

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.

Anonymous Referee #3

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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 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 publi-

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cation. I had the chance to read the general comments of the Anonymous Reviewer #2 and I do share all his/her general comments. 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. 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. 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 paper. 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. 82, 86, 87,96: TC, MST, BoB, COSMIC, abbreviations have not been introduced earlier. 91: The findings presented in Cairo et al. (2008) should be reported. 128-134: Such information should be presented as a table. 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. 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. 207-209:How robust is this feature in the data? Are all cyclones contributing to such enhancement? 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 anomalies. 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. 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. 330: “intensify” for “intensity”? 330-339: It seems that (exactly) these results are already been reported in the quoted Ravindra Babu et al., 2015 paper. I do not understand why they are repeated here. 364: “intensity” for “intense” ? 366: “effecting” for “affecting”? Figure 1 caption, “strom” for “storm”

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