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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Evidence of ice crystals at cloud top of Arctic boundary-layer mixed-phase clouds derived from airborne remote sensing" *by* A. Ehrlich et al.

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This is a fascinating paper and from this reviewer's perspective, the first-of-its-kind to use optical remote sensing to infer the vertical structure in mixed phase clouds. The paper reports on results from the ASTAR 2007 field study of Arctic boundary layer mixed phase clouds. The study was conducted over the Greenland Sea in early spring, 2007. The data presented are from a single day during the mission, on 7 April 2007. An airborne platform was instrumented with situ instruments (polar nephelometer, Cloud Particle Imager, and FSSP-100) and a shortwave radiometric package called the Spectral Modular Airborne Radiation measurement sysTem (SMART-Albedometer) which acquired downwelling and upwelling spectral irradiance, and nadir spectral radiance. The following is a brief summary of what I interpreted to be the main points.

With the focus on a single case, an effort was made to use the measured microphysical properties in a radiative model in order to test the ability to reproduce the observed scattered radiation and test the fidelity of the in situ cloud profiling to represent the appropriate vertical cloud structure. The finding from the in-cloud probes was that the upper half of the sampled cloud (1200–1600 m) was characterized predominantly by liquid water droplets with mean effective diameter of 15 μm and total optical thickness of 7.5. The bottom half of the cloud contained mostly ice crystals with effective diameter 103 μm and optical depth 0.5. Below cloud base (800 m) precipitation was observed.

When these properties were input into a radiation model it was determined that the total cloud optical thickness was incapable of reproducing the measured cloud reflectance in the conservative scattering spectral domain, that is, where the absorption by liquid water and ice is negligible. The authors adjusted the cloud optical thickness, keeping effective particle size fixed, to match the conservative scattering reflectance, but this adjustment overestimated reflectance in the near-infrared spectral regions where condensed water absorption occurs. (fig. 4)

To fix this mismatch the authors adjusted the relative water-ice optical thickness fractions (fig. 5), from no-ice optical thickness to all-ice optical thickness, yet no simulated spectrum was capable of matching the measured reflectance in the near-infrared. In my opinion, fig. 5 is the most enlightening figure of the paper: it shows the clear signatures of ice and liquid water cloud reflectance, and various mixed-phase signatures in the intervening regions between these two ideal (homogenous) limits. Clearly the measured spectrum in this instance does not adequately match any of the simulations. This level of disagreement would be difficult to explain on the basis of radiometric imprecision because such instruments are highly reliable in their wavelength to wavelength relative accuracy, the most important requirement in the present context.

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On the basis of the inadequacy of figure 5 to resolve the measurement-model comparisons, the authors invoked the spectral dependence of the vertical weightings of cloud reflectance to conclude that more ice in the upper cloud layer might explain the measured cloud signature. The contribution to cloud reflectance is weighted more toward upper regions of the cloud with increasing condensed water absorption. For this reason, the slope of the reflectance spectrum in the 1500-1700 nm band for a mixed phase cloud will be determined by the relative vertical position of water and ice in a mixed phase clouds. It was this slope which could not be reproduced in any of the simulated spectra in figure 5.

Based on this analysis, the authors conducted two additional simulations, one with a mixed-phase layer at cloud top and another with a pure ice layer at cloud top. In both cases the added ice in the upper region of cloud had optical thickness of 0.5. The calculated reflectance spectra from these two models were able to much more closely reproduce the measured spectrum, at least within measurement uncertainty, if not perfectly matched at all wavelengths.

An additional piece of information was used to test the validity of their conclusions that ice was in the upper cloud layer, in contrast to the microphysical measurements. A glory was observed in the flight over this cloud. The glory results from the back-scattering from spherical particles. Was the inferred ice in the upper cloud region sufficient to eliminate the glory in the reflected radiation? The analysis provide in the text concluded that the glory would still be observed in the mixed phase cloud layer assumed for fig. 9. That is, the presence of the glory was not by itself sufficient to eliminate the possibility that ice was present near cloud top.

As hard as I tried, I could not find any major issue to counter the authors' conclusions. However, I do have some comments that I feel need to be better addressed. I am not surprised that the in situ measurements could not be reconciled with the cloud reflectance measurements. I was a little surprised that the cloud optical thickness had to be more than doubled to match the visible reflectance, but I don't think it is necessarily unrealistic. The reason provided in the text, sampling mismatches caused by spatial inhomogeneities, is plausible. What I found lacking was no description of the in situ and remote sensing sampling flight strategies. What kind of flight legs for each? Vertical spirals for in situ? How long? How closely matched in space and time were the in situ and remote sensing flight legs? What was the spatial footprint at cloud top of the reflectance measurement? And finally, what I really could not understand was the attributes of the single cloud reflectance spectrum applied to this study: was this really a single spectrum or was it averaged over a flight leg? What were the authors' motivations/ justifications for analyzing this single spectrum (rather than an average, etc.)? Was this representative of the entire domain under which the cloud was sampled?

Could there have been any chance that the underlying surface was not open ocean? My immediate concern was that variable surface albedo would impact the observed spectral reflectance over this low- to moderately thick cloud layer. However, the single instance in the text where surface albedo was discussed suggests that indeed the measurement was made over the Greenland Sea and that a measurement of the ocean albedo under the cloud layer occurred on that same flight.

My conclusion is that this paper should be published because it provides important new findings related to the applications of optical remote sensing to the vertical structure of mixed phase clouds. These results point to a potential inadequacy of a purely in situ based observation strategy to provide an adequate picture of mixed phase clouds in the Arctic. These are novel methods of analyzing cloud near-infrared reflectance spectra; it is evident that the spectrum presented in this paper is from a mixed phase cloud and that the authors succeeded in determining one or two cloud models that could reproduce this spectrum. What is less clear is how unique these results are; could other combinations of vertical ice/water distributions and cloud particle sizes produce similarly good matches? Unlike the authors, I include cloud particle size as a free parameter because it too could be biased by the same sampling issues that impacted other elements of this study. The conclusion from this study suggests that our model of Arctic mixed phase clouds dominated by liquid water at cloud top and ice at cloud base may need to be revised. This question of uniqueness of the results would need to be resolved before the full impact can be understood.

The following are specific comments:

1. I counted 21 instances of "SMARTAlbedometer" in the paper. The text would flow more smoothly without the instrument name repeated on every occasion. There is no ambiguity about the instrument used to acquire the spectra reflectance – no other instruments were used. 2. Similarly, I counted about 50 times where "ABM clouds" was used. It is clear that the focus of the study in on mixed-phase clouds in the arctic. Is it necessary to repeat ABM on every occasion where clouds are discussed? 3. P. 13805, l. 26: Absorption does not increase monotonically with wavelength in the near-infrared. The sentence should be rephrased: "The greater the absorption and the solar zenith angle, the larger the . . ." 4. P. 13806, l. 5 (and several other instances): Use "solar spectra cloud . . ." rather than "spectra solar cloud . . ." 5. P. 13808, l 5: Change "partly identical to" to "similar to". 6. P. 1309, l 5: The location of water vapor and oxygen absorption maxima are poorly identified. Oxygen-A is at 762 nm and the water vapor maximum is closed to 940 nm than 920 nm. 7. P. 13809, l. 16: It is difficult for the reader to understand the behavior of ice indices without reading the 2008a Ehrlich paper. The case can be made most simply and directly by stating: "Ehrlich et al. (2008a) stated that phase discriminating spectral features are most sensitive . . ." 8. P. 13810, l 13: ". . . is of less importance." Less important than what? 9. P. 13810, l 22: The cloud is split into two equally thick layers and on is called cloud top and the other cloud base. Upper cloud layer and lower cloud layer is more appropriate. 10. P. 13812, l 8: "This reveals that in the model cloud less ice crystals are present than indicated by the remote sensing measurements." Should add: or the model cloud ice crystals are too small. At this stage of the paper particle size cannot be ruled out. 11. P. 13812, paragraph starting at l 10: Again, without reading the pervious paper the indices don't mean much. But to describe the physics than explain the behavior

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of indices that may not be understood by the reader. 12. P. 13812, l 12, and several instances thereafter: absorption minimum should me maximum. It is confused with reflectance minimum. Absorption max = reflectance min. Check other instances in this paragraph (l. 18) and throughout text. 13. P. 13814, l 11: replace “gets” with “is”. 14. P. 13815, l 8: I think the authors may mean “example” rather than “exemplary”? 15. P 13815, l 9: Fig. 8 should be Fig. 6. 16. P. 13815, l 18: change “related to” to “from” or “emanates from”. 17. P 13816, l 1: Change “amplifies” to “supports”. 18. P. 13816, l 2: see comment 16. 19. P. 13818, l 1-2: here the time delay between in situ and remote sensing measurements is mentioned. How long was that delay? 20. P. 13818, l. 14: more on indices. 21. P. 13818, l. 25: Change “in” to “over”. 22. P. 13819, l. 5: “The shift of the maximum for the different wavelengths produces the rainbow-like colors of the glories.” What are rainbow-like colors? Both the rainbow and glory disperse light – so they are glory-like colors for the glory. Better to say “The shift of the maximum for the different wavelengths produces the color dispersion of the glory.” 23. P. 13822, l 5: “solar radiative cooling” is awkward. At terrestrial temperatures there is only solar heating (no terrestrial emission at these wavelengths). While the impact of these clouds may be to cool the surface by reducing absorption that would occur in their absence, this is not the same as radiative cooling as it is more typically used.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 13801, 2009.