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Interactive comment on "A synthetic data set of high-spectral resolution infrared spectra for the Arctic atmosphere" *by* C. J. Cox et al.

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We thank both Anonymous Referees for their thoughtful comments, which we have used to improve both the manuscript and the data set. We are appreciative that both Referees believe the idea is novel and useful. The main concerns expressed by the Referees regard which simplifications should be made in modeling the clouds and what realistic aspects should be emphasized. Specifically, both Referees indicate that the clouds should be made more complex to be more realistic. The clouds could be made more realistic in a great number of ways, including those suggested by the Referees and a wide variety of others. As stated in the manuscript, there are two advantages to creating a simulated data set: (1) the cloud properties are knownÂăa prioriÂăand (2) the assumptions are controlled. Regardless of the complexity of the simulations, the first advantage always applies. The second advantage requires controlling cer-

tain assumptions in order to test others in isolation. Thus, a data set such as the one described could be made to include many combinations of simplifications while emphasizing the realism in a variety of other characteristics. How this is done depends on the questions the investigator wishes to answer. We chose to keep the clouds simple to test only the characteristics we felt were the most important for typical infrared retrievals, based on what has been reported in the literature. However, we recognize that this limits the ability to determine errors due to such simplifications themselves. Therefore, we are calculating new simulations following the suggestions of the Referees; these new data will be uploaded to the public archive to complement the base data set. We have updated the text accordingly and also added more discussion about the objectives of the data set to provide the context similar that provided in this response.

General Comments, Referee #1 "This manuscript reports a dataset of infrared spectra calculated with RT models for cloud retrieval applications. The objective of this study is good. The idea and method is straightforward. The presentation is clear. However, this study, except the amount of effort, is weak in reporting something new. The measurement data and RT models shouldn't have problem since they are widely applied. But the treatment of the data and the assumptions in modeling for the dataset are questionable. The examples for these drawbacks are listed below. Based on these concerns, this reviewer cannot recommend this manuscript be published at current status. Major revision need be considered."

We have made major revisions in response to this comment. See our specific responses below.

Specific Comments, Referee #1: "1. In Abstract, "Retrievals of cloud ... from ... infrared remote sensing ... are critical for understanding clouds." Why infrared is critical? For polar regions, sunlight problem?"

We have rewritten the statement to add clarity.

"2. In Introduction, the authors discuss the difference of retrieval results for clouds from different measurements, and say the difference is largely due to perspective and measurement sensitivity between different sensors. Is this true? Instrument idea (Is an instrument specifically applies a suitable physics/technique in remote sensing a specific target?) and retrieval algorithms, how reliable they are, are also or even more important here."

We agree. It is understood that perspective and measurement sensitivity are substantial sources of uncertainty, yes, but as you say the algorithms themselves are also a substantial source of uncertainty. The purpose of the described data set is to provide a platform that accounts for the former (perspective/measurement uncertainty) so that the latter (algorithm error) can be more easily isolated. We have modified the text in several places within the Introduction to make this point more clear.

"3. Assuming clouds, no matter ice or water, are composed of spherical particles in modeling, this may have significant effect on ice results. A sensitivity study need to be done for this treatment."

This is a good point. In response, we are adding 30 new simulations to the data set that use the same cloud and atmospheric properties as those already available, but include other more complex habits for ice (rough & smooth plates, rough & smooth columns, and bullet rosettes). The selection of the habits is based on those observed in clouds and typically used in infrared retrieval studies. The optical properties use published scattering databases (Yang et al., 2005, 2013); the authors also report the sensitivity of emission across the infrared spectrum to various ice habits; we have made note of this in the text. We believe the dataset and manuscript are greatly improved by this addition, and thank the reviewers. These additional spectra allow the dataset to be used to characterize the importance of ice habit. We have also included text in the manuscript indicating that we can provide additional spectra for ice habits of interest on request. We have also retained the original set of spectra. Infrared retrieval algorithms exploit differences in the frequency dependence of the absorptivity of liquid and ice to distinguish phase. Thus, the algorithms are expected to be sensitive to

phase independently of particle habit and size. It is therefore useful to model ice and liquid similarly to evaluate, at first order, the ability of the algorithm to distinguish phase.

"4. For cloud particle size 10 micron for water and 25 micron for ice, are these true in polar region?"

The values used are reasonable for the Arctic and are drawn from the results of Turner (2005), as noted in the text.

"5. Phase partitioning 1:6 and 2:3. Give the reason for these settings."

The reason is a matter of practicality in providing a data set designed for solving problems most relevant to retrieving properties of Arctic clouds. This reason is given in the text following the introduction of the phase partitioning ratios.

"6. The manuscript lacks validation"

The radiative transfer algorithms used to create the simulations are thoroughly documented and are the state-of-the-art algorithms used throughout the research community; relevant publications are referenced in Section 2.

General Comments, Referee #2 "This manuscript describes a synthetic dataset of high-spectral resolution infrared spectra for Arctic atmospheres. The objective is to provide such a synthetic dataset so that developers of retrieval algorithms can use it for evaluation. The manuscript is well written and structured. The objective is very useful. Unfortunately, although great care is taken to use realistic assumptions for some aspects of the simulations, for some other aspects questionable and seemingly unneeded assumptions are made. These assumptions defeat the purpose of supplying realistic synthetic measurements. It seems likely that potential retrieval algorithms have more realistic assumptions in them. Thus, I advise this manuscript to be published only after major revisions to the dataset and the paper. Below I list my three specific major concerns and a few minor suggestions."

We have made major revisions in response to this comment. See our specific responses below.

Specific Comments, Referee #2: “1) Page 6: Mie calculations are used for ice crystals, which will lead to unrealistic phase functions. It is stated that “spheres were specifically used for ice to simplify the dataset and any associated retrievals.” As said in my introduction, retrieval algorithms may use more realistic ice optical properties. Although light scattering may be expected to have a relatively small contribution to the IR spectra, I think it is not well known, or at least not discussed in this paper, what the expected errors from using Mie calculations for ice are for IR simulations. In line with the objectives of this dataset, more realistic ice optical properties need to be used. For example, I suggest using ice optical properties from the Yang et al. group (Yang et al., J. Atmos. Sci., 2013 ,doi:10.1175/JAS-D-12-039.1).”

We agree. In response to this comment, we are adding 30 new simulations that use optical properties for several realistic habits (provided by Dr. Yang and colleagues), and have added text stating that additional calculations are available on request. Please see also our general response and response to Referee #1, Comment #3.

“2) Page 10: It is stated that the highest clouds in the dataset are unrealistically thick. It is stated that this was done for “simplicity, consistency and computational efficiency.” Again this seems to defeat the purpose of this dataset, and retrieval algorithms can be expected to have more realistic assumptions in them. Since these computations need to be run just a single time and not repeatedly as in a retrieval algorithm, computational efficiency seems not to be a priority, nor does “simplicity”.”

We agree with this statement. However, on review of the literature, we find that the modeled high clouds are not necessarily unrealistically physically thick. There is a lack of support in the literature for our assumption that high clouds must be thin. While precise statistics are unavailable, physically thick ice clouds have been observed in the Arctic (e.g., Miller et al. 2015) and the distribution of all cloud thicknesses in the data set is in agreement with the literature (Shupe et al. 2011). We have fixed this error in

the manuscript. In addition, we have conducted a sensitivity study using the clouds in the data set that span multiple grid layers, comparing the calculated radiance (mW(m2 sr cm-1)-1) difference between assigning the cloud entirely in the center “cloudy” layer (optical depth, τ, is zero at all other cloudy layers) and distributing τ across the cloudy layers (as in the data set). This is equivalent to re-gridding the vertical coordinate to produce physically thinner clouds. Amongst the 104 applicable cases, the maximum sensitivity at 900 cm-1 (a microwindow that is particularly sensitive to clouds) is -0.8 to 3 mW(m2 sr cm-1)-1, which is small. It is important to note, however, that this sensitivity is not an expression of differences in the physical thickness, but rather the difference in the temperature (and ΔT) across the two different layer configurations (the physical thickness is not actually known to the radiative transfer calculation). That is, the different configurations are entirely different cloud cases, any of which are equally valid candidates for testing.

“3) It is a bit unclear whether all assumed cloud properties are assumed to be vertically invariant, but this seems the case. All real clouds, and certainly Arctic mixed-phase clouds are vertically inhomogeneous (e.g., Shupe et al. J. Geophys. Res., VOL. 106, 2001). For example, ice sizes generally decrease with height in all ice containing clouds, while drop sizes generally increase with height. Often tops of mixed-phase clouds are dominated by liquid, while ice dominates further below. All of these structures are expected to have substantial effects on retrievals that need to be quantified. One objective of the paper is to provide a dataset for evaluation of retrievals from different viewing geometries and such vertical structures are likely to have very different effects on measurements from either the ground or space. Thus, neglecting these structures leads to a dataset that is not very realistic and its usefulness for evaluation retrieval algorithms is questionable.”

Yes, the cloud properties in the data set are generally vertically invariant. We have clarified this point in the Introduction. In response to this comment, we have added cases with vertically inhomogeneous optical depths. We have also added cases with liquid-topped clouds, changing the layering such that a physically thin liquid layer lies above a physically thicker ice layer, similar to the clouds described by Shupe et al. This provides a case that is otherwise identical to a vertically invariant mixed-phase cloud through which retrieval uncertainty can be traced. It will be uploaded to complement the base data set. This modification is analogous to the sensitivity study described in the previous comment (#2) and the resulting radiance differences are similar as well. We feel that further levels of complexity are beyond the scope of the present work. There are many spatial heterogeneities in real clouds, and simulated clouds can be made more complex in many ways, including but not limited to those listed here. Thus, we have added a statement in the conclusion of the text that the framework for calculating the simulations is in place and that we would be willing to assist interested researchers in customizing data for their specific applications.

"A few minor comments: Equation 1: Please give a definition for effective radius as used here."

Done. Two additional references are also provided.

"Page 6, line 25: I believe "extinction cross section" needs to be "extinction efficiency"."

You are right. Thank you for catching this error.

"Page 6, line 26: Are both real and imaginary parts of the refractive index temperature dependent? Please clarify in the paper."

Yes. We have added clarity.

"Figure 1: I suggest to only plot the 522 cloud-containing profiles in this plot, since the clear-sky cases are not representative for this dataset."

The plot is already as suggested.

Other changes made by the authors: 1) We have updated our acknowledgements. 2) We have made a minor change to the presentation of equation 2. It is mathematically

equivalent to its original form, but the modified form improves clarity. 3) We have added a relevant reference to the introduction (Bulgiaro et al. 2011). 4) We have edited the to Reference list and in-text citations to correct formatting mistakes.

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