The Cryosphere Discussions

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Comment on tc-2021-340

Anonymous Referee #3

Referee comment on "Recovering and monitoring the thickness, density, and elastic properties of sea ice from seismic noise recorded in Svalbard" by Agathe Serripierri et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2021-340-RC3, 2022

This is a very interesting analysis of a uniquely dense sea ice deployment of seismographs using ambient noise guided mode dispersion curves to invert for bulk ice properties (in two propagation directions). With a bit more detail (suggestions below) it's suitable for publication in its present form as an initial analysis of a data set that I strongly suspect has a lot more to offer in more detailed analysis. Will the data be publically available somewhere (not apparently in the Acknowledgments).

The authors have done a good job in fitting analytical expressions to (beamform-selected) autocorrelations, with modes decomposed via a SVD method, to extract dispersion curves in EW and NS directions. The size and density of the array used (247 seismographs with interstation spacings as close as 1 m) is unprecedented to my knowledge for this type of application. Additional deployment and experimental details would be appreciated. Were the instruments buried, for example? Were ice cores taken for ice-truth measurements during of the experiment? How long did it take to deploy these instruments at this scale (important to know if such experiments might be repeated in other locations). Could similar (or at least practically useful) results be obtained with a smaller deployment (this can be tested by decimating the data set of NCFs). This would be valuable to address given the motivations expressed in the abstract and paper.

Most of my comments below address where I suggest that methodological and other points might be usefully expanded (including, perhaps, some supplemental figures that could be in a appendix).

Complicating methodological or physical factors that may explain the observed variance in the parameters were underexplored (e.g., can the dispersion curves be better resolved or meaningfully smoothed to improve the data as shown in example in Figure 4?). It was unclear (to me) exactly which stations were being correlated for use in constraining the NCFs (e.g., the linear array stations and azimuthally selected stations in the grid (which would mean that only stations intersecting a 10-degree cone from the linear array stations

were actually utilized, but no stations from the corners, for example (?)). It would seem that a great many potential crosscorrelations within the experiment were not used. That's fine for this initial study, but some additional detail and quantification of this geometry and data usage would be appreciated (with 247 stations there are potentially around 30,000 potential NCFs that could be calculated, of course; how many total NCFs went into the inversions here out of these potential ~30,000 station pairs?). I'd suspect that a more comprehensive subarray strategy might yield improved results, and the ability to assess spatial variations (the authors hint that this will be a next step, along with icequake analysis in future work at the end of the paper). Structural variation across the study region may explain some of this four-parameter estimation experiment (but again it's not clear to me but it would be nice to assess this effect more thoroughly; it's alluded to for example in line 270).

The authors employ a simulated annealing strategy to obtain a starting model. It was unclear to me, at least, how this leads to new information regarding determination of the measurement errors, unless this was simply evaluated to be consistent with the chi-square value, which is simply consistent). For this reason. I suggest that it is more accurate to indicate that the fit (e.g., line 280) between model and data is statistically "consistent", rather than "excellent". They subsequently use MCMC to obtain a posterior PDF with a uniform (limited) range prior distribution for each parameter, but do not show the covariances of the parameters. A posterior illustration of parameter covariances would also be helpful to illustrate the tradeoff space between the four parameters. It is interesting that the inferred ice thicknesses differ systematically in the NS and EW directions in Figure 7. Can the authors elaborate on this (in line 270 they allude to special variations in the physics of the problem, but I believe what they meant to indicate was something like "spatial variations in ice model parameters".

All things considered, this is a significant work with a unique data set that could use a bit more polishing to realize its potential, and will no doubt be valuable for further analysis.