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Atmospheric Measurement Techniques Discussions

Interactive comment on "Detection of multi-layer and vertically-extended clouds using A-train sensors" *by* J. Joiner et al.

Anonymous Referee #1

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General comments

The paper, "Detection of multi-layer and vertically-extended clouds using A-train sensors" by Joiner et al., concisely introduces a new multi-instrument technique to detect and differentiate multiple cloud layers and vertically-extended clouds. The technique makes use of existing data products and is targeted specifically at detecting cloud scenes that may negatively impact trace gas retrievals and calculations. The paper is well-organized and well-written. As a reader, I appreciated the authors' use of the active voice. As an algorithm developer, I was impressed by the discussions of algorithmic trade-offs. While the algorithm is quite specific and may only directly impact a small number of researchers, it is a useful contribution to the literature and appears to be well within the scope of the journal. The techniques developed appear to be

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robust and relatively well-documented, as well as scientifically sound. I have no major negative comments, although there are a number of specific comments listed below.

Specific comments and questions to the authors

1. I would like to have seen more discussion of the impacts of thin cirrus on both the algorithm and the trace gas retrievals the algorithm is designed to improve. I bring this up because of the issues with MODIS sometime mistakenly using the window channel (and opaque cloud assumption) cloud top pressure retrieval method in cases of thin cirrus and because of CloudSat's lack of sensitivity to thin cirrus. While it appears that you are trying to filter and only use CO_2-slicing retrievals in some cases, what are the impacts of missing thin cirrus?

2. I recommend including discussion about whether the results (90% correct identifications and 80% agreement in all three configurations, when compared with CloudSat) are good enough for the proposed use of the algorithm. This is also related to the previous comment.

3. On page 2714, line 25, the fractional cloud top pressure is introduced. I recommend adding a sentence describing why this representation of cloud top pressure was chosen.

4. Figures 3 and 4 in the manuscript are identical. This impacts pages 2715 and 2716. I'm guessing that Figure 4 was left out.

5. I appreciate the performance and inclusion of the the threshold sensitivity tests discussed on page 2716.

6. On line 15, page 2716, you state "(12 km) or MODIS (5 km). Figure 5 shows a flow chart of the CloudSat scheme. There is no dependence on τ as there is for the passive sensors. Clouds are said to be present in a layer l when the

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CloudSat mask shows a reliable detection in that layer (value > 5) and the layer-mean optical thickness $\tau_l > 0$." While I think I understand the point, technically requiring $\tau_l > 0$ implies a dependence on optical thickness.

7. I recommend substituting "as above for" with "similarly to" in line 18 on page 2716

8. Line 4, page 2719, is 3°S a typo? I believe the data is all north of the equator.

9. What is the grid resolution in Figures 11 and 12?

10. On lines 25-26, page 2723, you state "we have compared satellite cloud classification results from passive sensors with those from a coincident cloud radar on a global basis." Were the CloudSat comparisons truly global?

11. In Figure 2., ΔPdiff is always positive, implying that OMI cloud top pressure is always greater (lower cloud) than MODIS cloud top pressure. That doesn't seem to be the case in Figure 7. On line 2 of page 2715, ΔPdiff=(OCCP-Ptop)/(Ps-Ptrop). Please explain.

12. In Figure 6, I recommend plotting the CloudSat orbital tracks in a different color so that they can be distinguished from the continental boundaries more easily.

13. Please explain the upper rows of symbols and colors in Figures 7 and 8.

Interactive comment on Atmos. Meas. Tech. Discuss., 2, 2707, 2009.