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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Diagnosing the average spatio-temporal impact of convective systems – Part 1: A methodology for evaluating climate models" *by* M. S. Johnston et al.

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We would like to thank the anonymous reviewers for the time and effort they invested in making improving the paper. The issues highlighted have been noted and the paper have been thoroughly revamped in order to address, as much as possible, the reviewers concerns, suggestions, and grammatical errors and/or ambiguity.

General:

1.) We have updated the figures and tried to make them clearer and more legible. There was an error in figure 5 where the incorrect TMPA file was being plotted. This

has been corrected. The text throughout the article has been edited accordingly. 2.) We have also added a section discussing the movement of the DC systems and their interaction with some equatorially trapped waves. However, this topic is complex and can go far beyond the scope of the study; therefore, we have limited the discussion.

Specific:

1.) Referee 1 had a major concern with the interpretation of the results and especially the issue of equatorially trapped waves and the interaction with deep convective clusters. We would like to point out that this topic is very broad and complex and cannot be thoroughly addressed within the framework of the article being reviewed. However, since this apparent there is an observed movement of the deep convective systems within the Eulerian frame presented in the paper, it is only natural that we dedicate a section to discuss this in an adequate manner. This has been done and a new time-longitude plot was added to better highlight the movement of the deep convective systems. A separation of the data into the different waves, using for example the OLR as a proxy for clouds, is certainly a possible approach, but this goes far beyond the scope of this study whose purpose is to simply demonstrate the viability of the composite method to diagnose certain aspects of a GCM.

2.) The reviewer also suggested that changes to terminology "DC event", which is now changed to "DC system".

3.) The scattering of MW would not cause an underestimation of the UTH according the creators of the AMSU dataset. This has been verified.

4.) Separating precipitating clouds from non-precipitating clouds is possible for the observations, if one employs techniques similar to Chen et al 2011, but this goes beyond the scope of the article which tries to show the viability of the composite method in diagnosing a GCM. We have added some text to highlight this point.

5.) The text have been thoroughly checked for spelling/grammatical errors.

We believe the paper to be significantly improved and re-submit it for continuation in the peer-review process.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/13/C7708/2013/acpd-13-C7708-2013-supplement.pdf

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Interactive comment on Atmos. Chem. Phys. Discuss., 13, 13653, 2013.