More comments on Assessing Atmospheric Gravity Wave Spectra in the Presence of Observational Gaps

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Thank you for the generally useful clarifications and responses. Below, I still have a few comments that should be addressed.

We thank the reviewer for their insightful comments. We repeat the reviewers' concerns and provide our respective responses in italics. The changes take place in the revised manuscript.

Line 48: Why does b>2 necessarily lead to bias?! It will do so if there are data gaps, but why otherwise (assuming that a window was used before taking the spectrum)? Also, the existence of low-frequency variability ("longer periods that the observations..") is ubiquitous in geoscience, but why is this different from the usual problem that is solved by windowing?

We did not apply a window in order to see the leakage effects for Beta> 2 (see Klis 1994, Deeter and Boynton 1982), since the effect of windowing has been studied in many other papers. Low frequency variability is an example of the same leakage problem. The prewhitening and postdarkening technique is proven to fix this leakage problem without bringing a huge loss of amplitude/power.

Line 53: I think the problem here is that the authors have set themselves the problem of identifying periodic components that stand out in the presence of a scaling "background". In this case, the main methods of astronomy and seismology are not necessarily very helpful since they mostly focus on finding the exact frequency and phase of spectral peaks, not in estimating the scaling properties of "background" spectra (although there are exceptions…). The problem of accurately estimating the "background" i.e. a signal from a wide range of frequencies is more typically a turbulent, hence atmospheric, oceanic problem.

We have replaced "such as astronomy and seismology" with "in other branches of geophysics."

Line 74: The L-S and MTM together are still poor when the spectrum is scaling and with gaps, see comment on line 110 below.

We have removed the following text to avoid confusion: "however, a combination of both the LS and MTM seems to improve on its disadvantages (Springford et al., 2020)."

Line 93: As mentioned in my previous comment, determining spectra from series with missing data using interpolation will lead to a bias, unless the interpolation method has the same exponent b as the series i.e. in general, fractal (not linear) interpolation is needed. But the right exponent is needed before such interpolation can be done! Perhaps a “bootstrap” method of iteratively interpolating with closer and closer approximation fractal would be possible…

Presupposing the slope of the spectra by assuming a value for Beta defeats the purpose of this article in quantifying the effect of estimating the bias caused by three popular techniques for generating spectra.
One technique, FFT requires interpolation and linear interpolation is the simplest, straightforward, and commonly used technique. Our work quantifies the bias that one would expect using FFT with linear interpolation.
However, “bootstrapping” is a significant, labour-intensive addition to our current study and would be better suited to a future stand-alone manuscript.

Line 110: As indicated in the previous comments, the problem with Lomb-Scargle is that there is massive spectra leakage when either the spectral spike to too big, or the low frequencies have too much power (the exponent b is too big, b≈>1-2). This is because L-S is a regression technique that does not conserve the total spectral power. Adding MTM is not justified when there is missing data since the weighting functions are not orthogonal on nonuniform bases. The MTM often performs very poorly in scaling series with gaps.

We have addressed this point in the answer to line 74.

Line 125: The Haar order q = 2 exponent is exactly equal to b-1 irrespective of the size of the intermittency correction: the exact general result is x(2)=2H-K(2) whereas the result for spectral exponent is spectral exponents are exactly b = x(2)+ 1. The Haar fluctuations are very good for scaling processes but are not optimal if there are large periodic components superposed on it.

We rephrased line 125 to: ”Hurst exponent by $\beta = 1 + qH - K(q)$, since the power spectral density is a second-order moment we take $q = 2$.”

Line 179: I don’t understand the simulation. There are 35 frequencies and 35 phases. We are told that the frequencies are chosen from a uniform probability distribution, but what about the phases? Please write down the theoretical spectrum that this model generates. It shouldn’t be too difficult, and it is needed to clarify the properties of the process which are not self-evident. There are also potential issues of convergence since using only 36 sinusoids for each simulation, seems like a small number. At the moment, I can’t evaluate the model. The authors haven’t clearly responded to this key question raised in the earlier comments.

We reiterated that “The phase shifts φ_i are also randomly chosen from a uniform distribution within the interval $[0, 2\pi]$.” on line 185.

We have explained the choice of the number of waves on line 183. We are looking to replicate observations which indicate that there are a finite number of waves carrying most of the energy during a night of lidar observations. $M = 35$ is perfectly reasonable.

The theoretical spectrum $S(f)$ of our simulation is derived from the Fourier Transform of the discrete time series $x(t) = \sum_i^M f_i^{-\beta/2} \sin(2\pi f_i t + \varphi_i)$ to be $S(f) = S_0 f^{-\beta}$, where S_0 is an arbitrary constant, see Kirchner 2005 (Eq. 14), where in our simulation the highest frequency simulated is equal to or less than the Nyquist frequency. The spectrum will then show M peaks at the frequencies f_i of the sine waves. The height of each peak in the spectrum is proportional to the amplitude squared ${f_i}^{-\beta}$ of the corresponding sine wave.

Line 243: I don't see how aliasing can enter here. Isn't the lowering of the high frequencies and raising of low frequencies simply due to the smooth (linear) gap filling line having a paucity of high frequencies compared to the signal? We're replacing a signal with lots of high frequencies with one with only low frequencies. That's why interpolation is a bad idea!

We have removed "(i.e., aliasing)".

Line 335: Spectral leakage occurs for any finite nonperiodic signal, it doesn't matter what the lower frequencies would have been had they been present.

We agree, we mentioned this in line 286.

References:

- Klis, M.: Rapid variability in x-ray binaries—towards a unified description, NATO Science Series C, Springer, Dordrecht, Netherlands, 1995 edn., https://hdl.handle.net/11245/1.421015, 1994.

- *Deeter, J. E., & Boynton, P. E. (1982). Techniques for the estimation of red power spectra. I-Context and methodology. Astrophysical Journal, Part 1, vol. 261, Oct. 1, 1982, p. 337-350., 261, 337-350.*

- Kirchner, J. W.: Aliasing in 1/f α noise spectra: Origins, consequences, and remedies, Phys. Rev. E, 71, 066 110, https://doi.org/10.1103/PhysRevE.71.066110, 2005