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Atmospheric Measurement Techniques Discussions

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

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color

1 Reviewer comments

Summary: We agree with all the reviewer suggestions and have incorporated them into a new revision, appended at the end of this document with changes tracked in red. A point-by-point response follows below, with reviewer comments in blue.

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 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.

We reorganized the manuscript following this suggestion. There are now three independent sections on the cloud phase retrieval, AIRS comparisons, and spatial scale analysis. Each has its own method and results. The content does not change significantly, but we added introduction text to present the organization and improve narrative flow. Overall, we agree with the reviewer on this new organization and feel the manuscript is stronger for the change.

Questions to address on p. 11, eqs (14-16): Exponent in tropics is 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?

On significance, there are two related questions: (1) is the difference statistically significant, and (2) is a statistically significant difference physically meaningful? We moved the model equations into a table of coefficients along with 95% confidence intervals to show the statistical significance and better isolate the two questions. We have also added confidence intervals to the curves in Figure 9. The scaling exponents are not significantly different between Northern and Southern hemispheres; their mutual alignment relative to the tropical case demonstrates that even the unevenly-sampled Hyperion dataset shows a significant difference in spatial scaling properties between distinct cloud populations. We added explanatory text, with two new references:

> We report scaling exponents in terms of variance; these could be translated to other conventions using structure functions or the power spectrum domain. Differences between the power law exponents for tropical and extra tropical clouds were statistically significant. Differences between the power law exponents for tropical and extra tropical clouds were statistically significant. The extratropical scaling exponents of 0.42 and 0.44 are similar to, but slightly in excess of the classic Kolmogorov scaling of 1/3 (-5/3 in the power spectral domain). The tropical scaling exponent of 0.62 is in excess of the classic Kolmogorov scaling of 1/3 but is consistent with tropical cloud reflectance variability reported in Barker et al. (2017), and mid-tropospheric water vapor mixing ratio in the tropics from the AIRS instrument, e.g. Kahn et al. (2011). At finer spatial resolutions there is also evidence of scale breaks dependent on 20 altitude. Consequently the scaling exponent is dependent on the length scale range calculated (Kahn et al., 2011).

> The additive offset c defined the variance at zero distance, i.e. the irreducible measurement error for each spectrum. This implied a noise equivalent change in liquid water fraction of approximately 7.5% for tropical and 10-11% for extra tropical clouds. The addend and prefactor coefficients differed significantly between extra tropical and tropical clouds, and to a much smaller degree between the two extra tropical populations. We note that all three populations were subject to the Hyperion datasets' biased sampling of ocean and land, and of instrument noise conditions dominated by solar zenith angles. Consequently we would not ascribe the differences between Northern and Southern Hemispheres to cloud scaling properties, since these are small relative to the notable difference with the tropical population. In all cases, measurement error was a small contributor to observed variance âĂŤ most of the variability arose from spatially-correlated structure. Outside the tropics, measurement error accounted for just 25% of variance observed at GCM grid scales of 100 km. The remaining 75% was therefore attributable to spatial structure at subgrid scales.

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.

This was due to a typo in our transcription of the offset coefficients, now remedied. The scaling exponents, scaling prefactors, chart, overall trends, and conclusions do not change. We thank the reviewer for catching this error.

1.1 Sequential Comments

- p. 3, Eq. (1): Use the conventional / once, rather than twice "-1". Fixed.

- p. 3, Eq. (1): Add the customary subscript $_0$ to θ for SZA. Fixed.
- p. 3, Eq. (2): Coefficient b usually precedes the variable λ in the writing of a 1st order polynomial. Fixed.
- 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. Fixed.
- p. 3, Eq. (3), line 1: The first 3 terms are used to fit -log(a+bλ); 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. Fixed.
- p. 3, Eq. (3), line 2: - Fuzzy math: a formal vector of parameters can't be 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. Fixed.
- p. 4, l. 15: What are the important ETW$_x$ properties? The outcome of (3) maybe? Please define quantity, not just acronym. We now define the quantity as the outcome of the prior expression, e.g. the optical absorption path length in millimeters.
- 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. We removed these confusing labels.
- p. 5, l. 17: Unless ramp slope is redefined earlier, m already used. Changed to ℓ which is unused.

- p. 6,l. 1: n->m We have changed this symbol to ℓ.
- p. 6, l. 30: Do you mean LTF(x)? Rather that denote LTF by x. See next item. Yes, you are right - we fixed this notation.
- p. 7, Eq. (13): Isn't h actually d? Also, the squared difference is in *dependent* variables: LTF(x), I believe. Fixed; thank you for this catch.
- p. 7, l. 5: Marcotte (1993) -> (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. Fixed, thank you.
- p. 7, l. 12: What is a degenerate variogram? We clarified that we excluded scenes with variograms that evaluated to zero at short distances due to the lack of sufficient contiguous cloud pixels.
- 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. Agreed. we made this change.
- p. 8, Fig. 2: For completeness, please indicate date and location. And make it visible (different color or arrow?) on Fig. 1. Done.
- p. 10, l. 4: Profiles –> Distributions (since not along vertical here). Fixed.
- p. 11, l. 9: exponential scaling factor –> scaling exponent Fixed.
- p. 11, l. 10: exponential factor –> exponent Fixed.
- p. 13, Fig. 7: Distill caption down to "As in Fig. 6, but for ice phase." Fixed.
- p. 14, Fig. 8: One legend is enough, preferably located in the middle. Fixed.

- 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. We assume the reviewer means "lag distance" rather than "wavenumber." We used selected lag distances to ensure a log-constant density. The first two bins corresponded to the identity bin which was not plotted, and 90m. Selected bins were dense near zero, and became widely separated at long ranges.

- p. 15, Fig. 9: Please distinguish or mark the N and S extra-tropical data and fits. Is N/S offset physically significant? The plot has been fixed. See the answer above with respect to significance. We emphasize -and have now written into the text- that each datapoint corresponds to a lag distance for the aggregate variogram, so separation between the curves does not imply separation between populations.

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