Responses to Reviews of “**Climate Processes: Clouds, Aerosols and Dynamics (B6)”**

**Reviewer A**.

*“…the general comment I offer is the recommendations section (4) needs more teeth and specificity. The authors don’t do a good job at calling out specific gaps in our understanding and what we might do to address them. They miss an opportunity to be more visionary providing a road map for research in the future. The recommendations are little too broad and not specific enough to develop a program or series of programs around it.”*

We agree and are grateful to the reviewer for contributing some ideas here. With more time to gather ideas from the reviewers and others, and to reflect a bit, we have rewritten this section. It now includes some of the reviewer’s recommendations (probably not as forcefully as the reviewer would like, but this will be the case for all the other quite diverse opinions we have heard).

*“…there are other cloud feedbacks that contribute to the spread and it’s a little to strident to claim its all low clouds”*

Text modified to reflect this better.

*“…I don’t accept this statement about the positive feedback associated wit Hartmann’s FATA/PHAT?whatever the latest version is of the feedback. I have outlined this in my criticism of chap 7 of the FOD and the lead author knows them know. Don’t buy it and obs don’t support it, despite what Zelinka and Hartmann claim - their study is incomplete”*

What we say in the paper reflects the peer-reviewed literature, which now includes many studies, including observational ones, supporting this mechanism. We were unable to find any study supporting the scepticism of the reviewer, nor have we heard any clearly articulated explanation of what is or could be wrong with the reasoning or evidence behind this rather straightforward mechanism.

*“The statement that the difference between obs and satellite is due to lack of resolution in satellite data and not separating cloud from aerosol is not even close to correct….”*

This statement was not meant to blame discrepancies solely on satellites, but to suggest this as one source of error. We now address LWP and cite the noted articles.

*“This whole section is too limited-area model centric - I think you need acknowledge papers from van den Heever (2010) for example who have studied aerosol effects in high resolution large domain models with somewhat sophisticated microphysics where large scale circulations play important roles in the response (that also includes convection)”*

Done.

*“I think that arguing superparamterization is the possible path forward for more advanced microphysics is a bit of a stretch and what has been done so far is crude. It is no better than large scale models as far as I can tell.”*

We did not mean to endorse this strategy specifically. The recommendation has been rewritten and should now be clearer.

**Reviewer Yoden**.

*“…the second paragraph is a nice description on convectively coupled equatorial waves and tropical variability. However, this paragraph could be moved from the clouds and convection sub-section to the dynamics sub-section, because of the dynamical nature of convectively coupled equatorial waves.”*

This topic lies at the intersection of the two areas. We have chosen to leave it where it is.

*“In the dynamics sub-section 3.3, on the other hand, the description on the hot issues in tropical meteorology is little. I suppose such materials would be fully described in another paper in the Monograph. If so, very compact summary about them is enough in this review, just by citing the paper explicitly.”*

We have added a paragraph on this.

*“Another point which is not mentioned well in this review is the stochastic nature of (internal) atmospheric variability and the use of ensemble technique to study it. This subject might be described in another paper in the Monograph as well. However, compact summary about the stochastic nature of large-scale internal variability through highly nonlinear dynamics might be useful. Extreme events with very rare occurrence could be interpreted as a tail part of the probability distribution function of corresponding variations. Numerical experiments with a large number of ensemble members have become a standard method to study the nature of variations in general non-Gaussian distributions.”*

We have added a short paragraph to section 3 on this.

*“The heading of Section 2.3 is “****Dynamics from small to global scales****”, but the order of subsections is rather random. It could be from the subsection on small scale dynamics to global ones. Paragraphs in Section 3.3 could also be the order from small to global scales.”*

The section has been reorganized, thanks for the suggestion.

*“But the second sentence stating scientific problems lies between the sentences on the issue of our community: “Persistent problems in climate models include poor resolution of boundary layer and cloud processes, the representation of MJO and other modes of tropical variability (e.g., Lin et al. 2006), and the incorrect representation of the frequency of occurrence of high- and low-intensity rainfall events (e.g., Stephens et al. 2010).” This could be moved at the beginning of this section 3.3.”*

Done.

***Typos***

Fixed. Thank you for noticing these.

**Third Reviewer**

This reviewer begins with a thought-provoking discussion of what recommendations (s)he would make in Section 4. We agree and are grateful to the reviewer for contributing some ideas here. With more time to gather ideas from the reviewers and others, and to reflect a bit, we have rewritten this section. It now includes some of the reviewer’s recommendations (probably not as forcefully as the reviewer would like, but this will be the case for all the other quite diverse opinions we have heard).

We have included a reference to NICAM along with MMF.

We have also endeavoured to identify key questions in Section 3.

*Try to avoid phrases like: ”It is now well recognised”*

Done, although in some cases our job seems to be to track what the community is thinking or how thinking is evolving, and this is not always possible to document.

*A factual error is the conflation of mass fluxes with cloud parameterizations on page 4*

Fixed. This comment was meant to refer to cumulus parameterizations.

*For the aerosol forcing estimates, more could be made of how much smaller they are than originally hypothesized (see the early papers by Charlson). This can be seen as an area of tremendous progress. Also the manuscript (page 6) is rather critical of observationally based approaches*

Reviewer A also picked up on this. It has been reworded, since it was not the intention to be too critical of the satellite estimates.

*The manuscript presents a rather parochial view of field work*

We have eliminated references to individual field programs.

*I agree that the treatment of the aerosol is crude in all global models. I don’t agree that this is equally true of the other parameterizatons (page 7). The problem with the aerosol is that it parameterizes effects that live in the parameterizations.*

The distinction raised by the reviewer is not necessarily crucial. We have left the statement (“It could be said that…”) as is, since it is an arguable proposition.

*Second statement of section 2.3.5 would benefit from qualification, or references.*

The statement is not really necessary and has been deleted.

*I don’t agree that there is no existing fundamental basis for relating convection to free tropospheric humidity. This is what entrainment does.*

Of course, but this just restates the problem, since one then needs an entrainment rate scheme! And this ignores structural aspects, which are now being rethought by e.g. some of the other studies cited.

*The discussion of ice would benefit from a discussion of the complications that arise because of ice habit, and irreversibility.*

We judge this to be beyond the scope of this brief overview.

*Scale interactions tend to be strongest locally, i.e., neighboring scales interact most strongly. This is important when considering the parameterization of small scale inhomogeneous processes (RE discussion of aerosol scale interactions on page 14)*

Section has been reworded.

*I am not sure what ”fully resolved numerics” on page 15 means. One resolves features, and uses numerics to improve the effective resolution that one can obtain with a given number of degrees of freedom.*

Fixed.