

Interactive comment on “Effect of tropical cyclones on the Stratosphere-Troposphere Exchange observed using satellite observations over north Indian Ocean” by M. Venkat Ratnam et al.

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This is an interesting study of the impact of cyclones on ozone and water vapour in the upper troposphere and lower stratosphere. It is based on the analysis of some cases study, using satellite measurements to estimate the air flux across the tropopause. It is surely a valuable contribution on a hot topic in stratospheric research, since the ability to predict future changes in the stratosphere relies on correct estimates on how tropical troposphere to stratosphere transport might evolve. I definitely agree that the role of deep convection in cyclones is worth of more research, and it is appropriate for the

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journal, so I encourage the publication of this work. However, there are a number of open issues that have to be addressed; therefore I recommend a revision before publication. I had the chance to read the general comments of the Anonymous Reviewer #2 and I do share all his/her general comments.

Reply: First of all we wish to thank the reviewer for going through the manuscript carefully, appreciating actual content of the manuscript and offering potential solutions to improve the manuscript content further. We have revised the manuscript while considering both the reviewers comments/suggestions.

In particular I find strange how the results from previous work of Ravindra Babu et al. (2015) are used in the present paper: on one hand, figures and conclusions from that paper are reproduced in a way that seems redundant, on the other hand a description of the method used in that work, which is duplicated in the present one, is lacking so to force the reader to go to the original reference. I therefore suggest to briefly summarize the results AND methods presented in Ravindra Babu et al., and to skip fig.2.

Reply: The methodology explained in Ravindra Babu et al. (2015) is re-produced briefly in the current manuscript as suggested. Note that figure 2 is very important even for the current manuscript and thus retained.

Detailed comments:

lines 49-52: These sentences seems more to describe what the article is aimed for, than an introduction, The authors should support their claims with references, or the sentences should be made less assertive.

Reply: We have provided more references at the appropriate places as also mentioned by other reviewer.

61: Again, the assessment of the effectiveness of cyclones in promoting STE is the objective of the paper. References should be made to previous studies supporting this claim, or the sentence should be dropped, or reformulated to introduce the aim of the

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paper.

Reply: We have added relevant references for the text used in the present study at appropriate places.

62-63: The Stenke and Grewe paper deals mainly with the impact of water vapour increase on ozone chemistry. I did not find any claim of temperature increase induced by an increase of WV, there. On the contrary, there is a lot of modeling evidence (and even some experimental study, see as instance Maycock et al., Q. J. R. Meteorol. Soc., 2014), in the literature, that an increase stratospheric water vapor would lead to a cooling of stratospheric temperatures. So the sentence in the paper seems not correct.

Reply: We have corrected the sentence while adding suitable reference.

82, 86, 87,96: TC, MST, BoB, COSMIC, abbreviations have not been introduced earlier.

Reply: The abbreviations are elaborated when they appear for the first time in the revised manuscript.

91: The findings presented in Cairo et al. (2008) should be reported.

Reply: Reported.

128-134: Such information should be presented as a table.

Reply: We have added one more table with the classification of cyclones over north Indian Ocean as suggested.

139-140: This sentences is not clear. Is it suggesting that only long lasting cyclones have been selected in order to have enough MLS WV profiles in the cyclone area? This is quite an important point, and the average number of MLS profiles used should be quoted, maybe even in the form of a table, for each cyclone (the developing stage of the cyclone corresponding to the observations could also be accommodated there, see line 192). Moreover, I think it is worthwhile to discuss in further detail how the horizontal (given the spatial variability of the WV and ozone in the cyclone area) and

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vertical resolution of MLS are adequate to the goals of the paper.

Reply: We reported available MLS profile for each cyclone in the form of table in the revised manuscript.

162-177 and fig. 2: I do not see the point to reproduce Fig.2, from Ravindra Babu et al. (2015), here. In 3.1 I do not see any novelty with respect to the analysis presented in that 2015 paper. The methodology and main results of that paper could be just shortly described and summarized.

Reply: It is well known that the tropopause characteristics play an important role in controlling the STE processes. Though the tropopause characteristics are mentioned in our earlier draft, we would like to retain figure 2 in this paper as it will be easy to refer the tropopause characteristics by the readers so that this paper will remain stand alone. This will also avoid going through our earlier paper as rightly mentioned by both the reviewers.

207-209: How robust is this feature in the data? Are all cyclones contributing to such enhancement?

Reply: It will change based on cyclone intensity. This will be more in the case of maximum intensity of cyclone such as SCS and VSCS category. Please see figure 5 for more details. Note that we calculated based on intensity and are not showed in the manuscript. However, our analysis confirms that the ozone is more in the case of VSCS compare other SCS and DD categories. During the VSCS time the ozone detrainment is reached to the 146 hPa level. Since the available profiles of MLS are less for different intensities so we combined all the profiles that are available within 1000 km from the centre of all 16 cyclones.

224 and 246: Cyclone winds can lose their axial symmetry near the top of the cyclone, and concentrate in one or two curved outflow jets. The authors may review the literature and see whether this can explain the upper level asymmetry in ozone and WV

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anomalies.

Reply: This is very important point that the cyclone winds play important role in the distribution of the water vapour and ozone above the cyclone. As mentioned earlier, the higher ozone mixing ratios are observed in the western and northwest side and more water vapour is located at the eastern side of the cyclone centre because of the upper level anti-cyclonic circulation over the cyclones. This will push the water vapour towards the south and east side of the cyclone centre. In the other side of the cyclone, the detrainment of the lower stratospheric air may occur along with strong subsidence in the cyclone centre. This might be the region for higher ozone in the west and northwest side and more water vapour in the east and southeast side of the cyclone centre. Note that Ray and Rosenlof (2007) also reported higher water vapour mixing ratios in the east side of the cyclone centre for Atlantic and Pacific oceans. Further, very recently Reutter et al. (2015) reported that the more stratosphere-troposphere transport takes place in the west side of the cyclone centre due to west ward tilt of the cyclone with height. These aspects are mentioned in the revised manuscript.

294: the authors should dwell more on the method they used to estimate the term F_{am} . At present, it seems their choice of 0.5 hPa is quite arbitrary.

Reply: Since we do not have pressure variation with time we have assumed different pressures while considering minimum to maximum possible pressure variations, which is the best way when no observations are present.

299- 303: It seems this spatial asymmetry is a common, constant feature throughout the database “. . . the downward flux is always more. . .”. the authors should really dwell more on that, trying to find possible explanation in terms of the cyclone dynamics.

Reply: The tropopause flux is calculated for each cyclone maximum intensity day only so on the higher intensity time within 500 km from the cyclone centre the anti-cyclonic flow dominated and cause the upward flux in the east and southeast side. Whereas, subsidence dominating in the other side cause downward flux in the west and north-

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west side of the cyclone.

330: “intensify” for “intensity”? 330-339: It seems that (exactly) these results are already been reported in the quoted Ravidra Babu et al., 2015 paper. I do not understand why they are repeated here.

Reply: For completeness we have included these sentences in this paper also as someone may be interested to see the tropopause variations during these cyclones and to make this manuscript standalone we retained those statements and related figure.

364: “intensity” for “intense” ? 366: “effecting” for “affecting”? Figure 1 caption, “strom” for “storm”

Reply: Corrected in the revised manuscript.

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