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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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11

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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 #1

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General

The paper is carefully designed, elaborated, and focusses on an important issue in the field of continuous atmospheric monitoring capabilities.

I am not an expert for passive IR remote sensing. I am an expert for active remote sensing. So from the 'real-world profiling' point of view, I simply miss comparisons with real-world cloud base observations (performed with the proposed infrared radiance spectrometer and, e.g., a ceilometer, side by side, over hours, days, months). Is there no facility (somewhere around the world, at midlatitudes or at polar latitudes) where such an observational configuration is given. . ..? If there are test versions of the proposed type of spectrometers, why is there no attempt to put it close to a lidar or ceilometer?

To our knowledge, the proposed instrument, or test versions of it, do not exist. Retrievals from similar instruments (at a resolution of 0.5 cm-1) have been performed and compared to lidar and are referenced within this work. Additional real-world retrievals are an important topic for future work. We have added text to address this in the Discussion (Section 5.5) and in the Conclusions.

Even the most complex and comprehensive error analysis is not really convincing (at least to me). Long test measurements and comparison with alternative approaches (e.g., cloud height observations by means of active remote sensing) are convincing, only!

We agree. However, our purpose is not to convince the reader that infrared cloud height retrievals are worthwhile, as this has already been done extensively in the

literature. The retrievals described in the manuscript are designed to inform development of a new instrument and are, therefore, a “proof of concept". Since ground-based low-resolution, low-power infrared instruments are currently not used for cloud retrievals, this paper focuses on a theoretical study that investigates the errors with a hypothetical instrument. Our purpose is to use simulations to investigate how individual sources of error affect measurement accuracy, which is complicated in the real world by the simultaneous presence of multiple error sources. Retrievals from existing instruments cannot provide a careful examination of the effects of individual sources of error (instrument bias, noise, errors in atmospheric temperature, etc) on retrieval accuracy because individual errors cannot be easily separated in field measurements. This work is novel in that it uses simulated radiances to probe the effects of particular errors and determine the degree of accuracy that may be achievable for certain instrument characteristics. The results provide useful and novel information to the retrieval community and help to determine whether the more limited capabilities of a hypothetical autonomous (yet low power) infrared spectrometer are sufficiently suitable to justify development of a prototype.

To make our purpose more clear, we have added clarity regarding the choice of data in the Abstract and Introduction, and we have made major revisions to the text, including placing greater emphasis on sensitive studies at a likely resolution for the proposed instrument (4 cm-1; see the new Fig. 5), rather than at the resolution of existing instruments (0.5 cm-1), and we have revised Figs. 4, 5, and 6, as well as Tables 1 and 2 to show estimates of combined error budget (based on retrievals rather than propagation of individual errors).

If the assumed water vapor profiles have a very sensitive influence on the retrieval, I would like to see simulations with the worst cases (completely different humidity profile structures, not just a simple height-constant systematic bias for the true one. . ., or did

I miss this?, and such a case is give in the article?).

Yes. In addition to 3% errors, we show 10% errors, and we show retrievals for a height-constant systematic bias, used to represent the worst-case scenario because biases that change in sign with height will partially cancel out. Because water vapor decreases approximately exponentially with height, and because the instrument is more sensitive to water vapor closer to the instrument (at the surface), biases near the surface are more important. These uncertainties are discussed in Section 5.5 and shown in Tables 1 and 2.

What structure of cloud base did you assume? Just a simple temporally constant cloud base (a very sharp edge or increase in terms of cloud drop extinction coefficient at cloud base)?

We assumed temporally constant, sharply defined cloud bases with approximately constant optical depth within the cloud. This is now made clear in a new section: Section 2.1 Base dataset.

Did you also simulate a slowly increasing extinction coefficient from base to the inner part of the dense clouds so that the cloud base is badly defined?

No, the optical depth was held constant through the cloud layer. We agree that this is an important consideration and thus we have made major revisions to address this comment. First, we calculated new simulations with the optical depth varying through the cloud layer, which now form part of an extension to the base data set (See Section 2.2 Subset: cloud inhomogeneity). These simulations are then analyzed alongside the constant optical depth version (all other properties held constant) in new Sections 4.3, 5.3, and Table 3.

Did you simulate clouds. with strong virga, weak virga, some drizzle below the cloud base, which may not allow a proper retrieval?

Below-cloud virga has been modeled in the literature as ice cloud below the main cloud layer. We have included major revisions in response to this comment by including in the data subset cases that include liquid clouds with ice below (a common cloud type in the Arctic from which virga may form). The results from analysis using these new simulations are discussed in Sections 2.2, 4.3, and 5.3 and in Table 3.

Cloud base heights depend on updraft-downdraft characteristics (and sometimes on well organized wave motions). Thus, cloud base can vary with time (within seconds and minutes). So, did you also play around with temporally changing cloud bases, or even horizontally inhomogeneous cloud bases (in the field of view of the spectrometer)?

We have made major revisions to address this comment. We added simulations of clouds with optically thinner cloud top and bottom edges and with top and bottom edges removed. Such spectra were then averaged to simulate time-averaged spectra such as what would be observed by averaging over a long time period. A horizontally inhomogeneous cloud is expected to produce similar spectra. Results from these retrievals are presented and discussed. (See Sections 2.2, 4.3, and 5.3 in Table 3)

All the questions should be answered, and as you see, at the end you need long-term comparisons with continuously running ceilometers (or something else) to convince us (scientists) that this a reliable and useful approach what you propose.

We have made major revisions to answer these questions, as described above. We would like to emphasize that we are not trying to convince the reader that

cloud height retrievals are a reliable and useful approach, as there is a large body of literature on the subject (particularly for satellite-based retrievals). We hope the changes have made the purpose more clear.

Some details:

The introduction is long and cumbersome. I always prefer a short introduction, just briefly mention the importance of the field, the gaps in the field, your contribution, and an outline of the paper (sections 2,3,4. . .).

This is a good point; we have shortened the introduction by approximately one page.

Section 4 (Results): Because this is the starting point and an essential issue: Please show your true cloud base height scenarios in a figure, may be in terms of the cloud extinction coefficient for a visible (500 nm) and an IR wavelengths, you are using!

True cloud base heights are given in Fig. 1a; we prefer not to add this additional figure as it can be briefly summarized. The simulations were not performed in terms of an extinction coefficient, but rather the visible optical depth. Visible optical depths for the simulations are described by Cox et al. 2016, referenced here. They varied from 0 to 12 and were drawn from the same distribution for all cloud heights (see Fig. 7b of Cox et al. 2016). Approximated as the visible optical depth divided by the cloud physical thickness, visible extinction coefficients vary from 0 to 0.01 m-1 near the surface and 0 to 0.001 m-1 above 4 km. We have added text similar to the above in response to this comment.

And then may be show different retrieval products (retrieval scenarios in addition).

Retrieved cloud heights for the base dataset and the new subsets created in response to Reviewer's comments are shown in a variety of figures and tables.

Section 5 (Discussions): Here I began to ask myself, what does all these discussions help, without any comparisons with cloud height profiling tools, showing the reaility, ... real clouds with holes, with inhomogeneous cloud base structures etc.

The discussion is meant to help explain the following which do not require reference to cloud height profiling tools: 1) How this analysis improves on the error analysis in prior work. Note that the prior measurements did include comparison to a lidar, but simulations are needed nevertheless to provide an error analysis propagating sources of error to uncertainties in retrieved cloud heights. 2) How sources of error affect whether CO2 slicing/sorting or MLEV produce more accurate results, and benefits of combining the methods. This also cannot be determined from comparison to cloud height profiling tools. 3) How instrumental spectral resolution affects the retrieval. 4) How instrument characteristics should be fixed for a portable infrared instrument that could be deployed where active instruments cannot (due, e.g. to power constraints). Here, this is done through an extensive error analysis of some of the major sources of error because it is not feasible to develop a wide variety of instruments to determine in field experiments which set of instrumental error characteristics is ideal. To provide additional insight into real clouds, we have made extensive revisions to address the Reviewer's questions posed above.

So, if possible, please provide at least few comparisons with ceilometers, or at least provide a broader discussion on the need of real-world comparisons, because of the reasons, I mentioned above.

These comparisons will indeed be important work, but are left to future studies.

The work presented here makes important strides towards development of the proposed instrument and also provides a novel perspective on infrared retrievals of cloud heights that we believe will inform the broader infrared retrieval community of some of the uncertainties that more conventional comparisons are unable to isolate.

Figure 1: Cloud temperature is not very specific! Cloud base, center top temperature? What do you mean?

Good point. It is cloud mean temperature. We have modified the figure caption.