

Thanks to the Reviewer for insightful and helpful comments to improve the paper.

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.

The main focus of this paper is to understand the response of upper tropospheric clouds and water vapor (H<sub>2</sub>O) to sea surface temperature (SST) changes over the Indian Ocean, focusing on an oscillatory Indian Ocean dipole (IOD) mode. Many previous studies have investigated the impacts of IOD on precipitation anomalies that are crucial to India and larger East Asian monsoon regions. Examination of the upper troposphere (UT) H<sub>2</sub>O and clouds response is motivated by two reasons 1) given the important radiative impacts of UT H<sub>2</sub>O and clouds, understanding their variability is needed, 2) UT H<sub>2</sub>O and clouds changes are closely related to deep convection; their variability with SST changes associated with the IOD indicate variability of convection and hence surface precipitation. Following your suggestions, the introduction and other sections have been revised to state our objectives more clearly. Also, the points a, b and c have been addressed and incorporated in the revised paper.

#### 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.

ANS) Temperature is not being used as the proxy for convection. The regions with large ice water content (IWC) are generally collocated with regions of low OLR; hence IWC acts as a good indicator of deep-convection [Su et al., 2006; Jiang et al., 2007]. The reviewer's confusion may be caused by unclear statements in the original manuscript. We have modified the sentences. (Page 8; line 170-175)

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.

ANS) The statistical significance level are included in the revised manuscript. (Page 7; line 150 and Page 8; line 162-163)

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?

ANS) Following the reviewer's suggestion, we have shown the EOF analysis for the entire tropics as below. We find that the first mode explains 12% of variance, and exhibits a dipole mode in the Indian Ocean. The second mode explains 6.9% variance and exhibits single-sign variability in the Indian Ocean. The PCs for EOF1 over the entire tropics and over the Indian Ocean only are shown in Figure 1 and Figure 2, along with the Nino 3.4 ST anomaly time series.

We observe that the Nino 3.4 index is very well correlated with the PCs for EOF1, suggesting that most of the PC mode signal is related to ENSO. But we also observe that there are a few cases where the PC for EOF1 in the Indian Ocean is not following ENSO.

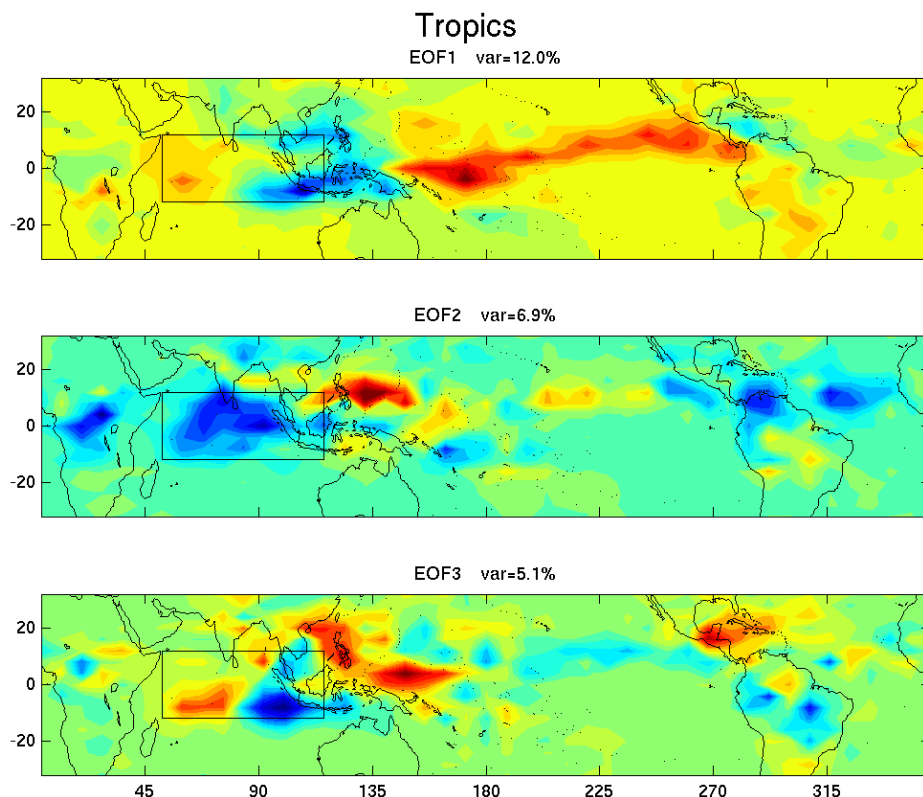


Figure 1: EOF modes for entire tropics for IWC at 215 hPa (box show the Indian Ocean region).

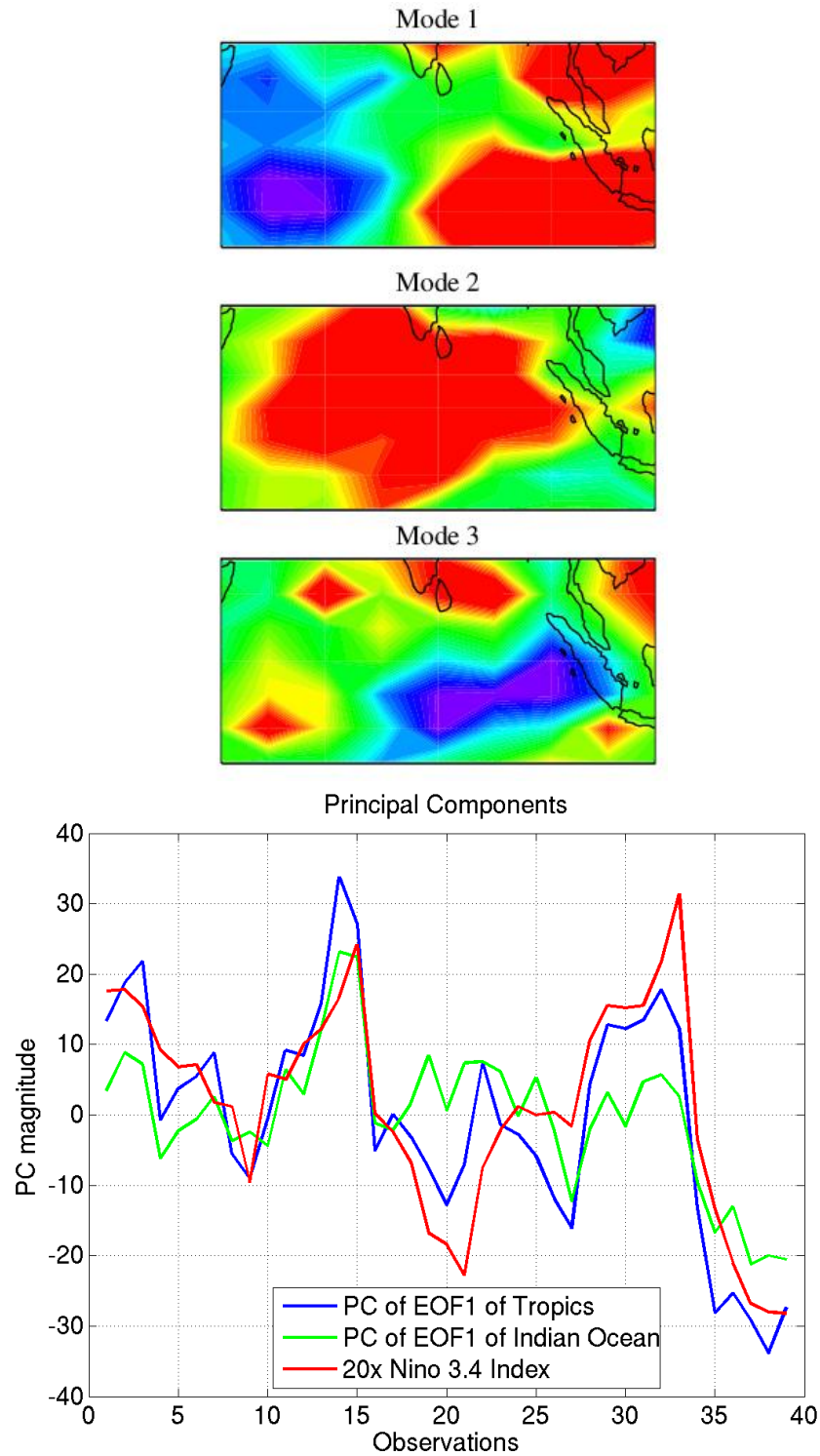


Figure 2: Top panel shows EOF modes for Indian Ocean for IWC at 215hPa, bottom panel shows the Principal Components for EOF1 over the entire tropics and over Indian Ocean along with the Nino 3.4 SST anomaly time series

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

As you rightly said, the upper Walker circulation is already an established fact. We show both the correlations of UT H<sub>2</sub>O/IWC with local and remote SST variabilities, and intend to distinguish the roles of direct thermal response and remote teleconnections.

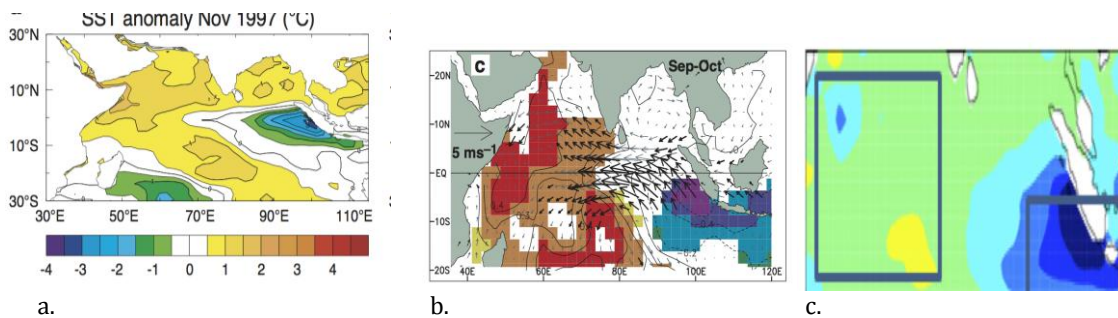
#### Minor Comments:

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

Fixed; modified sentence. (Page 2; line 26)

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

The dipole pattern observed in the present work is in agreement with the IOD modes that were discovered before. The IOD mode is manifest predominantly during September, October and November (SON) and not during any other season of the year. IOD is initiated by an anomalous upwelling along the Sumatra-Java coast at the start of the normal upwelling season in May-June. This enhances cooling of SST in the EIO, which couples with a westward wind anomaly along the equator and drives rapid growth of IOD. The wind anomaly and associated Ekman pumping generate off-equatorial Rossby waves that travel westward, deepen the thermocline, and warm SST in the WIO, causing the peak of an IOD event a few months after it begins. The decay of an IOD event is characterized by a slow eastward propagation of warm anomaly along the equator, with warm SST leading deepened thermocline depth. The deepened thermocline arrives at the eastern boundary and reduces the rate of cooling during the next upwelling season (Feng et al., 2001, 2003; Saji et al., 1999; Webster et al., 1999; Vinayachandran et al., 2007). Thus SON months are considered the months when we find this dipole mode at its peak. During JJA this dipole mode is weak and in its starting phase. We do not observe any periodic oscillation during DJF and MAM. Also, the two boxes indicated in figure 1 in the SST plots are the region in which this dipole oscillates. These boxes are used in the previous studies (Saji et al., 1999; Webster et al., 1999).



**Figure 1.** Ideal SST variability pattern observed in Sep, Oct, Nov, during IOD (a) from Webster et al., Nature 1999; (b) from Saji et al., Nature 1999 and (c) from the present paper for 2006, indicating the IOD pattern.

P21773, L21: Are these 2nd and 3rd modes significantly different from each other? See comments on the EOF analysis. Please show the PC. PC figure is attached. Also the figure for 2<sup>nd</sup> and 3<sup>rd</sup> modes for entire tropics is shown See response to Major Comment 3.

P21773, L25: 16% v. 11% variance is hardly 'dominant': it might be significant. Fixed; changed dominant to significant. (Page 6; line 118)

P21774, L12: Show the PC of the EOF. Fixed (EOF for tropics and PC figures are shown above).

P21774, L15: I do not see a strong correlation in Figure 3. What is the correlation coefficient and significance? Figure 3 shows anti-correlation between WIO and EIO region during the strong dipole period (September-October-November). The correlation coefficient between WIO and EIO is -0.41 with 95% statistical significance. (Page 7; line 138-140)

P21774, L20: dominates is too strong as noted above. Changed to significant as suggested. (Page 7; line 145)

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? Fixed

The percentages shown in the paper are reported for entire tropics (30°N – 30°S). As we are concentrating over a small region over the Indian Ocean i.e. IOD region we observe stronger regional increase in IWC at all the three levels. (Page 8; line 160-165)

P21775, L19: You have not shown a contrast in convection. You could get some data for this, but you are just inferring it. The correlations in figure 3 top panel are for the difference between the WIO and EIO region. Hence the “greater convection contrast” referred is to the strong increase in the difference of H<sub>2</sub>O between these two regions corresponding to the difference in SSTs in those regions. (Page 8; line 176)

P21775, L24: Why? At 215 hpa there may still be positive correlations between T and h<sub>2</sub>o from convection? How do you know there is convection there? Need to use some estimates of convection (OLR, precip, etc) Fixed, Added previous references which addresses relationship between IWC and OLR [Jiang et al., 2007; Su et al., 2006]. (Page 8-9; line 179-184)

P21776, L1: 100hPa, the change in water vapor. Fixed, made change as per suggested. (Page 9; line 189-190)

P21776, L2: You need better references here. The relationship between water vapor and temperature in the TTL goes back to Brewer 1949. Fixed (Added references [Brewer, 1949; Jensen et al., 1996, Corti et al., 2008]). (Page 9; line 190-191)

P21776, L2: 'transition level': No support fo this statement: need a reference. We define the 147hPa level as the “transition level” as it is present in between the convection dominated 215hPa to temperature-controlled 100hPa. (Page 9; Line 191-193)

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. Fixed, removed the misleading word comparison. (Page 9; Line 194)

P21776 L15: Are these slopes significantly different than zero? Fixed, changed to small negative slope. (Page 9; Line 194)

P21776, L18: "significantly weaker". Statistically significant? How do you know? What is your metric. The correlation of IWC with Nino 3.4 SST is 0.4 with little statistical significance hence we used the word significantly weaker. (Page 10; line 207)

The summary needs more references to previous work.

ANS) The teleconnections between ENSO and the upper troposphere have been reported, i.e., effect of ElNino on the troposphere (*Kent et al.*, 1995; *Wang et al.*, 1996 and *Massie et al.*, 2000) and stratosphere (*Bronnmann et al.*, 2004; *Ineson and Scaife.*, 2009). However, the studies about the implications of the IOD on the upper troposphere have been limited (*Kug et al.*, 2009; *Liu et al.*, 2007). (Page 10; line 219-223)

All the references enlisted here are included in the revised main manuscript.