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Atmospheric Measurement Techniques Discussions

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

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## 1 Overall Response

**We have made major changes to address the reviewer's comments, as detailed below. These include specific changes to address the reviewer's comments, and, because we felt that the purpose of the manuscript was not clear to the reviewers, extensive changes were made that we hope greatly improve clarity. Overall, nearly every figure was altered or replaced, existing tables were modified and several new tables were added, almost a page of introductory text was removed, and several pages of new text were added. Because of the increased length of the document, much of the existing text was modified to be more concise and remove unnecessary text, some reorganization was done, and three figures (previously Figs. 3-5) were removed. Reviewer's comments are included below, and responses follow in bold.**

## 2 Detailed Responses

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

**We have made major revisions in response to this comment. We have added retrievals for cases in which the temperature inversion is not captured in the temperature profiles used for the retrieval (now Fig. 4g), and added discussion of temperature inversions in Section 2.4, as well as a paragraph in Section 5.5 discussing this source of error.**

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?

**Yes, next steps include doing retrievals from real measurements – although the proposed instrument does not exist, so this will need to occur with a prototype,**



**or existing measurements will have to be modified to emulate those expected (e.g. noise and resolution degraded to varying levels). We have added text to address this in the discussion (Section 5.5) and in the conclusions. We have further made changes throughout to emphasize the connection between these studies and instrument characterization (see response to Reviewer I).**

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"

**We prefer our current title, which implies that the instrument does not yet exist, especially given Reviewer I's comments.**

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 +/- 0.5 km for low clouds

**We have modified the abstract to be clearer, instead giving the average error. The error was previously given in the abstract as the bias error (mean error) and the standard deviation of the error, but this is not described until the main text, so we removed it from the abstract.**

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.

**We have modified the sentence, and thank the reviewer for this point.**

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

**The text now reads, "Active instruments, such as lidar . . . have a small footprint, so that monthly or seasonal averaging is needed for global coverage."**

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

**Yes. The text now reads, "... the Atmospheric Radiation Measurement (ARM) West Antarctic Radiation Experiment (AWARE) is making a broad suite of measurements from November 2015 to 2017."**

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

**We have calculated new simulations and retrieved cloud height for clouds with a variety of ice habits. Results are given in Table 4 and discussed in context in Sections 2.3, 4.3, and 5.3. See also our response to the comment below regarding "the assumption of ice clouds as spheres".**

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

**We follow the convention in Mahesh et al. 2001, upon whose formulation our method builds.**

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

**This is not an assumption of the retrieval (but habit is specified for the cloudy-sky simulations, which now include multiple habits). The retrieval itself does not require cloudy-sky simulations, but only calculations related to the clear sky. Where habit comes into play is in how it affects the emissivity variation with**



**wavenumber (an assumption of slowly-varying or constant emissivity is made). A discussion of this is now included in Section 5.3 in relation to retrievals for simulations for different ice habits, including, "... the retrieval does not require any a priori knowledge or assumptions about ice habit, but rather relies on the assumption that the emissivity is constant or varies slowly with wavenumber, thus ice habit affects cloud property retrievals only inasmuch as it alters the frequency-dependence of the cloud emissivity."**

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 the results?

**We agree. Error in the CO2 profile has little affect on the results. We have removed this sentence as to avoid confusion.**

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.

**That is not true in general. It depends on the resolution and source of error. For example, the figures show cases for which biases are larger for MLEV and cases for which they are smaller. Overall, MLEV incurs greater biases in the presence of instrument noise; we have had discussion emphasizing this point. The utility of MLEV is discussed in the conclusions where errors are summarized. In addition, because of the differing sensitivities of the two methods, an approach that combines the two methods is recommended. Note that we have added retrievals for estimates combined errors (see responses to Reviewer I), and for these MLEV incurs greater biases.**

A figure is needed to show what actual downwelling infrared spectra look like and their

sensitivity to cloud height.

**This figure has been added (see the new Fig. 2).**