

Interactive comment on “Toward autonomous surface-based infrared remote sensing of polar clouds: Cloud height retrievals” by Penny M. Rowe et al.

Anonymous Referee #2

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This manuscript presents a detailed set of calculations to explore the capabilities of hypothetically-deployed surface infrared spectrometers to measure cloud properties including cloud heights. The paper explores different retrieval algorithms and instrument specifications including spectral resolution and noise. The paper finds that cloud heights can be retrieved from autonomous infrared spectroscopic observations.

The paper may be acceptable for publication in AMT, but some key issues need to be addressed first.

1. One of the central challenges to low-cloud height retrievals with infrared spectra concerns errors introduced from the lack of knowledge of the temperature profile. Under cases where there is a strong but unknown inversion, temperature profile uncertain-

ties can be significant. This needs to be explored throughout the paper and discussed more prominently in the paper.

2. Where does the research go from here? How can the myriad assumptions made in this scoping study be relaxed to get more realistic estimates of the cloud height retrieval performance of autonomous infrared instruments? Can this be confronted with real data?

Below are some additional minor comments:

A more appropriate manuscript title would be "Scoping studies in support of autonomous surface-based infrared remote sensing of polar clouds: cloud height retrievals"

Page 1, Lines 14-16: Perhaps there is a type-o. I do not understand what is meant by a retrieval accuracy of -2 ± 2 km for high clouds and $\sim 0.2 \pm 0.5$ km for low clouds

The final sentence of the abstract is weak and does not leave the reader motivated to consider these instruments as a value-added proposition to existing instruments, which is what I believe the authors are trying to convey.

Page 2, Line 17: explain the purpose of monthly or seasonal average of small footprints

Page 2, Line 32: Are the authors referring to the AWARE campaign? If so, they should say so.

Page 4, Line 9: The modeling of ice as spheres could be a major assumption. This needs to be justified.

Page 5, Line 13: The use of lowercase 't' for transmittance is non-standard.

Page 6, Line 16: Also include the assumption of ice clouds as spheres.

Page 7, Line 2: Doesn't this technique rely on the accurate knowledge of the temperature profile, not the CO₂ profile? Will variations in CO₂ be so significant as to affect

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the results?

Conclusions: The figures suggest that MLEV does not incur biases at the same level as CO₂ slicing. Why is that? Statements regarding the utility of MLEV should be made in the conclusions.

A figure is needed to show what actual downwelling infrared spectra look like and their sensitivity to cloud height.

Interactive comment on *Atmos. Meas. Tech. Discuss.*, doi:10.5194/amt-2016-49, 2016.

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