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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Diurnal variation of upper tropospheric humidity and its relations to convective activities over tropical Africa" *by* E. S. Chung et al.

Anonymous Referee #1

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General comments:

This is an interesting paper discussing the diurnal cycle of UTH based on new high spatial and temporal resolution satellite radiances from METEOSAT-8. The diurnal cycle of UTH is consistent with previous study (e.g., Tian et al. 2004). In addition, some new diurnal cycle results based on EOF analysis and Lagrangian framework are also presented.

Specific comments:

The abstract should emphasize the new results in this study, that is, EOF analysis and

similarity/difference between Eulerian and Lagrangian frameworks.

Page 353, line 2: Although there are significant variations in the diurnal phase over the oceans, it has been well established that the oceanic deep convection generally peaks in the early morning, especially over the open waters. As I understand, the various hypotheses referenced by authors mainly aim to explain the early morning maximum in the oceanic deep convection instead of the regional variations of the diurnal cycle. Some literatures, such as Yang and Slingo (2001), Mapes et al. (2003), and Tian et al. (2005), do discuss the regional variations of the diurnal cycle.

Page 353, line 13: Should it be clear-sky OLR?

Page 353, line 28: There is a phase lag, although small, between deep convection, high cloud, and UTH over land in Soden (2000).

Page 354, line 6: What does 'more comprehensive measurements" really mean here? Is it referring to high temporal and spatial resolution of Meteosat-8 (15 minutes and 3 km)?

Page 356, line 21: Any reference for MTH algorithm?

Page 357, line 7: Define diurnal amplitude and phase which are mentioned later.

Page 357, line 17: Should we discuss the methodology for Lagrangian framework in this section?

Page 358: The discussion of harmonic analysis and diurnal anomalies in section 3.1 should be significantly reduced because most results here are consistent with those reported by earlier studies, such as Tian et al. (2004).

Page 358, line 17: duplicate what are stated on page 357, line 10.

Page 360, line 23: Diurnal anomalies should be in different section because they do not belong to harmonic analysis.

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Page 361, line 14: The claim that the diurnal cycle of precipitation is not directly related to deep convection is not right. The different phase between PI and DCC is due to their different definition. By definition, PI takes more account of colder clouds (such as TB11$<$215K), while DCC weighs all clouds above 230K equally. It is well known that there is a strong diurnal phase lag among clouds at different levels especially over ocean (e.g., Fig. 6 in Tian et al. 2004). Thus, the diurnal phase lags among PI, DCC, and CAC shown in Fig. 4 is not totally unexpected. If the authors change the TB threshold to 210 or 215K for DCC definition, the agreement of PI and DCC will be much better.

In any case, I would suggest to delete DC and HC is all figures and the authors should focus their discussions on PI, CAC and UTH (maybe MTH too) only.

Page 362, line 5: The afternoon peak of PI in January case is kind of surprising to me. This does not seem to be supported by the map in Fig. 3. When the authors calculated the diurnal anomalies over land and waters, did they use the area-weighted average?

Page 362, line 18: It is clear to me that there is a phase lag between PI (deep convection), CAC and UTH in Fig. 4. Of course, the phase lag is small over land and much larger over ocean. This is consistent with the results of Tian et al. (2004) based on the 3-hourly IR data over the whole Tropics. Thus, it seems to me that the authors' claim 'the same trend occurs between CAC and UTH' might not be true. As a result, their conclusion that the evaporation of ice particle from CAC plays only a small role in moistening the upper troposphere is not very convincing. These comments also apply to their discussion in other parts of the paper: abstract (page 352, lines 14-17), section 3.2 (page 365, line 1), and conclusion (page 368).

Page 363, line 13: The spatial pattern of the EOF analysis is very interesting. The 1st mode of PI seems to be related to the land-sea contrast. It is well known that the diurnal cycle is strong over land and weak over oceans. Thus, the smaller values over oceans are expected. The authors may want to increase the contour levels over water

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to highlight the spatial pattern.

Page 369, line 5: Gray and Jacobson (1977): Diurnal Variation of Deep Cumulus Convection. Monthly Weather Review, Volume 105, Issue 9, pp. 1171-1188.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 351, 2007.