Atmospheric
Chemistry
and Physics
Discussions

Interactive comment on "CALIPSO (IIR-CALIOP) Retrievals of Cirrus Cloud Ice Particle Concentrations" *by* David L. Mitchell et al.

Anonymous Referee #2

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This is an excellent paper and should be published. My comments are not damning and may be ignored. This author group is at the forefront of IR remote sensing and microphysics. And this paper is a major addition to their portfolio. The manuscript by Mitchell et al. aimed at retrieving ice particle number concentrations in single-layer semitransparent cirrus clouds (optical depth between 0.3 and 3) from satellite observations. A relationship established between and N/IWC from field campaigns were applied to the CALIPSO CALIOP/IIR derived to estimate N, the number concentrations. The authors justified their method by comparing derived from satellite observations from CALIPSO and field campaigns, as well as comparing the results from other independent in-situ measurements. This retrieval method was then applied to two years of CALIPSO IIR data and findings from global and seasonal scales were discussed. I think this is a

well-written paper and the novel technique presented can be accepted for publication.

The manuscript is quite long. If this is an issue, one could consider splitting up the retrieval/radiative aspects and the interpretation of the results.

The following are my and a colleague's comments:

Section 2.2.0. How do you handle the lapse through the cloud. Cirrus can be extensive in their vertical dimension. Are you errors larger for geometrically thick cloud?

Section 2.2.0: You mentioned you used the 0.55 micron extinction derived mean cloud temperature. I would expect that the cloud weighting functions are a bit different for the 10.6 and 12 micron observations and these may also differ from each other. Does this matter? Or this effect absorbed in the Blackbody radiance calculation in 2.2.3?

Section 2.2.2: What does the bias look like between model and observations before correction?

Section 2.3: You reference Yang (2005) and make some mention of habits on page 10. Are allowing habit to be a free parameter or are discouraging people from using habits at all in prescribing properties from databases such as Yang's? This relevant information to the remote sensing community.

Figs. 2 and 3 and so on: Why are computed from the TC4 campaign mostly concentrated in regions less than 1.1?

Figs. 8a and 8b: In these figures, IIR median and in situ compare relatively well. Can the authors show an example scatterplot of the comparisons?

Section 5: The selection criteria resulted in less than 2% of qualified pixels. This makes me wonder if the selection requirement is relaxed to include more cirrus clouds, how much do the results in Figs. 11-16 change?

Figure 11. Your highest Beta_Eff occur at the highest clouds where your sample size is often relatively small. Is that an issue? Or is the "hom" effect.

Figure 17: Comparing the two figures, can the authors explain why some peak regions remain similar magnitude such as in southwest Southern America, but many weaken significantly for instance over the Arctic Ocean East of Greenland?

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