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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Variation of upper tropospheric clouds and water vapor over the Indian ocean" *by* R. L. Bhawar et al.

Anonymous Referee #1

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Variation of Upper Tropospheric Clouds and Water Vapor over the Indian Ocean by Bhawar et al.

This paper analyzes ice, water vapor and temperature data at upper levels over the Indian ocean to examine the existence of a 'dipole' structure in the Indian ocean. The paper is not especially insightful, and does not have sufficient statistical rigor to be publishable in ACP. It needs major revisions as detailed below. There is nothing fatally flawed, but the conclusions drawn are not really justified by the analysis. The paper needs (a) better statistics (especially), (b) better justification of results, and (c) better referencing of previous work, particularly in the conclusions/summary.

I am dismayed that there is little effective use of the vertical structure or temporal sampling MLS to actually try to trace anomalies.

Fundamentally this paper also focuses too much on a small region of the tropics in one season, and only through correlations with ENSO are other regions treated. I think a broader analysis in space and time would be more insightful, and is certainly possible with the data (MLS) and analysis methods (compositing/averaging and EOF) used here.

To be publishable in ACP, the issues noted a-c above need to be fixed.

Major comments:

1. In particular, the paper spends a lot of time discussing convection and just using temperatures at different levels as a proxy for convection. That is not sufficient. Use a direct proxy for convection: such as opaque cloud top pressure, OLR or rainfall. All are available for this period, particularly on a seasonal level.

2. There is NO significance assigned to any of the statistics (slopes) or to the EOF analysis (are the modes significantly different). This needs to be conducted.

3. EOF analysis: I think the limited domain is not really appropriate for an EOF analysis: the variations may be ENSO or monsoon driven, and you will not pick that up by drawing a tight box. What happens if you use a tropical domain? What does the PC of the dipole pattern look like? You never show it. Also, the dipole is far from 'dominant': I am not even sure it marks a statistical test to be different than the next pattern when all months are used. How sensitive is the EOF analysis to the domain chosen?

To me you seem to have 'discovered' the upper level Walker circulation in the Indian Ocean. This is not too novel.

Minor Comments:

P21770, L20: what is convective intensity? It is never defined. Vague.

P21773, L13: I do not see a dipole in SST. You need something more objective here like an average SST and a correlation coefficient.

P21773, L21: Are these 2nd and 3rd modes significantly different from each other? See comments on the EOF analysis. Please show the PC.

P21773, L25: 16% v. 11% variance is hardly 'dominant': it might be significant.

P21774, L12: Show the PC of the EOF.

P21774, L15: I do not see a strong correlation in Figure 3. What is the correlation coefficient and significance?

P21774, L20: dominates is too strong as noted above.

P21775,L12: Relate these percentages to your data. Do they agree or not? What is the significance of the slopes. Are the correlations and fits good enough. Given the small sample, I am not sure about this. What about using individual months?

P21775, L19: You have not shown a contrast in convection. You could get some data for this, but you are just inferring it.

P21775, L24: Why? At 215 hpa there may still be positive correlations beetween T and h2o from convection? How do you know there is convection there? Need to use some estimates of convection (OLR, precip, etc)

P21776, L1: 100hPa, the change in water vapor

P21776, L2: You need better references here. The relationship between water vapor and temperature in the TTL goes back to Brewer 1949.

P21776, L2: 'transition level': No support fo this statement: need a reference.

P21776, L5: The difference comparison here is misleading: there is probably a factor of 5-10 difference in mixing ratio between 100 and 147 hpa.

P21776 L15: Are these slopes significantly different than zero?

P21776, L18: "significantly weaker". Statistically significant? How do you know? What is your metric.

The summary needs more references to previous work.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 21769, 2011.