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Discussions

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

G. G. Buffett et al.

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Dear Editor, Ocean Science

The authors thank Stefan Carpentier for his contribution to this manuscript. His extensive experience in this field provokes some interesting questions about our methodology and interpretation of the data and is valuable to both the geophysics and oceanography communities.

We address his comments below and have followed most of his recommendations by making moderate revisions to the text. Where we disagree, this is discussed below. In the context of our research, we find particularly interesting the more recent research by Carpentier and others about the exceedingly high values of Hurst number (higher than

1) as this raises important questions about the significance of the values we obtained in relation to the migration operator.

We have taken into account all of the minor comments (addressed individually below). The most significant comment about migration we have addressed in the text and have modified it accordingly, as it was our decision to use unmigrated data rather than migrated data in the analysis.

The pdf file of the following comments is also attached to improve readability.

Sincerely,

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Interactive comment on “Stochastic heterogeneity mapping around a Mediterranean salt lens” by G. G. Buffett et al.

S. Carpentier (Referee): carpentier@aug.ig.erdw.ethz.ch Received and published: 18 January 2010

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 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 subwavelength fine-scale structure, but are the effect of leftover uncorrelated noise or other high-wavenumber artefacts?

Specifically, we have added the following paragraph to the text, to this end:

The data presented herein are unmigrated. Given the near horizontal reflectivity of the seismic data (dips average at about 3° with a standard deviation of 0.3°) migration does not appreciably change the position of reflectors. With this in mind and due to the fact that the spatial band-limit that is imposed by the migration operator smoothes the data, thus removing the information in the first few lags of the autocorrelation function, we chose to analyze the unmigrated stacks. Intriguingly, in recent studies by Carpentier et al. [2009 A,B] Hurst numbers were found to actually exceed 1 in both synthetic and real data in order to obtain von Kármán best fits. These exceptionally high Hurst numbers were said to be a result of the lateral smoothness of the data, caused by first-Fresnel-zone averaging during migration. Although migration, in principal, should completely collapse the 2D seismogram's first-Fresnel zone, it is complicated by factors such as the lack of a sufficient recording aperture (zone of seismic illumination), complex scattering effects, wavelet attenuation and wavefront healing, and the uncertainty inherent in choosing the correct migration velocity (sound speed). Therefore there will always be some residual first-Fresnel-zone averaging. Furthermore, 2D migration in a 2D seismic profile will only collapse the first-Fresnel-zone and does not address out-of-plane reflections. This result supports the fact that the Hurst numbers we obtained from unmigrated data are not in the upper end (nor do they exceed) the range of theoretical Hurst numbers, and therefore may be closer to the true values than those extracted from migrated data, which are biased by many of the factors mentioned. While we cannot definitively state that the low-end Hurst numbers are not a simply result of some remaining uncorrelated noise (thereby representing high-wavenumber artifacts, not actual sub-wavelength thermohaline finestructure) we can neither dismiss the possibility that the information contained in the first few autocorrelation lags actually has a positive effect on the von Kármán fitting process.

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.

We thank the reviewer for his comments. Specifically, after lines 8-10, mentioned, we have added the following to address horizontal resolution:

Horizontal resolution is determined by the Fresnel zone width [Yilmaz, 1987, p.470], itself a function of frequency, sound speed and depth. For the upper and lower limits of the meddy, considering a dominant frequency of 50 Hz, the horizontal resolution thus ranges between 12 and 30 m.

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”.

Thank you, we have changed this sentence correspondingly.

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.

At the time of analysis and manuscript preparation we were unaware of this Levander/Holliger paper, although some of their works have been cited. The 2 cycles used was, as mentioned, precisely just a minimum value for vertical window size.

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 overestimation factors up to 10 for Hurst number in Carpentier et al. (1).

Thank you, this change has been made to the text, citing the more recent publication by Carpentier et al., 2009A.

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.

Thank you, this change has been made.

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?

We thank you for your comment. As has been addressed in the new paragraph in the discussion, since we performed the analysis on unmigrated data (therefore, not smoothed) we cannot dismiss outright the possibility that some of the information content in the Hurst number is biased by uncorrelated noise. Nor, can we dismiss the possibility that the noise has a positive effect on the estimation of Hurst number, especially given the large values obtained in the latest publications by Carpentier et al.

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.

Thank you. Yes, this has been misstated and has thus been rewritten:

The notably lower Hurst numbers seen at the bottom of the meddy (Zone B) may be partially a result of the fact that the frequency content of the source used in this survey (10-70 Hz) was too narrow-band to recover the smaller thermohaline staircases known to occur at the base of meddies [Ruddick 1992].

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.

In lines 6-8 of page 9 we were at the time only referring to vertical resolution. The 10 m value is an upper estimate for “practically” resolvable vertical thickness based on a cautious assumption of [Widess, 1973]’s one-quarter wavelength criteria. As stated, one-quarter of the dominant wavelength is an upper estimate for resolvability. Due to the thickness and sharpness of acoustic impedance interfaces, as well as acquisition parameters, among other factors, this value can be increased. For water, there is both heat and mass diffusion across interfaces that produce a gradual change over a finite boundary width that may extend for several tens of meters (see Hobbs et al, 2009)1. So, we do emphasize that this value is an upper limit. We have updated the text to include this caveot. Regarding lateral resolution, we have followed your earlier recommendation and have added the following text:

Horizontal resolution is determined by the Fresnel zone width [Yilmaz, 1987, p.470], itself a function of frequency, sound speed and depth. For the upper and lower limits of

the meddy, considering a dominant frequency of 50 Hz, the horizontal resolution thus ranges between 12 and 30 m.

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

1 Hobbs, R. W., D.Klaeschen, V. Sallarès, E. Vsemirnova, and C. Papenberg (2009), Effect of seismic source bandwidth on reflection sections to image water structure, Geophys. Res. Lett., 36, L00D08, doi:10.1029/2009GL040215.

Please also note the supplement to this comment:
http://www.ocean-sci-discuss.net/7/C13/2010/osd-7-C13-2010-supplement.pdf

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