Title: Random Noise Attenuation of Sparker Seismic Oceanography Data with Machine Learning

authors: Hyunggu Jun, Hyeong-Tae Jou, Chung-Ho Kim, Sang Hoon Lee , Han-Joon Kim

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reviewer: Richard Hobbs

General comments– The paper has an improved balance of content, though I still think much of the detail of the filtering method could be moved to supplementary material as it will be of low interest to the principal readership of this journal.

Editor to advise if American spelling is acceptable and some phasing is still awkward in places but it does not detract from the understanding.

Comments and corrections.

line 8 punctuation

line 23: “dropping equipment” ? Rephrase as this is ambiguous

line 25: first published use of technique was by Gonella, J., Michon, D., 1988. Deep internal waves measured by seismic-reflection within the eastern Atlantic water mass. Comptes Rendus de l Academie Des Sciences Serie II 306, 781–787 (in French with English abstract). However it was Holbrook who first used the term seismic oceanography.

line 27/28: “The differences in temperature and salinity between water column generate ...” poor english

line 29: need reference here suggest Ruddick, B., Song, H., Dong, C., Pinheiro, L., 2009. Water column seismic images as maps of temperature gradient. Oceanography 22 (1), 192–205.

line 31: need reference for “conventional oceanographic methods” and link to the equipment that was dropped in line 23.

line 34: redundant work “careful”

line 39: “...higher exploration expenses...” ambiguous as higher than what

line 45: the vertical resolution of 1.5 m is based on the theoretical Rayleigh limit however in a 3D environment it is unlikely that you can achieve this due to interference from out-of-plane scattering.

Line 49/50: the maximum impedance contrasts are smaller

line 53 & 64: most noise sources are not random so the authors need to define which noise sources that consider as problematical, especially for SO data where a dominant noise is caused by pressure variations at the receiver due to waves on the surface. This peaks at frequencies below ~5 Hz so with sparker SO you should do better then airguns as you can set your low-cut filter at a higher frequency without damaging your primary signal bandwidth. I think what the authors are looking at here is noise generated from the ship as it has a distinct time characteristic and I would not be surprised if this was during periods of tidal flow against the direction of travel. So perhaps the ships propellers were run at a faster rate to maintain speed over the ground which has resulted in more cavitation. However, that period does not match with the statement that the ship speed was 5.5 knots. Suggest authors include auto-correlation functions both in space and time of the red boxes (Fig 3) indicated to justify that the synthetic random noise used in training datasets is appropriate.

Figure 2 – does this show the whole line or just the portions after the shelf sections were omitted?

Line 163 no comma in velocity value and possibly need to correct style to m s^{-1} and other units as necessary. Also this is ambiguous as 'water velocity' in the context of oceanography describes the mass movement of a body of water. An acceptable term is 'sound-speed'. As the temperature and salinity of the water is known (XCTD) then authors could do better than just use a generic sound-speed by using the equations of state.

Line 173; define what you mean by very small as it looks like the calculated reflection coefficients are at the level of the minimum resolution of the XBT and given that you have two XCTD why did you use a constant density for those?

Line 209: justify why you used 1000 kg m^-3 for the density as true density is provided as part of the model – why does this change make the synthetic more SO-like if you don't apply a scaling an shift to the compressional wave velocities too. It sounds like a fix to overcome inefficiencies in your modelling process eg calling depth time and followed by a convolution. Given the level of adjustment and modification (see line 224), I see no purpose for using these sophisticated models as a set of time-shifted spikes (eg an enlarged version of Fig 16) would have probably done equally as well.

Line 210: were the simulations acoustic or elastic?

Line 247-260: you don't actually know the 'ground truth' as the section without added noise is, as you correctly point out, still contaminated with noise and will contain inter-layer peg-legs and out-of-plane events which you algorithm will also treat as primary signal.

Line 275: what is x and y in equation 5

line 339 suggest a sub heading here to indicate you have changed to dealing with the SO data

line 342 "and" → "to reveal"

lines 342-344 suggest: Holbrook et al. (2013) suggested analysing the slope of the spectrum for the complete data before calculating the slope of the spectrum from the water reflections because the noise that needs to be suppressed is more evident in the spectrum of the complete data.

line 369 "clearly imaged" → "improved"

Line 375-378, 385-387 & 389-393: I think you are making too much of this as the differences between the approaches is marginal as shown by your fig 20. Essentially both methods worked on the data to a sufficient degree to enable meaningful estimates of the slope of the internal wave to be discerned. If you feel you want to emphasise the benefit of D2 vs D1 then show a difference plot of the two.

Line 397: what sound speed model did you use for conversion?

Line 406: would be useful to quote horizontal limit of resolution due to the Fresnel zone which I estimate, by eye-balling your data, to be ~0.02 cpm so above this value you would expect the amplitude to the perturbations to be strongly filtered so the fact the signal becomes noise limited is to be expected and further noise reduction will not provide any useful information. I note that your data appear to have a lower frequency content than I would expect – how do you explain that? What were the acquisition parameters? As these directly effect the data signal bandwidth.