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Ocean Science Discussions

Interactive comment on "Seismic imaging of a thermohaline staircase in the western tropical North Atlantic" *by* I. Fer et al.

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This manuscript demonstrates the utility of seismic oceanography in providing new insights into the spatial distribution of thermohaline finestructure and internal wave energy. Such insights are particularly useful considering the coarse horizontal resolutions of more traditional oceanographic techniques and subsequent lack, for example, of observed horizontal energy spectra. By the thorough analysis of independent regional measurements, the manuscript clearly shows (by qualitative examples) that a thermohaline staircase has been seismically imaged, and is associated with low internal wave energies. However, I feel that the novelty of the manuscript is compromised by the absence of a detailed qualitative description of the imaged thermohaline staircase, and a lack of quantitative comparison between seismic and oceanographic data.

The main contribution of seismic reflection profiling to oceanographic observations is its ability to image thermohaline features at unprecedented horizontal resolutions. A discussion (and better image) of the horizontal amplitude variation within the staircase reflectors, particularly where they diminishes at the staircase western edge would have been interesting (as suggested by reviewer 2). Moreover, the whole seismic section shows some spectacular structures, which are not interpreted or even mentioned in the manuscript.

I am unconvinced by the reflection coefficients computed from the seismic data. No errors are given, but the coefficient values do not match particularly well to those of the C-SALT CTD. Moreover, I suspect that the other CTD data used in the manuscript is likely to show even smaller reflection coefficients, and thus be even less correlated to the seismically deduced values. Why do the reflectivity coefficients change so much across the staircase section in regions where the stacked image amplitudes appear more or less continuous? Please see later comments.

I am unsure that the synthetic shot gather contributes to the manuscript. Other than the influence of the direct wave, the same conclusions could be achieved from computing a zero-offset seismogram from the CTD data (i.e. convolving the reflectivity as computed from the hydrographic data, with the seismic source wavelet). No attempt is made to look at amplitude variation with offset, incorporate background noise or input a background velocity field, which spatially varies over the streamer length. Such synthetic modeling from hydrographic data is not novel. Displaying a shot gather without giving any background about the seismic reflection profiling experiment makes the synthetic modeling section inaccessible to readers not familiar with the technique. The results of the synthetic analysis are not displayed clearly, or in a manner which can be easily compared to the observed stacked seismic section.

The authors claim that the data gives insight into the longevity of thermohaline staircases. However, no evidence is given as to how long the staircase was present before the seismic data was shot. I would recommend cutting this analysis completely and simply adding a sentence referring to the typical lifetimes ($\sim$ several months) of such features as found from previous studies.

The data is novel, as far as I am aware, in showing a towed displacement spectra of a thermohaline staircase. However, with such a wealth of proximal current and microstructure measurement available for comparison, it seems a shame that the spectral analysis of seismic displacement spectra was not more quantitative. It would be good to compute dissipation rates from the seismic spectra (e.g. see Klymak and Moum (2007), Sheen et al., (2009)). Best-fit slope estimates at different wavelength regimes would also add some insights. It would also be nice to see internal wave energies (or at least spectral amplitude) mapped across the whole section. See later comments.

Overall, I recommend publication but after improvement to figure clarity, robustness of reflection coefficient estimates and perhaps more in depth analysis of the data. Some more detailed comments follow.

Figures:

Fig 2 - Mark HRP locations on axes. It would be better to see the staircase inset enlarged in a separate figure, as this feature is the focus of the whole paper. I would also include a larger region, to encompass the fading out of staircase reflectors at depth and on the western edge. Reflector separations could be clearly marked, showing their 20 m separation and increasing thickness with depth.

Fig 7 - Can you include the temperature and salinity profiles here? In addition a plot of the reflection coefficients convolved with a source wavelet would enable easier comparison to seismic data, and demonstrate more robustly that the seismic data is able to resolve the staircase layers. In addition, not accounting for the source wavelet means that positive reflection coefficients are ‘missing’, making it difficult to compare to Fig 8. Alternatively plot the reflectivity depth profile computed from the seismic image, de-convolved from the source signature. It would also be useful to plot the reflectivity profile of HRP station 123, which is used for the synthetic modeling and is more

proximal.

Fig 8 – Plot is not very clear. Perhaps show the inset from Figure 1 alongside this Figure for easier comparison. The caption here indicates that the higher reflectivity is erroneous - can we really trust these values? I think that the normalization value, A_0 should be constant throughout the seismic transect. Perhaps it would be better to compute the normalization value averaged over one or two a horizontally layered sections, which show a clear multiple and seabed reflection and use the same value throughout the data. You could plot a depth profile of the reflection coefficients on Figure 7.

Fig 9 - Confusing for readers not familiar with marine seismic data acquisition and processing. What is the strong linear feature, which comes in at times later than the direct arrival? It is very difficult to compare the model to the CTD data or seismic section. Why plot the whole shot gather, when the region of interest is too small to see clearly? Plot the inset bigger so that reflector depth variations can been seen. What are the brighter hyperbolic reflections at 850 m depth? How is depth computed from two-way-travel-time? Perhaps perform a normal move-out correction and overlay a modeled stacked seismic trace in Fig 2/Fig 6. Offset (km) should be added to the axes.

Fig 11 - Please give a clearer indication of the regions over which the reflectors were tracked. It is better to de-trend the spectra by multiplying by kˆ2 (or kˆ(2.5)), otherwise it is vey difficult to compare slopes with the GM spectra. You should reference Krahmann et al., 2008, for justification of the assumption that every 6th reflector is de-correlated.

Text:

Page 362, line 9: Clarify ’background levels’. Does this refer to other regions of seismic image or the noise level?

Page 362, line 19: Not sure it can ’improve estimates’. Seismically deduced dissipation rates have large uncertainties and no attempt is made here to extract quantitative values of mixing from the seismic data. Seismic techniques can certainly add to mixing estimates (particularly from horizontal spectra) and give a better indication of the spatial variation of ocean mixing.

Page 364, line 16: Remove 'full'

Page 364, line 25: I don't see how this contribution of temperature to reflectivity is shown in section 2.2.

Page 365, lines 8-9: Double-diffusive thermohaline staircases are density compensating, as demonstrated in lines 22-25, p370. It follows that staircase seismic reflectors, which largely follow temperature gradients, do not necessarily track isopycnals. Please check vertical density gradients against that of temperature or acoustic impedance.

Page 365, lines 25-27: Was the direct wave addressed at all in processing? Figure 9 indicates that the direct wave may affect reflectivity up to $\sim$ 400 m depth, very close to the thermohaline staircase. To clarify to readers not familiar with seismic techniques, add a sentence explaining the reason for Kirchoff migration, and also why time-migration is used for reflectivity computations.

Page 366, lines 23-26: Widess, 1973, state a vertical resolution of one eighth the DOMINANT wavelength. For the dominant source frequency here ($\sim$ 30 Hz) this gives a resolution limit of > 6 m. In addition, the Widess criterion is for a single, isolated, high velocity layer in a homogeneous background. Here we have a series of steps, temperature changes are smoother in water compared to rock layer interfaces and there are effects such as interleaving. Please be more conservative than 4 m.

Page 367, lines 7-9: Later you say that the typical acoustic velocity is 1491 m/s?

Page 367, line 14: What do you mean 'moved up'. Mixing of time (earlied/later) with geology (up/down).

Page 367, line 24: Why use a high-pass filter? It would be better to convolve the

reflectivity series with a wavelet similar to source wavelet.

Page 368, section 2.3: Explain in more detail what you are modeling i.e. shot gather, the source frequency content, techniques used (finite difference?)

Page 369, lines 11-12: 'sampling' - well it is affected by the source wavelet frequency content. I would omit lines 11 and 12. There is no reason for there to be processing artifacts and this sentence only leads reader to think that there may be one!

Page 370, lines 1-3: These lengths don't match up (212 km -189 km is not equal to 11 km)

Page 373, lines 18-20: Station 127 does show higher dissipation rates but it is also much closer to the continental shelf, than the staircase CTD (123). Would it not be better to compare station 123 with say station 125?

Page 374, lines 14-15: Klymak and Moum (2007) show that in general it would be surprising to find a kˆ(-2) horizontal spectra in the open ocean. They show that the vertical spectra roll off at 10 m in a GM wavefield, affects the horizontal spectra, depending on the frequency content of the wave field. A -2 slope is not generally observed and that is why Garrett and Munk fit a -2.5 spectra in 1975.

Page 374, lines 16-27: The staircase spectra appears to exhibit a steeper slope in the internal wave regime than the open ocean spectra. Can you compute the mean slopes at different wavelength regimes? What are the kinks at wavenumbers around 10ˆ(-2.3) cpm and 10ˆ(-1.9) cpm, which are observed in both the open ocean and staircase? The first kink may represent the transition to the stratified turbulent regime (Klymak and Moum (2007), Riley and Lindborg (2008)). The fact that the first change in spectral slope occurs at lower wavenumbers in the staircase spectra compared to spectra from the 'open ocean', adds to evidence for lower internal wave energies in the staircase. Perhaps the second kink is the influence of noise, but then I would expect all spectra to reach the same level.

Page 374, lines 25–26: Are you sure that the drop-of above the 1 km scale is not a spurious data point at the end frequency, due to averaging over the whole tracked line?

Interactive comment on Ocean Sci. Discuss., 7, 361, 2010.