

# ***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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**Summary:** We agree with all the reviewer suggestions and have incorporated them into a new version, appended as a supplement with changes tracked in **red**. A point-by-point response follows below, with reviewer comments in **blue**.

[This manuscript develops a very useful approach to investigate cloud thermodynamic phase based on data from 2005-2015 obtained by the Hyperion imaging spectrometer](#)

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on EO-1. The approach combines spectrum fitting and spatial scale analysis. The validity is demonstrated with a comparison with AIRS. I recommend the work for publication in AMT after addressing/clarification of the comments listed below.

Page 3, Eq. (3): The introduction/use of 'm' and 'n' is confusing. The development in equations (2-3) should be clarified. What about error due to this approximation?

Reviewer 1 made a similar suggestion. We changed this formula to use a single slope value, which makes the linear continuum more obvious. We clarified that we present both positive and negative slope (one of which must be zero) to the nonnegative least squares solver. Naturally, the notion of a continuum is a convenience, and the cloud particle scattering will invariably depart from a perfect linear relationship. However, linear continua over the 1.4-1.8 micron interval were effective in prior modeling and validation experiments (e.g. Thompson et al., JGR 2016). A relatively simple 5 Degree of Freedom model fit all channels within our conservative noise estimate (see discussion below), suggesting that the model captured the major physical processes in play.

Page 4, line 8-9: 'These bulk absorption spectra were generally independent of particle scattering and did not relate directly to particle size'. Is there a reference or evidence for this statement?

This statement was simply a definition of the Kou et al. coefficients. In other words, the attenuation uses the same absorption coefficients that one would use for a thin sheet or volume of water or ice. We have added more text to clarify: "These bulk absorption spectra were molecular properties of H<sub>2</sub>O, independent of particle size and scattering — a common practice for Shortwave Infrared observations of clouds (Kokhanovsky, 2004)."

Page 5, Eqs. (8-9): Are the "m" and "n" the same variables in Eq. (3)?

Reviewer 1 also noted this repetition; we've switched to a new variable here for clarity.

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Page 5, Eq. (9): [Should] be divided by number of degree of freedom because your fit uses a reduced Chi-Square, which is defined as 'Chi-Square per degree of freedom'. So  $\chi^2 = 1$  is a conservative estimate of measurement noise as shown in Fig.4. Please clarify.

We added text to clarify: "Specifically, [the reduced  $\chi^2$  score was the Chi-square score per degree of freedom, with  $\chi^2=1$  equivalent to estimated measurement noise. This was more appropriate than a classical Chi-square test for ur spectroscopic observations where errors could be correlated across adjacent wavelengths."

Page 11, Eqs. (15-16): I believe that the authors made a typo for the offsets. It should be [0.012 and 0.010]. Please check.

Reviewer 1 also noted this. We have corrected the typo in our revision.

Please also note the supplement to this comment:

<https://www.atmos-meas-tech-discuss.net/amt-2017-361/amt-2017-361-AC2-supplement.pdf>

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