Review on the paper ”Atmospheric waves as scaling, turbulent phenomena” by J. Pinel and S. Lovejoy.

The referee comments are in normal font, the author responses are in italics.

Referee 2:

This paper is the attempt to unify the very different approaches used in atmospheric physics, i.e. the highly nonlinear turbulent behaviour and the linear idea of wave theory that somehow contradict themselves. There is unquestionably a great need for such comprehensive research, and the authors already improved this manuscript. However, there still remain open questions that should be answered before publishing. Let me explain my rating:

The scientific content of this paper consists of four major parts, namely Sects. 2-5. In Sect. 2 the authors derive the propagators for waves and turbulence in Fourier space and relate them to the spectral densities *P* such that classical properties like the dispersion relation for waves and the Kolmogorov-Obhukhov spectrum is retained; modifications for ”space-time extension” and anisotropy are also included. In Sect. 3 both approaches are combined, allowing the wave propagator exponent to be of fractional dimension. Sect. 4 consists of a discussion of experimental IR data obtained by a geostationary satellite and the fit of the theory presented in the previous section. In Sect. 5, the authors tried to identify the singular behaviour of the waves from the satellite data as there is the expectation that at least this singularities should be observable. Still, the language or the syntax is sometimes unconventional, thus I had to read it twice to understand what is meant. I listed here the passages:

1. page 14801, line 21: ”infra red” should read either ”infra-red” or ”infrared”;
2. page 14801, line 7 and others: I would avoid colloquial use of contractions like ”let’s” or ”needn’t”;
3. page 14802, lines 8-10: Drop the parenthesis at the variables f and I and the ”the” in front of the ”g(r, t)”;
4. page 14803, lines 4-8: It seems that the sentence ”Indeed, ... contribution.” lacks a verb;
5. page 14804, lines 19f: The citations should be outside the parenthesis except for the years;
6. page 14809, line 6: I would use instead of a tilde in a running text words like ”about” or ”roughly”;
7. page 14812, line 13f: ”(anisotropic and fractional)” may be more readable;
8. page 14814, line 8: The variable ”a” should be in math style, not text style;
9. page 14814, line 10-13: The syntax of this sentence is somewhat odd;
10. page 14814, line 15: ”Gallilean” should read ”Galilean”;
11. page 14815, line 12-17: The word ”where” is used three times in succession;
12. page 14815, line 18: There is a power of 1/2 missing at the parenthesis, if I am not mistaken.

Au: Thanks, these have been modified accordingly in the text.

In addition, I have some comments concerning the figures:

1. Fig. 1: Is it possible to rearrange the axis names? At first it is confusing, especially log10 E(ω) and log10 k(km)−1 ;

Au: Done

2. Fig. 2: This second version is hardly readable.

Au: improvements were made.

3. Fig. 3: Why are the axis labels changed in comparison to Fig. 2, first column?

Au: The axes were inverted and only positive ω were considered in order to allows easier comparison with figure 3 of Wheeler and Kiladis, (1999).

2. In behalf of part 3) "Presentation Quality", the points mentioned above should be considered.

Concerning part 1) "Scientific Significance", I came to the conclusion that a publication is possible, even though the authors themselves acknowledge the speculativeness of their idea. However, I will mention some main points the authors shall address nevertheless:

1. p 14800: I welcome the comments on the implications of the fractional propagator given in Sects. 3 and 6. I would also appreciate some general conclusions by assuming anomalous wave propagators that can be added to the introduction, if possible.

Au: We have added appendix B which gives more mathematical details of the consequences of fractional propagators, notably for energy transport, we have referenced this in the introduction.

2. p 14801: I still do not fully understand why we can say that the wavelike part having a factor of 2-4 is of relevance if compared with a factor of 10^5 for the turbulent part. Even the purely turbulent figures 2a) are really good! The authors may stress why the wave-like part should not be neglected anyway.

Au: Turbulence is responsible for most of the atmospheric variability so that when we fit the data with $H_{wav}=0$, we already obtain an excellent fit. Still, atmospheric waves play a role in transferring energy and momentum and to obtain a more complete description of atmospheric dynamics, we have to take them into account as well. Finally, numerical simulations indicate that the morphologies of structures such as clouds can change quite bit even with only a relatively weak wave aspect.

3. p 14806: If the wave and turbulent propagators are combined, can we say something about the forcing φ in Eq. (15)? Is it still ε1/3 as for the turbulence?

Au: If I represents the velocity field, then the forcing φ in Eq. (15) will be that $\varphi=\varepsilon^{1/3}$ with

the overall constraint on the exponents H=1/3 due to dimensional grounds (as usual). For other atmospheric fields (such as the radiance field analyzed here), we expect the same basic mathematical structure to hold, but φ will be a different flux (it is still unknown in the case of the IR radiances) .

4. p 14809: I am still wondering if it is justified with $v_{wav} = 1.0 \pm 0.8$ to say that turbulent and wave speed are nearly equal? Because the v_{wav} error span is from 1.8 (where wave speed is nearly twice of turbulent speed) to 0.2 (thus the wave speed is one order of magnitude weaker than the turbulent speed).

Au: We are not claiming that the wave speed is equal to that of the turbulent wind. We claim that the value of the parameter which gives the best fit is $v_{wav}=1.0\pm0.8$ and we mention for the better understanding of the reader that the case $v_{wav}=1$ means that the wave speed is equal to that of the turbulent wind.

5. p 14813: If turbulent atmospheric dynamics is compatible with the observed waves and linear theories are not necessary, why do they exist (with more or less success)? Maybe the authors may comment on this in more detail.

Au: Thanks, we have added a paragraph at the end of the conclusions on this.

Some minor remarks: The authors may explain what a ”singular set” is (a singularity or somewhat different?).

Au: See modif. in text.

In Eq. (16), where is σ included (as mentioned in line 15)?
Au: See modif. in text.

What is the definition of v_{max} , and is it necessary (because it emerges just one time)?

Au: the parameter v_{wav} is the phase speed nondimensionalized by the turbulent velocity V_w. It is necessary to take into account phase speeds not equal to 1.

Is the choice of $||k|| = (k_x^2 - a^2 k_y^2)^{1/2}$ and $g_{wav} = \{i(\omega' / v_{wav} + ||k|| \text{sign}(k \cdot \mu)^2)\}^{1/2}$ simple heuristic?

Au: The short answer is yes, although it is a heuristic inspired by the Kelvin wave dispersion relation and the observed spectrum.

The problem is that there are only weak constraints on the form of the propagator so that a wide range of different propagators can be considered.

Some remarks to Part 2) ”Scientific Quality”. In general, the further elaboration of the theory and evaluation of measurements (except for the smaller annotations mentioned above) are rather appropriate.