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Interactive comment

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

David R. Thompson et al.

david.r.thompson@jpl.nasa.gov

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color

Summary: We generally agree with 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.

Recommendation: this paper has the potential to be an interesting contribution to knowledge, but requires major revision before being accepted as a publication.

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Discussion paper

The comparison with AIRS is flawed. It appears to be a purely statistical comparison, involving mainly land scenes on the part of Hyperion, and global scenes on the part of AIRS. Given the variability of clouds and the sharp differences between maritime and continental clouds, the AIRS data should have been subsetted to match the Hyperion locations. Mention should be made of the differences (or similarity) between the Hyperion and AIRS sampling: I think that they sample at completely different times of the day? So even if both instruments were retrieving cloud phase perfectly, the comparison would be flawed by the different sampling strategies. The results shown in Figs. 6, 7, A1 and A2 consequently are troublesome to interpret.

We agree the two datasets are different. We would suggest that this is not necessarily a flaw, but rather conditions one must consider in interpreting the comparison. We intend it mainly as a "sanity check" of broad latitudinal distributions, and feel it is independently interesting for the fact that the instruments use very different measurement strategies. However, we agree that the differences are important and our revision calls them out from the start:

> [This] was a dramatically different measurement obtained from thermal infrared spectra with a coarse 13.5 km footprint rather than reflected solar energy at fine spatial resolution. Kahn et al. (2014) detail the algorithm, and Jin and Nasiri (2014) validate it using pixel-scale comparisons with CALIPSO data [....] We anticipated several differences in the result. First, AIRS sampled uniformly over the Earth's surface while Hyperion imaged only during the day and favored land areas. We also expected differences in sensitivity; AIRS was far more sensitive to thin clouds, while the Hyperion analysis intentionally excluded them with a strict cloud mask [...] Additionally, the AIRS algorithm classified ambiguous clouds as "unknown." This population likely contained mixed phase clouds but also a large fraction of supercooled liquid clouds due to the current AIRS phase algorithm [...] While we expected some discrepancies due to differences in instruments

and sampling, the comparison provided a useful check between two very different measurement techniques.

Regarding the comments on comparing at the pixel scale, we agree with the reviewer that this is the most robust method that minimizes temporal and spatial mismatching. However, since the two instruments are in different orbits, and the Hyperion record is limited to targeted acquisitions, this would leave insufficient coincidences to provide robust statistics. A strict spatial distance and time difference criterion would have to be used in order to account for diurnal variability. These issues would persist even if we subset AIRS to the spatial points of the EO-1 observations, without requiring exact temporal coincidence.

Naturally, it is fairly common practice to compare data sets that retrieve the same geophysical variable and make statistical comparisons from completely different satellite platforms and spatial/temporal sampling. This is done with cloud properties including cloud microphysics, IWP, and LWP. One example is Stubenrauch et al., ASSESSMENT OF GLOBAL CLOUD DATASETS FROM SATELLITES: Project and Database Initiated by the GEWEX Radiation Panel, Bull Amer Met Soc, 2013. These types of comparisons still yield scientifically important insights.

Finally, we would emphaisze that the main Hyperion result stands apart as an independent contribution, and that the latitudinal distributions are consistent those reported for more similar instruments such as MODIS, as summarized in Hirakata et al. (2014). The main contributions of the manuscript are to demonstrate the first global scale cloud phase measurement from reflectance spectroscopy, to provide the first global study imaging cloud phase at 30 m spatial sampling, and to assess spatial scaling properties.

They should have addressed the sampling errors of each instrument, not simply a vague error bar for Hyperion and nothing for AIRS. The comparison is also weakened by the empirical correction factors to AIRS data discussed in section 2.5.

Our revision clearly describes our methodology for calculating 95% confidence intervals: "[We calculated] confidence intervals with nonparametric bootstrap variance estimation (Wasserman, 2006) that resampled the dataset 10,000 times with replacement." We now add that "The corresponding AIRS error bars would be far smaller due to the large number of samples, so we omit them for clarity."

The definition of LTF is flawed. The signal measured is based on the absorption of solar radiation integrated over the entire photon pathlength, yet eq. 2 refers only to the thickness of the cloud.

In fact, the reviewer has interpreted our equation exactly as we had intended: the LTF refers to the absorption along the photon optical path, with no implication for the physical vertical dimensions of the cloud. We call it a "thickness" for consistency with prior literature, such as Gao and Goetz (1995). This is also consistent with our own previous usage (e.g. Thompson et al. 2015, 2016).

By their nature, clouds are heterogeneous, so that horizontal variability dominates the radiative transfer process. [This also means that the retrieval technique is at a coarser scale than the postulated 30 m due to the effects of radiative smoothing, and is likely closer to 100 m.] I think this is correctly acknowledged in p.4 line 13 ff. However, it is not really clear whether the LTF is being interpreted correctly. I take it to be the fraction of average photon path that is liquid. Not the fraction of the cloud that is liquid, which would require all paths to extend to the cloud base. An opaque cloud has little transmission, so that most of the reflected paths relate to the top of the cloud. This probably doesn't matter much for the Hyperion retrievals standing alone, but becomes troublesome when compared to other techniques that sample cloud tops differently. It would be good to see a clearer discussion of what is meant by the "effective proxy for thermodynamic phase."

We modified the text to clarify our definition:

In summary, following Equation 3 we modeled the entire interval from $1.4 - 1.8$ μm with five free parameters: a continuum offset l; a slope, represented by a single degree of freedom in the variables m and n; and the vapor, ice and liquid thicknesses u_j. These thicknesses represented the length of the optical path through an equivalent homogeneous volume, as in the Equivalent Water Thickness (Gao and Goetz, 1995). As in previous work, we wrote the absorption path length u_2 as the Equivalent Water Thickness due to Liquid in millimeters, EWT$_{liquid}$. Similarly, u_3 was the Equivalent Water Thickness due to ice, EWT$_{ice}$. We then defined the Liquid Thickness Fraction (LTF) as:

$$\text{LTF} = \frac{\text{EWT}_{liquid}}{\text{EWT}_{liquid} + \text{EWT}_{ice}} \tag{1}$$

Prior in situ validation had demonstrated a robust relationship between the LTF and thermodynamic phase (Thompson et al., 2016). We emphasize that “thickness” referred to the absorption along the optical path; clouds were heterogeneous, so the LTF was not necessarily related to their vertical dimension. In opaque clouds the measurement would be most sensitive to the upper layers.

Section 2.3 is a strange, stand-alone paragraph that seems incomplete. How is ‘dominant’ defined? Greater that 50%? What comparisons were made with historical datasets? This section should be rewritten to provide better context, or incorporated elsewhere.

Following a comment by another reviewer, we have restructured the manuscript to follow a more thematic organization which places this paragraph in better context. We have modified the text to indicate that we used a 50% cutoff threshold.

Section 2.4 presumably refers to the uncertainty in determining the LTF of a single scene, but this is not clear. It also stops abruptly with no relation to the esults. This needs to be rewritten for clarity and context.

We agree; our manuscript restructuring clarifies the implication of these χ^2 values. The fits demonstrate that the model explains the variability observed in the spectra to within our noise estimate, showing that the retrieval method of Thompson et al. (2016) also applies to Hyperion. We have added text to this effect. We also state that noise is calculated on a per-line basis (it is dominated by constant factors like the solar zenith). However, we calculate χ^2 statistics independently for each spectrum, since we fit a model independently for each 30 m $\times$ 30 m spatial location.

Section 2.5 should provide a reference to how AIRS obtains cloud phase and whether this has ever been validated.

We now state: "This was a dramatically different measurement obtained from thermal infrared spectra with a coarse 13.5 km footprint rather than reflected solar energy at fine spatial resolution. Kahn et al. (2014) detail the algorithm and and Jin and Nasiri (2014) validate it using pixel-scale comparisons with CALIPSO data."

The use of the word 'trends' p.5, p7, p.13. This is better reserved for long term climate change. Here we are looking at 'relative dependence on latitude' or similar.

We substitued the term "distributions."

Fig. 4 shows results for clear retrievals, yet the scene looks completely overcast. Are these all in error, despite the low values of χ^2 for many of these? Given the range of χ^2 shown, presumably the only results retained where when χ^2 was less than some threshold? This could be discussed better.

We understand how the clear sky case could be confusing. In fact, the scene is a fragment of a larger image that included clear sky areas, and these statistics come from the clear parts (which are outside the area shown in our figure). Ironically, the χ^2 values for the clear cases had been low despite the fact that the instrument saw the Earth's surface. The image was acquired over an ice shelf and ocean, and the former showed ice absorption while the latter had nearly zero reflectance in this range. Since we do not include clear sky cases in our global statistics (for obvious reasons) there is no reason to report these χ^2 values. For clarity, we have removed the case from the histogram and the text.

Fig.5 is too cryptic for the typical reader. If the vapor transmittance around 1.4 μm is zero, how can there be any reflectance to work with? Does the theory include the vapor paths both above and within the cloud? Probably need to explain what is meant by transmittance in this context.

Thank you - this was a good catch. It was an artifact of our figure, which had used the first order Taylor approximation of the transmittance. The approximation was not accurate near saturation. We replotted the figure using the true transmittances, which were naturally greater than zero. The product of plotted transmittances (and the continuum, not shown) now reproduces the observed TOA reflectance. The changes are very subtle outside the 1.4 micron vapor feature, and the general shapes and relative depths are not significantly altered.

Normalization of occurrence: p.6, l.11 is -60 to +60°, Fig. 6 is 0 to 60°. Which is it? Is the normalization done separately for each cloud phase? Are the AIRS data similarly normalized?

This was a typo in the figure caption; the normalization uses the whole -60 to +60° interval. We updated the caption, and changed the text to emphasize that both AIRS and Hyperion are normalized.

Fig. 8 is flawed by the nonuniform sampling with latitude. Perhaps an indication of the relative number of samples per histogram would help.

We have added a note to the caption and text reminding the reader that sampling is nonuniform, and that the results should be evaluated in light of the bootstrap uncertainty analysis. This figure is presented in the context of Figures 6-7, where bootstrap variance estimation shows the uncertainty due to sample size in each histogram bin.

p.10 line 9. Appendix 4? Appendix A.

Fixed, thank you.

Fig. 9 shows NH and SH curves for extra tropical clouds, but which is which? Eq. 14-16 don't seem to match the values on the figure.

We have remedied a typo in the offset values for these equations - now they match the figure. We have also changed the figure, labeling the two extra tropical curves.

p.13, line 4. This caveat comes far too late in my opinion as it dominates the comparison throughout. Note that CALIOP also offers high-resolution phase information that also has fewer sampling limitations.

The new version emphasizes nonuniformity throughout, in the following locations:

- Section 2.1: "[Hyperion] performed targeted acquisitions for specific regions of interest, with occasional pointing off nadir. Most targets were on land, with a high concentration in the mid-latitude northern hemisphere. There was sparser coverage of extreme latitudes and oceans, but several island targets offered a view into cloud systems over ocean (Figure 1). Many targets of interest were revisited multiple times during the mission."
- Figure 1: portrays the Hyperion image locations
- Section 2.2: "Note that Hyperion sampling is nonuniform across 20 histogram bins, and Section 1 quantifies uncertainty for different latitudes."
- Section 3.1: "AIRS sampled uniformly over the Earth's surface while the Hyperion dataset favored land areas."
- Section 5 (conclusions): "The Hyperion datasets were spatially biased and strongly favored land mass over ocean. Insofar as the latitudinal trends show

> asymmetries across northern and southern hemispheres, this may be related 15 to the spatial distribution of land mass in the southern hemisphere midlatitude areas. Southern hemisphere observations were often acquired over islands, which would exhibit a more oceanic influence on cloud cover."

We believe that the closing discussion is an appropriate place to contextualize these results and draw implications. Obviously the Hyperion mission was not designed for cloud observations. However, the ability to form cloud phase maps at 30m resolution, is a unique new capability that makes the investigation meritorious. Additionally, results generally agree with existing cloud phase records from AIRS (and multi-instrument comparisons such as Hirakata et al., 2014). Because the measurement technique is distinct, is is a useful complement to other instruments using polarization (CALIOP) or thermal emission (AIRS). We have modified the conclusion to emphasize this. Finally, the Hyperion datasets provide a "first of a kind" observational record at sub-kilometer scales. Spatial granularity reaches a factor of three below CALIOP observations, though one should also note the reviewer's caveat about within-cloud scattering placing a lower limit on achievable resolution.

Please also note the supplement to this comment:
https://www.atmos-meas-tech-discuss.net/amt-2017-361/amt-2017-361-AC3-supplement.pdf

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