Comments on Assessing Atmospheric GravityWave Spectra in the Presence of Observational Gaps

Mohamed Mossad, Irina Strelnikova, Robin Wing, and Gerd Baumgarten

General comments:

Limitations of spectra:

There has been a resurgence on interest in techniques for determining scaling exponents, scaling properties of geosystems. Over the last decades many techniques have been developed, particularly to characterize/quantify the intermittency/ multifractal aspects of the system (e.g. trace moments, double trace moments, generalized structure functions etc.). Although the atmosphere is highly turbulent/intermittent/multifractal, the effect of the corrections is often viewed – as it was 50 years ago - as no more than an often fairly small "intermittency correction" to the spectral exponent (the $K(2)$ in this paper). This is a shame since even if this correction to quasi-Gaussian spectral exponents is small (for the velocity field it is typically of the order of 0.15 in the horizontal, but closer to 0.25 in the vertical), the effect of intermittency may nevertheless be huge if measured for example at moments higher than 2 (the moment relevant for the spectrum). For example, the physically important energy fluxes depend on the 3rd moment of the velocity and their variances depend on the 6th moment, and the latter are apparently divergent (see the review [1])!

Overall, the focus on spectra – that at best characterize the second moment – is thus rather limiting, especially if one is attempting to test theories that predict H (typically on dimensional grounds), but that ignore the intermittency. In these cases, at best the spectra must be supplemented with other techniques to determine $K(2)$ so that H can be determined using the relation $\beta = 1+2H - K(2)$. Techniques such as Haar fluctuations that directly estimate H are therefore advantageous.

Difficulty in estimating spectra when data are missing:

An additional problem with spectra is that they are notoriously difficult to estimate when there are missing data. For example, using interpolation to fill gaps (one of the methods used here) can cause huge biases. This is because the spectral exponent is related to H which is the maximum order of differentiability of the series (it is related to the fractal dimension of the signal). Therefore - as is typically the case in geophysics where $H<1$ then using linear interpolation (i.e. using nonfractal curves with $H = 1$) will badly bias the statistics (depending on the amount of missing data). This should be stated somewhere in this paper. Alternatively, as discussed to some extent by the authors if instead of interpolation, Lomb-Scargle is used, one finds that it has big problems with spectral leakage [2] and these are often not improved with Multi-Taper Methods (as advocated by some [3]). This could be mentioned.

Gravity waves

Since the atmosphere is highly nonlinear / turbulent, with Reynolds number of the order of 10^{12}, it is not clear why linear gravity wave theory should apply. [4] have argued that in reality, what is observed is a consequence of (high Reynold's number) scaling, intermittent fractional wave equation consistent with stratified Kolomogorov type turbulence. This would explain the existence of wave-like structures in the high Reynold's number limit as well as the observed exponents and their intermittencies. This alternative possibility should be mentioned.

Specific comments:

The paper is essentially a series of numerical tests of analysis techniques applied to synthetic series with a gap model, I have problems with both.

First the simulation method is problematic since it is not clear what the statisitcs of the resulting series are. Since it is a linear model, it should be nonintermittent (monofractal), and therefore presumably quasi-Gaussian, but this needs clarification. It is not trivial to even theoretically deduce the spectrum from the mathematical definition. Even if one wants to avoid more realistic multifractal models and stick to Gaussian ones, why not use fractional Brownian motion of fractional Gaussian noise that are standard, well-defined processes? At least we would know exactly what we are dealing with in the absence of data gaps and analysis issues!

Also of concern is the gap model. The statistics of the model are perhaps not as clear as they seem. The problem is that when the probability of gaps becomes large enough, many join together to become "supergaps", ultimately the left-over nongaps - where the data are sampled – may be a fractal set, see Mandelbrot's "trema" constructions (and theory) in [5]. The thing is that the fractal dimension then becomes a crucial characteristic of the sampling, and thus of the biases.

Sampling data with fractal holes/gaps is indeed highly pertinent since it seems to be a fairly general problem with many geophysical data sets. However it requires its own study: how does the fractal dimension of the sampling affect the analyses?

Haar:

I was disappointed that the Haar technique was not further discussed since it is the only one that (largely) avoids most the problems discussed above (I did note that the biases reported in the paper are apparently the smallest for the Haar, as found already in [6] and reiterated in [2]). Although it was mentioned that it is easy to apply to data with gaps, the reference to this nonuniform algorithm should be given (it is in appendix of [7]). The Haar method also allow for the determination of the intermittency corrections (the entire codimension function in fact).

References

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