

## ***Interactive comment on “Global Spectroscopic Survey of Cloud Thermodynamic Phase at High Spatial Resolution, 2005–2015” by David R. Thompson et al.***

**Anonymous Referee #3**

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General Comments:

This is an interesting use of Hyperion/EO-1 data to investigate cloud phase and, broadly speaking, spatial statistics. A significant part of the study is a juxtaposition of Hyperion and AIRS data. Cloud detection and classification methods for both instruments are compared, as are the resulting cloud climatologies for the lifetime of EO-1 (2005-2015).

However, the new and timely result is to use the fine spatial resolution of Hyperion to build scale-by-scale statistics, basically, semi-variograms that show a robust scaling regime once the natural variability emerges from the instrumental noise. A 3-parameter

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nonlinear function is used to fit the data. This is a timely development because of the emergence of scale-aware parameterizations of subgrid cloud processes in GCMs.

This manuscript thus has the potential to become a significant contribution to the literature. However, it needs in my opinion a major revision to get there. What made me struggle with the narrative was the decision to use the classic "2. Method" (should be plural) then "3. Results" structure when there are actually three distinct exercises in data analysis: (1) cloud phase classification, (2) comparison with AIRS, and (3) spatial scale analysis.

In their revision, I urge the authors to write three different sections on these topics, each with its subsections on "method" and "results". That way, we don't have to slog through a bunch of method descriptions with applications postponed for several pages. I strongly suggest a "describe, use (show Figs., etc.), and move on" structure iterated three times. That would become a much more powerful build up to the truly new and interesting results on spatial scale analysis.

Questions to address on p. 11, eqs (14-16): Exponent in tropics is  $\sim 2/3$ , the classic turbulence value. Nice that the N and S extra-tropical counterparts are very close (as are, to some extent the multiplicative and additive constants). But why the significant difference with classic  $2/3$ ? Same question about the multiplicative and additive constants? Significance of N-S difference in prefactor of scaling term?

Out of curiosity I plotted the fits in Eqs. (14-16) in 2 ways: same axis limits as in Fig. 9, and lowering the  $y_{\min}$  enough to see all. "T" curve looks the same. Something amiss with "N" and "S".

Sequential Comments:

\* p. 3, Eq. (1): Use the conventional "/" once, rather than twice " $\{-1\}$ ".

\* p. 3, Eq. (1): Add the customary subscript " $_0$ " to  $\theta$  for SZA.

\* p. 3, Eq. (2): Coefficient "b" usually precedes the variable ( $\lambda$ ) in the writing of

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a 1st order polynomial.

\* p. 3, l. 10: Probably a good idea to list only water vapor, rather than generic gases, since it is the only one considered. Then assign it explicitly to  $j=1$ , e.g., water vapor ( $j=1$ ); then repeat, but incremented, for liquid and ice water. Then we know exactly what  $j$  is all about.

\* p. 3, Eq. (3), line 1: The first 3 terms are used to fit  $-\log(a+b\lambda)$ ; only two parameters required, and your  $(m-n)$  is one of them. Please make this crystal clear in the math, not just with a vague justification in the following text.

\* p. 3, Eq. (3), line 2: - Fuzzy math: a formal vector of parameters can't be  $\geq 0$ , but each of its elements can. Best to use words in this case. I understand that the fitting algorithm enforces positive values, hence the apparent need to use  $m$  and  $n$  even though one has to be 0 if the other is not. Better to say that both signs are tested for the "ramp" function.

\* p. 4, l. 15: What are the important ETW<sub>x</sub> properties? The outcome of (3) maybe? Please define quantity, not just acronym.

\* Table 1: I'm a bit confused by Land, Vegetation and Desert ... over ocean, but these are just results from the band math in (5-7). Right? Maybe clarify in caption.

\* p. 5, l. 17: Unless ramp slope is redefined earlier, "m" already used.

\* p. 6, l. 1:  $n \rightarrow m$

\* p. 6, l. 30: Do you mean LTF(x)? Rather than denote LTF by  $x$ . See next item.

\* p. 7, Eq. (13): Isn't  $h$  actually  $d$ ? Also, the squared difference is in dependent variables: LTF(x), I believe.

\* p. 7, l. 5: Marcotte (1993)  $\rightarrow$  (1996), also in References (p. 18, l. 27). Very interesting and, for this study, enabling paper, BTW, that I had to look into. That is how I uncovered the apparent error in year of publication.

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\* p. 7, l. 12: What is a "degenerate" variogram?

\* p. 7, l. 14: This and the next paragraph should be a designated subsection on cloud phase discrimination, or something to that effect, having 3 figures to their credit.

\* p. 8, Fig. 2: For completeness, please indicate date and location. And make it visible (different color or arrow?) on Fig. 1.

\* p. 10, l. 4: Profiles  $\rightarrow$  Distributions (since not along vertical here).

\* p. 11, l. 9: exponential scaling factor  $\rightarrow$  scaling exponent

\* p. 11, l. 10: exponential factor  $\rightarrow$  exponent

\* p. 13, Fig. 7: Distill caption down to "As in Fig. 6, but for ice phase."

\* p. 14, Fig. 8: One legend is enough, preferably located in the middle.

\* p. 15, Fig. 9: Why do I see the smallest (maybe Nyquist due to FFT-based estimation?) wavenumber of 90 m? I thought Hyperion pixels were much smaller.

\* p. 15, Fig. 9: Please distinguish or mark the "N" and "S" extra-tropical data and fits. Is N/S offset physically significant?

\* p. 17, Fig. A2: Distill caption down to "As in Fig. A1, but for ice phase."

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Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2017-361, 2017.

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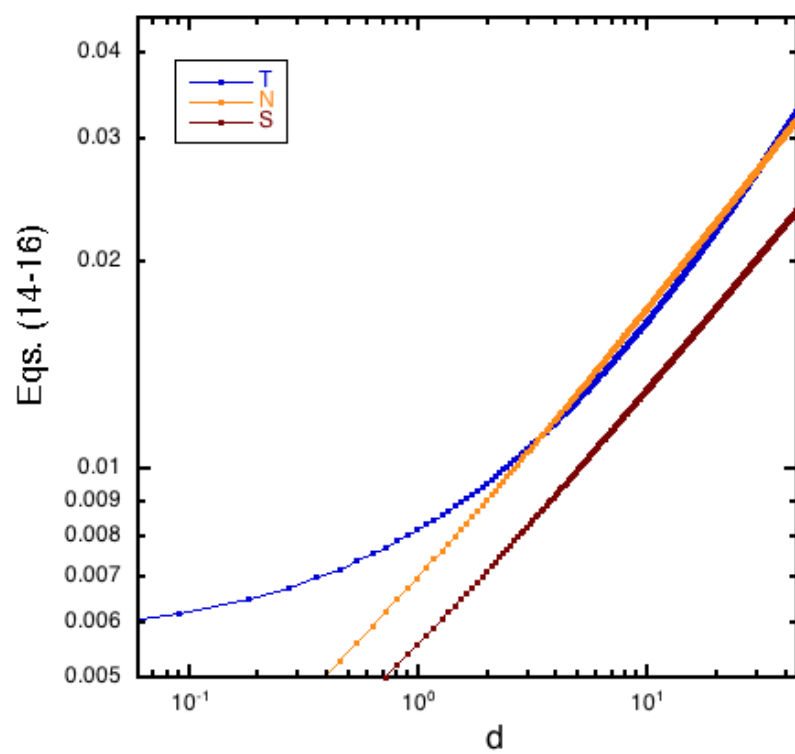


Fig. 1.

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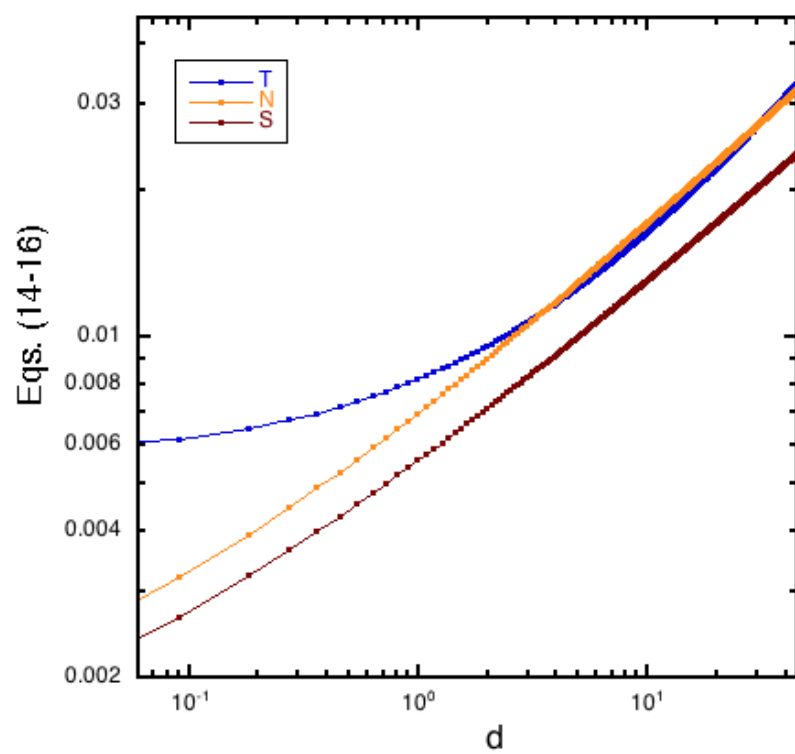


Fig. 2.

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