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Interactive comment on "Antarctic ice sheet thickness estimation using the H/V spectral ratio method with single-station seismic ambient noise" *by* Peng Yan et al.

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GENERAL COMMENTS

I thought this was a good paper that applies a relatively novel method in an Antarctic environment. The paper is generally well-written, though could benefit from more quantitative discussion and consideration of its limitations. The scope of the paper matches that of The Cryosphere and, with revision, I think it will be a good addition to the literature. I make some specific comments on three main shortcomings below, then mention some smaller issues that would be required in a corrected manuscript.

SPECIFIC COMMENTS:

The authors show the application of the H/V seismic technique for quantifying the thickness of an Antarctic ice sheet. Two approaches are tested, based on the estimation of resonance frequencies and a more-developed inversion approach. Ice thicknesses are then compared to observed depths in Bedmap2, with the authors concluding that inversion approach is preferred but still acknowledging that some mismatch between the inversion and the Bedmap2 reference. In the paragraphs below I suggest some areas where the paper could be improved. I would emphasise that I do think the paper will make a good contribution to The Cryosphere with some attention to these issues.

1) For a paper that considers inversion and quantitative data interpretation, there's a lack of detail in the text. While I appreciate that a thorough description of the inversion approach is perhaps not required, it sits uncomfortably that there is only one simple equation in the paper – and no presentation of the raw data or the inversion approach. The authors also consider the uncertainty in Bedmap2, but give much less attention to the uncertainty in their approach (which seems counter-intuitive since I'd suggest that the uncertainty in Bedmap2 is always going to be much less than in the H/V method). Table 1 does list uncertainties in resonance frequencies, but how these are defined should be clarified. For example, peaks E012 and N148 in Figure 3 seem to be more poorly defined than others, yet their uncertainty in Table 1 seems to be consistent with the wider dataset.

The lack of uncertainty analysis sits a little uncomfortably with the frequent description of the method being "reliable" (first instance in L16) and robust. These are subjective terms that would be best qualified with numerical evidence. This is not to say that the method is unreliable, but the authors could do more to demonstrate this rather than relying on qualitative descriptions. Just present the observations and let the readership decide!

2) The authors also seem very keen to justify the need for H/V analysis, in part by

pointing out the drawbacks in other techniques (e.g., L40-96). Some of these points are valid – gravity modelling is clearly a rather low-resolution technique (although the reference to gravity data processing in L54 is very out-dated) – but I don't see that the 'economic and logistical' requirements of H/V acquisition would be significantly less than RES or seismic. The authors could lessen the criticism of these methods, and present the case for H/V analysis more simply as another interesting option for a field survey.

Additionally, the authors often point out that this is the first application of the technique on an Antarctic ice sheet: I'm also unsure that this in its own right is significant. While the logistics of an Alpine study are likely simpler than an Antarctic deployment, I would suggest that the 'seismically quiet' Antarctic – featuring simpler subglacial geometries - likely offers better-quality data than in the Alps (as mentioned in L314-5) so it should be no surprise here that promising results are obtained.

To summarise this paragraph, the justification for the authors' approach should be slightly moderated: just let the results speak in the own right, and suggest how they would complement (rather than replace) existing geophysical practice.

3) The discussion section ends with some conflicting and speculative advice for H/V compliant seismic acquisition.

In terms of the conflicting recommendation, the authors propose a desirable record length for acquiring useful H/V acquisition. In L320, the authors caution against using a record length that is only 1 hour long vs. one that is 5-days long. However, in L322-323, they suggest that a 'proper' record length of 1-2 hours would be sufficient. Firstly, the word 'proper' is misused here and it is unclear what the authors mean by this – presumably they mean "a record length suitable for reliable analysis"? But more importantly, there is an inconsistency between the recommended record lengths. I don't see how a 1-hour record would be inappropriate, but a 1-2 hour record would be fine. Additionally, in terms of the cost and logistic requirements of a deployment, if

you're going to record seismic noise for 1-2 hours, why not record for 3-4 hours?! The logistic cost is presumably the same, but you'd maybe get better data quality!

In terms of the subjectivity of this recommendation, presumably the authors have longer record lengths from their seismic stations? It should be possible to show how the estimate of ice thickness converges (?) on the Bedmap2 thickness as a function of record length, and therefore remove the subjectivity from this argument.

SMALLER CORRECTIONS:

L11: "implemented at single stations using seismic ambient noise waveforms" seems rather specific for the first line of the abstract, which is just generally about H/V methods.

L16: "reliably measured" is subjective – objectify it with some performance metrics.

L31-33: "global climate change" is misplaced here. While ice sheet thickness is important to know for sea-level rise studies, linking it here to "global climate change" is a step too far.

L34: Logical jump. The sentence starting "Moreover" likely needs a new paragraph, or a bit more development from the previous sentence.

L35: The need for accurate thickness measurements is true, but it's more likely achieved with RES than it is ever going to be with H/V analysis. Yes, there are places where RES is problematic, but the places that H/V offers better accuracy and/or precision will be few and far between. This links partly to Comment (2) that I made previously.

L41-42: What is "deep seismic sounding" as opposed to the seismic reflection and refraction methods that are already mentioned?

L45: Remove "While".

L49-51: Reference to Bedmap data seems misplaced at this point in a background

review.

L54: How big a problem would terrain corrections specifically be in Antarctica? Also, the gravity processing reference (Drewry, 1975) seems very out of date.

L59: What complement, specifically, does H/V offer to established methods?

L72: Over-selling the technique: "which suggests its powerful effectiveness . . .etc". As with all techniques, there will be places where H/V is problematic.

L85: Another logical jump. Before talking specifically about the analysis parameters, you need to explain what the analysis requires.

L96: Repetition of the complementary application of H/V spectra (again without clearly explaining the complement).

L103: "relatively sparse" – spares compared to what?

L106: how does burying a station "guarantee" data quality? Presumably, you mean "to improve data signal to noise ratio"?

L124: "is not that robust" – very subjective. Defend and quantify what you mean by this. What kinds of errors result?

L157: Repetition of this point about sedimentary structure investigations.

L162: Capitalise "Geopsy" for consistency with earlier instance.

L208-209: Give the frequencies in the main text. I appreciate that they are listed in the table and in the figures, but key observations could be usefully included here.

L246: Define what you consider to be "consistent" – consistent to within what threshold?

L273-274: Again, define what you mean by "adequately constrained" – to what threshold? You could just say (e.g.) that estimates are consistent within a 5% threshold and let the readership decide if this is adequate.

L282: "inverted" rather than "inversion".

L284-287: what is it about these two stations that cause them to perform so differently?

Table 1: Could be useful to have % error, relative to the bedmap thickness?

Figure 3: Needs a colour key.

Figure 4: Plot the elevation panels at the same vertical scale. It's also a little unclear to me what the data in this figure show. If the red dots are the reference Bedmap2 thickness, how is the ice thickness defined in the panels showing the ice/rock interface? It can't be from bedmap, otherwise the red dots would coincide with this interface.

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