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Ocean Science Discussions

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Interactive Comment

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 is a very good paper, using a combination of legacy seismic and conventional oceanographic profiler data to directly address oceanographic questions focused on the strong thermohaline staircases near Barbados. The data are not coincident but an intelligent discussion of the effects of non-coincidence ameliorates this issue a great deal. The paper shows that staircase steps are well-imaged, including resolution, source wavelet effects, and with correct amplitude – and it's rare that amplitude is well-calculated in water column seismics. Oceanographic data AND seismic data are used in combination to investigate the internal wave field in the staircase.

My only complaint overall is that the main focus comes across as " we can see the layers and they are consistent with synthetic seismic images". I think the paper would

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be a lot stronger if the seismic data were used to tell us something NEW about the staircases. I have two suggestions. In figure 2 the staircase is visible from 190-212 km, and has a fascinating tendency to fade in and out. This is a pretty unique finding, it could be used to learn something about how staircases connect to a non-staircase adjacent region. Maybe the spacing or amplitude changes (or doesn't). Maybe there is a subtle shift from negative reflectors to reflectors of both signs (as in intrusions).

Secondly, a strong result from the paper is the analysis of internal wave energy levels from reflector displacements, with a good comparison with shear and microstructure observations. It's convincing, but this was discovered previously by conventional means. How about extending the analysis to map the internal wave energy over the whole eastern part of the section? In particular, you could map internal wave energy levels as one moves from the staircase to the region outside the staircase. This would help to test the "chicken/egg hypothesis" about staircases: ***Perhaps staircases filter out the shorter vertical wavelengths of the IW field, which reduces shear and turbulence, and so allows the staircase to be maintained.***

My recommendation is that the paper is strong enough to publish as it stands, but I would love to see the ideas above followed up. I have a number of minor suggestions below that could help with ease of reading, and a few minor science comments sprinkled in. I leave it to the authors to decide how to address them, and don't require a detailed accounting of how they are dealt with.

1. Figs 4-9 seem to jump between the three data sets. I suspect that fig 8 is the culprit and could be shifted.

2. P367. lines 24-26. So long as R is well-resolved compared with the source wavelet, it shouldn't depend on the source wavelet at all. In particular, high pass filtering to remove content longer than 50 m shouldn't be needed, or even used. The act of convolution with the source wavelet takes care of all of that.

3. P 370 lines 14-16. Statement beginning :Although the density ratio...". Figures 4 and

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5 don't really prove this statement to me. Can the figures be improved to bring this out? Idea is to show that the shear and not the density ratio account for the non-existence of staircases. Perhaps superimposing a smoothed density ratio field and shearˆ2 field as contours or color shading.

4. Also, figures 4 and 5 could be made to match the geography of the profile locations if they were plotted with 128 on the left.

5. Figure 4b. You apparently masked $0<R<1$, which is relevant to diffusive sense convection and therefore staircases vs intrusions, and definitely exists in some of the profiles. Probably best to put it in.

6. Fig 5 caption. The "(i.e., every 10th data point)" is confusing unless you state delta-z = 0.5m.

7. Fig 7 really should be augmented with a second panel that shows the result of convolving R with the source wavelet, and somehow showing the source wavelet, likely showing how negative apparent reflections are generated by the wavelet sidelobes.

8. The calculated R (fig 7) is pretty much completely negative, but there are strong positive reflection bands in the seismic image of figure 2 inset. And the reflection coefficient in fig 8 appear to be both positive and negative, if I read the figure correctly. This is all pretty confusing - can you change the text to clarify?

9. Fig 8 caption "100 and 400 m" Do you mean 1400 and 4000 m?

10. Fig 9. Axis should include offset in km. And you have an opportunity here to point out the usefulness of the streamer, with non-vertical reflections, since the figure clearly shows larger amplitude at non-zero offset.

11. P371, line 20. "Changes" is unclear - maybe use "vertical contrasts"?

12. P 372, top of page (lines 1-3). You use CTD info to calculate a synthetic, and then compare reflector depths to CTD-derived depths? Hmm......

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13. Fig 10. If the point is to compare Eps_obs and Eps_IW, why not show them in the same panel, with stn 127 on the left?

14. Section 3.4 on Longevity of staircase. There must be some useful CTD info in the usual databases to better address this.

15. It's more common (?) to show and use slope spectra, and is easier to justify for seismic reflectors since you don't have absolute knowledge that the reflectors follow isopycnals on all scales. If you don't do this, you should at least mention the kˆ2 link to slope.

16. P 374 line 6. "Spectral" seems like a change of subject and might have a paragraph break to indicate this.

17. P 374 Top few lines. "Another conclusion...". How can this conclusion come from figure 10? Perhaps you should tell us Nˆ2 or (N/N_o)ˆ2 in the caption, and/or show a dashed line in the figure corresponding to the 0.6 value.

18, "reflector" and "reflection" seem to be used interchangeably, but the reflector is the thing that causes the reflection that is seen in the image.

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