Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2017-443-RC2, 2018

Atmospheric Measurement Techniques Discussions

Interactive comment on "Neural network cloud top pressure and height for MODIS" *by* Nina Håkansson et al.

Anonymous Referee #1

Received and published: 1 March 2018

This paper describes a new approach to retrieving cloud-top height using a neural network. It is an interesting report and gives us hope for improved retrievals. It will be more valuable if additional information is provided. It is much improved from the original submission. I realize that this is a first step, but a bit more analysis would provide the springboard for the next steps. This is an important paper, but too brief.

Abstract

"Nowcasting" should be "nowcasting"

Here and elsewhere: please spell out the acronyms the first time they are used (e.g., MODIS, AVHRR)

Sec. 2.2 and 2.3: Please indicate nadir or viewing angles of the CALIOP and CPR.

Printer-friendly version

Discussion paper

Sec. 3.2 pg. 4, 25: while the CO2 absorbing band is generally referred to as the 15-μm band, the MODIS channels are in the 13.3-14.4 μm range.

Sec 3.3.2: Were the clouds single-layered or both single and multi-layered? It is not clear here. Please indicate if you are training only for single layered clouds or training for the topmost layer. Is there a lower optical depth limit of the clouds detected in the CALIOP 1-km product?

Sec. 4 Are there biases in any of the results for both CALIOP and CloudSat? The mean absolute error does not tell us any tendencies one way or the other. Knowing biases is critical. While MAE is an interesting and informative variable, it gives us less information about variability, which the standard deviation of the differences (SDD) along with the bias would provide us, especially when added to the MAE. Additions of the bias should be included in the tables and discussed. If there is no bias, then the SDD would still provide useful additional information and place the results in the same context as many previously published comparison studies. Addition of biases may help the discussion.

Pg. 8, 14: What is the motivation for comparing with CloudSat? Is this a better reference? If so, why use CALIPSO? If not, why is it here? How were the matches made on the larger CPR footprint? Are there sampling differences between CALIOP and CPR? The CPR often misses the top portions of ice clouds and has difficulty detecting clouds with small particles. If the biases discussed earlier are known, the CPR information might be useful if the results are interpreted more in the discussion section. Also, what is the vertical resolution of CloudSat? Would that impact the differences?

Pg.8, 26: The plots are distributions of the differences. Bias is the average of those differences. Please correct.

Sec. 5 The discussion section is very thin. There is a paucity of what the results shown in the figures and table might mean. For example, what do the differences computed using two different references, CALIOP and CPR, tell us? All samples, except in polar

regions are taken in midday or near midnight for Aqua. Could there be any diurnal impacts of training only with this dataset? What happens if the neighbouring pixel is turned off in the training? The conclusions state that that is an important input. Can its impact be quantified to support that conclusion?

Pg. 9, 22: It seems that using matches with Terra will not help much in the nonpolar regions. Is this a realistic possibility given the orbital differences?

Pg. 9, 30: This section is where the further work on the sources of error (e.g., various cloud types) could be presented. It would help the discussion considerably.

Sec. 6. More analysis in the discussion section would help flesh out this section.

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2017-443, 2018.

Printer-friendly version

Discussion paper