Ocean Sci. Discuss., 7, C3–C7, 2010
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Ocean Science Discussions

OSD

7, C3–C7, 2010

Interactive Comment

# *Interactive comment on* "Stochastic heterogeneity mapping around a Mediterranean salt lens" *by* G. G. Buffett et al.

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Received and published: 18 January 2010

Review of "Stochastic heterogeneity mapping around a Mediterranean salt lens" by G. G. Buffett, C. A. Hurich, E. A. Vsemirnova, R. W. Hobbs, V. Sallares, R. Carbonell, D. Klaeschen, and B. Biescas

by Stefan Carpentier

General comments:

In the current manuscript, the authors attempt to delineate a seismically reflective body in the Mediterranean-Atlantic outflow in terms of von Karman stochastic parameters correlation length and Hurst number. The reflectivity in many parts of the body is complex and diffuse. As such, conventional interpretation of this reflectivity may fail



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Interactive Discussion

Discussion Paper

to retrieve important finescale variations in impedance contrast and causative physical parameters. The motivation for using von Karman stochastic parameters is elegantly phrased in lines 21-24: "However, the original work of Theodore von Karman was to characterize random fluctuations in the velocity field of a turbulent medium (von Karman, 1948), indicating that the method is also suited to the characterization of ocean fluid dynamics." In my opinion, this is a key statement and an exciting incentive for directly coupling stochastic parameters of seismic reflectivity to actual causative physical processes. This makes the paper well-motivated and it can be of great interest to the Ocean Science audience.

The manuscript is well written and concise. I do strongly recommend that the authors consider and discuss some possible limitations on the information-content and interpretation of the parameters Ax and Nu. This can be considered a moderate revision. If they do so, and attend to some minor points that I found in the text, this paper will be a valuable contribution to the OS journal and to thermohaline dynamics in general.

Specific comments:

I have worked with correlation lengths and Hurst numbers quite a bit in the meantime and I must say that I have grown a bit skeptical on the usefulness of the latter in seismic data. Hurst number estimates found in this study tend to be well below 1 according to the histograms in Figure 1. In recent work of Carpentier and others (1,2), it was found that Hurst numbers actually exceeded 1 in both synthetic and real seismic reflection data to obtain best fits. The classical von Karman function had to be partially abandoned for this reason. Causing the high 'Hurst' exponents was the relative lateral smoothness of the data, due to band-limitation by the source wavelet and by limited seismic illumination. Fundamentally, lateral smoothness in seismic data is caused by first-Fresnel-zone averaging. Perfect migration will theoretically completely collapse the first-Fresnel-zone in a seismogram. In reality, lack of recording aperture (seismic illumination), attenuation, complex scattering, wrong migration velocity and many other factors will prevent this complete collapse from happening and a degree of first-Fresnel-zone averaging is maintained. Also note that 2-D migration in a 2D line will only collapse the first-Fresnel-zone in 2D, the out-of-plane Fresnel zone and associated averaging is left intact. This could be important given the points that the authors raise about that in the Discussion. I suspect that these GO data suffer from at least some of the factors mentioned above, having consequences for the lateral resolution. In this sense, I am wondering to what degree lateral fine-scale structure in these GO data can be resolved in the form of such low Hurst numbers. I am not contesting that these low Hurst numbers are found to best fit the lateral correlations in the data. But I do wonder what causes these low Hurst numbers and what information is contained in them.

In the recently published Carpentier et al. (1,2) studies, the Hurst number would drop to an artificial low level in synthetic data only in case of superposed uncorrelated noise, and in noisy parts of the real data a similar effect could be observed. In all other cases, the expected lateral smoothness caused way higher 'Hurst' exponents. Could it be that the low Hurst numbers estimates from your data do not represent actual sub-wavelength fine-scale structure, but are the effect of leftover uncorrelated noise or other high-wavenumber artefacts? I must acknowledge the good comparison with the Garret-Munk spectra of earlier studies and I am not familiar with the GO data and its quality. Also, the correlation length estimates appear bona fide and I have good experiences with the quality of correlation lengths in 'challenging' seismic data. It would be nice if the authors could discuss these possibilities on this forum and in their paper. In lines 8-10 of the Discussion, one sentence is devoted to aspects of vertical resolution. It would be good to discuss horizontal resolution and related aspects as well.

Technical comments on specific phrases:

Abstract, lines 6-8: This is a somewhat sloppy sentence. Hurst numbers have no associated scale lengths, this is the whole point: they denote the degree of scale invariance. Correlation lengths do imply a scale length; the upper scale to which scale invariance occurs. Better would be: “Lower Hurst numbers represent a richer range of high wavenumbers and correspond to a broader range of heterogeneity in reflection events".

Page 6, line 1: A minimum of 10 scale lengths is well on the safe side, I think this was recommended in a Levander or Holliger paper if I'm not mistaken. The 2 cycles seem on the low side, I wonder if the statistics in the windows are solid with this value. But I have no argument for this claim other than my own experience, where I used a minimum value of 4 cycles to prevent erratic estimations.

Page 6, lines 6-7: These over- and underestimation values were based on impedance contrast fields and Primary Reflectivity Sections. For complex scattered visco-elastic reflection data, I found underestimation factors of 6-10 for correlation length and over-estimation factors up to $\sim$10 for Hurst number in Carpentier et al. (1).

Page 6, line 24: I would change "... reveals the heterogeneity of the ..." to "... reveals the stochastic heterogeneity of the ...". It is a specific type of heterogeneity that you are mapping.

Page 7, lines 2-5: Again, the majority of your Hurst numbers are quite low. I wonder about the cause for this: actual structure or some kind of noise?

Page 8, line 28-29: "... frequency content . . . was too low ...". This sentence puzzles me. I would expect a very low frequency wavelet to cause smoother, more mixed data and therefore higher Hurst numbers. Maybe the frequency content is not so much lower, just less broadband and therefore less resolving.

Page 9, line 6-8: Can you really achieve up to 10 m lateral resolution? What band of source frequencies was used and which was the dominant frequency? It sounds quite optimistic for targets at 1 – 1.5 km depth.

For the rest I think the manuscript is very decent and should be published with some of the questions above answered.

(1) S. F. A. Carpentier and K. Roy-Chowdhury (2009), Conservation of lateral stochas-

tic structure of a medium in its simulated seismic response, J. Geophys. Res., 114, B10314, doi:10.1029/2008JB006123.

(2) S. F. A. Carpentier, K. Roy-Chowdhury, R. A. Stephenson, and S. Stovba (2009), Delineating tectonic units beneath the Donbas Fold Belt using scale lengths estimated from DOBRE 2000/2001 deep reflection data, J. Geophys. Res., 114, B10315, doi:10.1029/2008JB006124.

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Interactive comment on Ocean Sci. Discuss., 7, 1, 2010.