on the ice stream. Is this realistic given the uncertainties and range of A0?

Please see our earlier comments regarding uncertainty estimation. The small uncertainties result from $A_0 \pm 1\sigma$ resulting in the same inversion result. Adopted the uncertainty analysis we suggest above, including a minimum value should result in more representative estimates.

Fig. 6/7 caption – mention that the R values use the KR method.

Will do.

Fig. 6/7 and P11/L3 – The Vs uncertainties allow negative Vs velocities although the lower limit in the Zoeppritz search is zero. Vp looks to be restricted to 1440m/s although it looks like the uncertainties would take this lower given the symmetry. I suggest that negative Vs values are not plotted. This would indicate that the uncertainties are derived by error propagation which comes back to my point above about the optimisation of the inversion and accepting a single solution, the uncertainties cannot represent a suite of inversion solutions. How are negative Vs values derived by using the full A0 range with the Zoeppritz equations?

Please see our earlier comments regarding uncertainty estimation.

P15L10 - As you talk about transitions of 500 m it would be good to state the size of the Fresnel zone. You should then mention the scale length of the fluting and how this compares to the Fresnel zone.
The width of our first Fresnel zone is approximately 240 m (100 Hz signal at 760 m depth in a 3860 m s^{-1} medium) and the corresponding quarter wavelength is approximately 9.7 m. The fluting modelled by [Christianson and others, 2016] has a wavelength of 20 m; amplitudes of 5.75 m; and RMS heights of 4 m.

P15L15-17 – I don't agree that the comparison demonstrates the efficacy of the amplitude ratio method, as stated in the following sentence, it may correlate well but it produces values twice that of the AR method. Is this not a contradiction? Or does twice the A0 value not affect estimates of R to a high degree?

As we state earlier, we can reduce the gradient of the relationship by varying our attenuation estimate and path amplitude factor (γ). We have chosen not to do this as we think using values widely adopted in glaciology better emphasises areas where both methods are deficient. If we adjust our attenuation values until we produce a good fit between our Known Reflector method and the Primary-Multiple Ratio method the result would be misleadingly good. However, the methods do correlate well, as apposed to our analysis of direct path methods. We will clarify our language and thinking in this regard.

In closing we thank the reviewer for their detailed and constructive review. We appreciate the time and thought that went into it.

References

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