Norrköping, 2015-07-10

Response to Referee #1

We would like to thank the referee for her/his comments that have helped improving the manuscript significantly. Please find below our point-by-point response to your comments.

1.This paper shows plots of geographic and seasonal variation of cloud radiative heating profiles over India, and speculates on the role that these heating rates may play in the monsoon, shows pdf's of cloud radiative heating in the TTL, and compiles some regional mean estimates of radiative energy balance terms. I don't have a substantive criticism with the analyses in the paper. However, the paper is mainly descriptive, and does not contribute a compelling contribution to any particular topic, in my opinion.

While we agree that the novelty of our work was not put forward justifiably in the previous version of the manuscript, we respectfully disagree that our work does not contribute at any particular topic. We had clearly defined and addressed four scientific questions related to intra-seasonal cloud variability during monsoon that can only be investigated using CloudSat+CALIPSO data. The main focus of our study is to quantify the vertical structure of cloud radiative heating and this aspect is not addressed by any previous study over the Indian subcontinent.

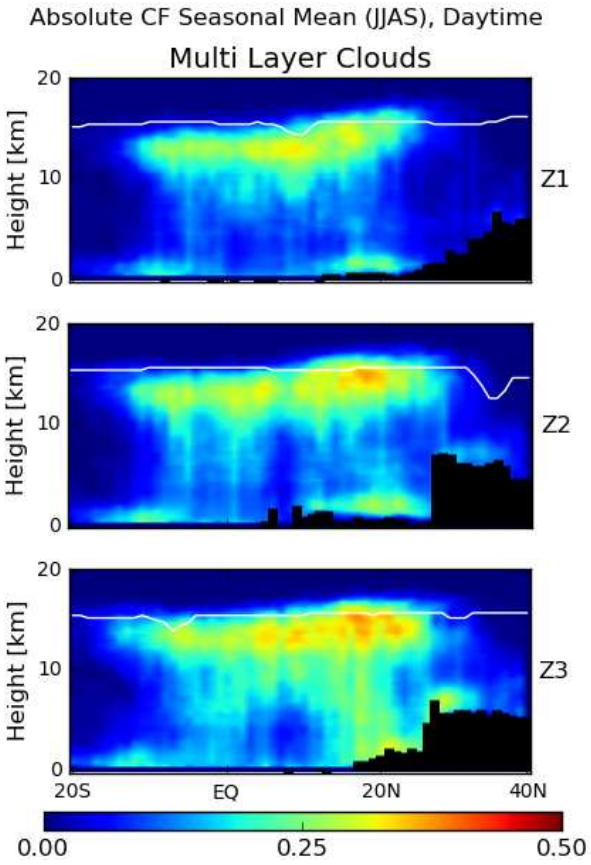
2. For example, a substantive paper might have compared the estimates of cloud radiative heating, ice and liquid water path, and cloud fraction they were using with the globalCERES, or other, data sets.

The point of the reviewer is well taken. However, it is to be noted that we have in fact used CERES data as shown in Fig. 7 of the manuscript. The limitation of CERES (or other imager-based) data sets are that they cannot vertically resolve different cloud types that are of particular interest for detailed monsoon studies (convective towers, cumulonimbus, cirrus etc), nor do these data sets provide radiative heating profiles. So a direct comparison is in our case unfortunately not possible. However, the CERES data are very useful to obtain a spatial overview of total net cloud radiative forcing and to set the stage for subsequent detail discussions using profiles from CloudSat+CALIPSO, as was done in the manuscript. Furthermore, we had clearly referred to CERES data wherever relevant (e.g. page 5439, line 19-23).

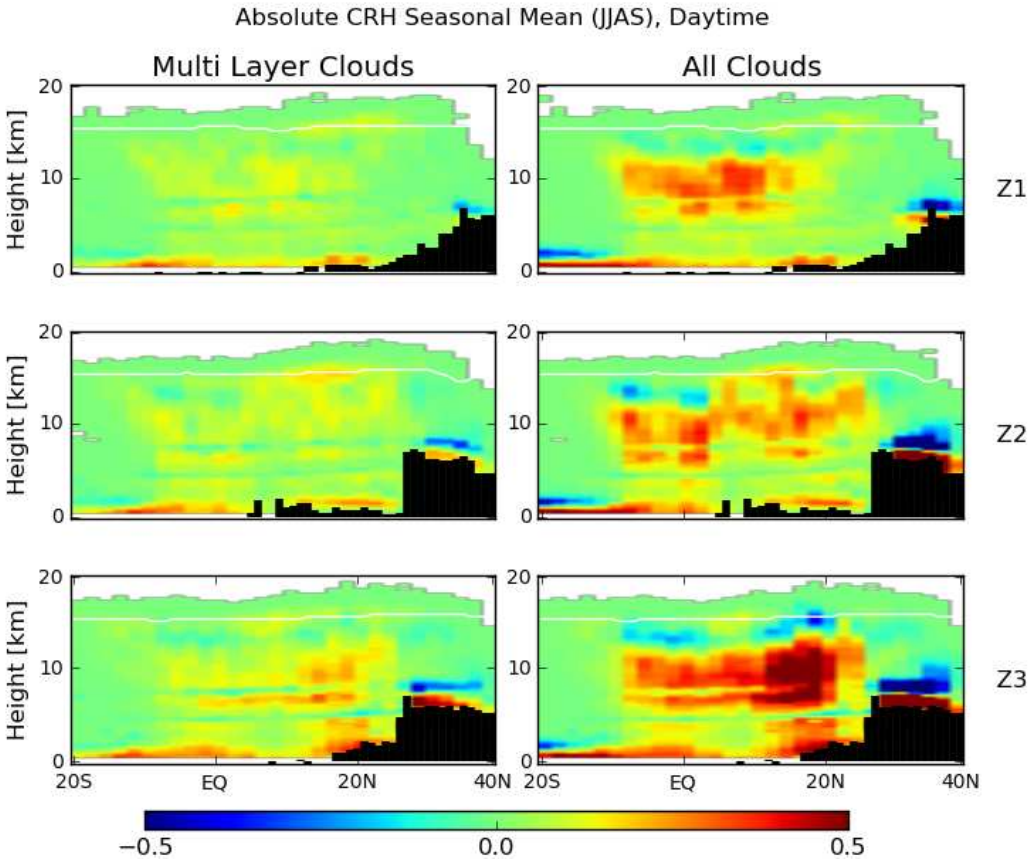
3. Maybe they could have looked at cloud vertical overlap statistics, an important consideration in cloud radiative modelling with larger grid cells. Perhaps these quantities could have been compared with model output.

The topic of multilayer clouds is indeed important, in particular for the modelling community. We have in fact analysed the cases with multilayer clouds, but chose not to show these results as the focus of the present study is on cloud radiative heating and the contributions from individual cloud types. The figure below shows a vertical cross-

section of cloud fraction when two or more layers are detected by CloudSat+CALIPSO over the selected three zones and averaged over the JJAS months. We can clearly see increasing importance of cloud overlap towards the east of the study area (from the Arabian Sea to Bay of Bengal, and also on the continental parts) as well as a preponderance of high cirrus clouds and a zonal gradient in their occurrence.



The figure below shows corresponding radiative heating from these multilayer clouds. For comparison, the radiative heating from all clouds (i.e. single and multilayer clouds averaged together) is also shown. It is evident, based on these two figures below and Figs. 8-10 in the manuscript, that the heating produced by individual stratiform and convective clouds dominates the radiative heating budget in the middle and upper troposphere, and that the atmospheric cooling produced at the tops of these clouds would dampen the heating produced by high clouds under multilayer situations.



4. Perhaps the variation in cloud radiative heating rates could have been lacked at in relation to other geophysical variables such as rainfall.

As mentioned before, the focus of the present study is **purely on the radiative component of the heating** as this has been the key knowledge gap. In the future, as we additionally will investigate the role of **latent heating component** in governing the monsoonal circulation, the rainfall becomes an important variable to be considered.

5. I would therefore like to see a more strengthened and focused paper, and think the best option would be to reject and perhaps resubmit later.

We have carried out additional analysis of cloud radiative effects to strengthen the focus of the paper. For example, a new subsection has been added in the revised manuscript where the sensitivity of the CRE to the estimated ice water path is discussed.

We hope that our clarifications would alleviate referee's concerns and that the revised version would meet her/his expectations. We kindly request the referee to read our

response to Referee #2 as well.

Technical Issues

I didn't find the cross-sections shown in Figure 4, 6, 8, 9, and 10, S1 - S5 very illuminating. What would have been more useful, and easier to interpret, would have been mean vertical profiles (and perhaps comparisons with other observational data sets).

As mentioned in the manuscript (page 5431, lines 3-14), the cross-sections are selected on the basis of distinct precipitation regimes that occur over these regions (Fig. 3, right panel) and also based on the spatio-temporal distribution of convective cloud regimes observed over these latitude bands investigated by previous studies (Devasthale and Grassl, 2009; Devasthale and Fueglistaler, 2010). We choose to retain height-latitude cross-sections as they provide important information on the zonal migration of cloud systems during monsoon months. Furthermore, these cross-sections illustrate the zonal gradient in radiative heating. Such information would be lost if we average data to mean vertical profiles as suggested by the referee. However we do understand the referee's point that having mean vertical profiles would be easier to interpret. Hence we have now revised Figs. 4 and S1-S3 and appended mean vertical profiles to them.

The paper at times contains odd language, e.g. "palpable", "potentcy", "tangible" in the Introduction

The introduction section is now revised and the usage of such words is avoided.

How are equations (5) and (6) related. Is "f" the same as "factor"?

Yes. This was already corrected in the manuscript version that appeared online.

It is unclear what "absolute fraction" after Eq (7) means.

As mentioned on Page 5430, lines 12-13, the absolute cloud fraction is the number of cloudy pixels within each altitude-latitude bin divided by the total number of observations (cloudy+clear) in that particular bin.

At various times, the paper has motherhood statements about the effects of cloud radiative heating on tropical dynamics. Of course this is true, but the paper does not really treat this topic, so the references to these interactions seems misleading. The paper also contains unneccessary references to e.g. the indirect aerosol effect.

The point of the reviewer is well taken. The revised version is more coherent and such statements are explained in their specific contexts.