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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Cloud vertical structure over a tropical station obtained using long-term high resolution Radiosonde measurements" *by* Nelli Narendra Reddy et al.

Anonymous Referee #2

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General Remarks In the present study, the authors report the long-term GPS-sonde observations of vertical structure of clouds over a tropical station in terms of cloud base and top altitude and frequency of occurrence of single and multi-layer clouds. Diurnal and seasonal variability of vertical cloud structure are discussed. The modification of vertical thermal structure of troposphere and lower stratosphere by the presence of multi-layer clouds is also addressed. The topic of the present study is of interest to climate scientists as representation of vertically resolved clouds and their feedback in the climate models remains as one of the major sources for uncertainty. However, the present study has limitations in vertically resolving the clouds and thus in identifying the multi-layer clouds. This should be discussed in details. I recommend this manuscript

for publication in ACP after a suitable revision by the authors. I am providing following specific comments, which should be addressed in the revised version.

Specific Comments:

(1) Earlier results from Gadanki have shown that during the Indian summer monsoon, this location is dominated by high-altitude cirrus clouds in the height region of 14-17 km. Given the fact that the radiosonde measurements of humidity are not valid at these altitudes, the frequency of occurrence of multi-layer clouds will be underestimated, especially during monsoon. Authors should discuss this aspect with sufficient details as it has greater implication on the present results.

(2) The authors claim that cloud layer boundaries detected in the present study are accurate by comparing the radiosonde detected cloud layers with that of Cloud Particle Sensor. By carefully examining this comparison shown in figure 2, one can note that there are no cloud particle at all in the detected cloud layers 2 and 4. Are they falls detection? This has important implication in estimating the frequency of occurrence of multi-layer clouds. The authors have to address this aspect.

(3) If authors can validate their radiosonde observations of CVS using micro-pulse lidar or ceilometer, the present study will be more credible as compared to earlier studies.

(4) Using figure 3, authors claim that the cloudiness is uniform around 50 km of Gadanki. However, this figure will not serve the purpose as it is a mean of 11 years. On a given day of the radiosonde observation, the cloud cover scenario may be entirely different. Except during the Indian summer monsoon, uniform cloud cover may not be present on most of the days. Authors can discard this figure as it is not serving its purpose.

(5) The explanation of diurnal variability of clouds is vague. The authors explain on the basis of interaction of short-and long-wave fluxes with clouds. The authors claim that in two-layer clouds during noon-time, the upper level cloud will be thinner due to

solar heating and lower level cloud will be thicker due to long-wave heating from the ground. But figure 7(b) at ∼14 hours shows the other way. Can authors explain this discrepancy? The thickness of the upper level clouds at 14 LT is comparable with that of 2 and 5 LT.

(6) Why there are no deep convective clouds at all in the single and multi-layer clouds in figure 6 and 7?

(7) As mentioned by the authors, Das et al. (2017) using CloudSat observations reported that the peak in occurrence of cloud base and top altitude are 14 and 17 km. This will be completely missed by the present analysis as mentioned earlier. How the authors will justify their results, which will be misleading. Authors at least should mention using other sources that what fraction of clouds will be underestimated by the present analysis.

(8) In figure 9, the authors show that the frequency of occurrence of high level clouds during winter is more than that during post-monsoon and comparable with that of monsoon. But the mid-and upper troposphere is very dry during winter as shown in figure 4(b). What is the reliability of this observation? Can authors check their analysis or provide a reference showing large occurrence of high level clouds during winter.

(9) Why figure 14 is discussed in summary section? Almost all the results presented in the study or below 12.5 km and the radiative effects shown in figure 14 are peaking near tropopause. There is no sufficient discussion on how single and multi-layered clouds will affect the radiative process. Authors have to discuss this aspect in details or remove this figure and corresponding discussion.

(10) There is a scope for improving the English grammar and the writing style. One gets the feeling that there are more statistics than the science in this manuscript. I suggest the authors to spend time on providing brief scientific explanations to their observations.

Minor comments: Line 248: Background weather condition should be replaced by background meteoroidal conditions. The term ‘weather’ should be used to address the refers to shorter period variations Line 340: Solar heating is diabatic process not adiabatic

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