

Interactive comment on “Influence of tropical cyclones on tropospheric ozone: possible implication” by S. S. Das et al.

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Response to Referee #3's Comments Review for “Influence of tropical cyclones on tropospheric ozone: possible implication” by Das et al. The authors present an interesting study where two cases of tropical cyclones enhance tropospheric ozone levels. The two cyclones Nilam and Phailin are discussed with data from ozone dropsondes, surface ozone measurements and relative humidity derived from satellite scans. Furthermore, numerical simulations are used to get a more complete picture of the dynamics of the events. All in all, the argumentation is clear enough and the data support the main statements. However, there are also some concerns which need some consideration before the manuscript is ready for publication.

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Response : We would like to sincerely thank the anonymous referee for very positive evaluation and constructive comments/suggestion for the improvement of the manuscript.

Point-by-point responses on how we have addressed each recommendations/suggestions are given below. Please note that manuscript is also altered in view of reviewer - 1 and 2's comments and suggestion.

Major concerns: 1. In its present state, the introduction is not very well structured and at several places remains rather unspecific. For instance, it is written that Appenzeller and Davies (1992) attribute the stratospheric intrusion to “disturbed weather conditions over mid-latitude”. This is not specific enough!

Response : We have rewritten the introduction section.

For instance, I would not agree that stratospheric intrusions are generally a slow process, as written in P19307,L10. See for instance recent studies by Bourqui and Trepanier (2010), Skerlak et al (2014) or the review by Stohl et al. (2003) with its particular focus on the synoptic scale of STE (see below). In short, a more careful review of current literature seems to be appropriate.

Response : Now the sentence is revised in view of recent literature survey and more references are added in this version of manuscript.

The first paragraph starts with some very general statements about ozone in the atmosphere. I wonder whether this could be considerably shortened and the focus brought much faster to the main topic of the study, i.e., how tropical cyclones influence the tropospheric ozone levels.

Response : We feel general introduction is necessary to make the reader comfortable. However, we have revised these introductory sentences taking in account of all the reviewer's comments.

At L19307,L8-9 it is written that stratospheric intrusions “... also decreases the

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stratospheric ozone, which in principle enhances the penetration of UV to reach the Earth's surface." In principle yes, but I doubt that it is of practical importance. Is there a reference for this statement?

Response : As per our knowledge there are no such literatures which have shown quantitatively the amount of decreasing stratospheric ozone and its effect in enhancing the penetration of UV radiation to Earth's surface. However, this sentence is well accepted and mentioned in many literatures (e.g. Baray et al., 1999 and Cairo et al., 2008)

â€” The last sentence of the introduction repeats statements from before. There is no need to 'complete' the introduction with such a summarizing statement.

Response : Now the sentence is omitted in the revised manuscript.

â€” In P19308,L8-10 the effect of humidity on ozone is discussed. But it remains unclear how, in the context of the paper's research topic, this fits in. The sentence looks a little 'out-of-context'!

Response : This sentence is omitted. Introduction is rewritten in the revised manuscript.

â€” The aim of the study is only handled in one single sentence near the end of the introduction: "The present study addresses the influence of tropical cyclones quantitatively on enhancement of tropospheric ozone by stratospheric intrusion." First, the sentence structure looks a little strange to me, second, I would appreciate when the aim of the studied is presented in some greater detail.

Response : This sentence is revised accordingly.

2. Some physical arguments remain unclear, or can be critically questioned. â€” In Figure 2 the surface ozone measurements for Phailin are shown. There is a nice shift in background ozone levels from 14 to 15 October 2013. The authors attribute this increase to air descending within a stratospheric intrusion. In the same line they men-

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tion other processes which influence ozone levels (P19313, photochemical reactions, biomass burning and lightning). However, they state that given the cyclones' characteristics, the impact of any of the three mechanisms will be very low. If this is the case, where does the diurnal cycle in the surface ozone measurement coming from? In short, I am not fully convinced that the other processes are really negligible. Note also that on 16 October the surface ozone measurement reaches very low ozone mixing ratios, although the stratospheric intrusion already 'took place'.

Response : By the statement "given the cyclones' characteristics, the impact of any of the three mechanisms (photochemical reactions, biomass burning and lightning) will be very low", we wanted to say that changes in these mechanisms would be low. We agree that the diurnal variability in surface ozone is influenced by photochemical processes under the presence of precursor gases like NO_x, CO, etc (David and Nair, JGR, 2011). But the enhancement in ozone is observed in the middle and upper troposphere and as the day progresses the enhancement height of ozone decreases. If the enhancement is due to local activities e.g. biomass burning then initially we might have observed ozone enhancement in the surface or lower troposphere which is in contrast to our observations. Considering the variability in the surface ozone at which is ~9.5 ppbv (i.e standard deviation during month of October), the observed enhancement (~10 ppbv) fall under the day to day variations. Thus, in the view of this, it is possible that observed enhancement could be due to surface reaching intrusion effect but possibility of local/anthropogenic activity could not be fully ruled out.

There are few studies which clearly proven the enhancement of surface ozone due to intrusion of stratospheric into the troposphere associated with severe weather condition (Stohl et al., 2000; Jiang et al., 2015).

â€” As a 'proof' that stratospheric air is really coming down, it is written that "Enhanced potential vorticity 0.5–1.5 PVU is also observed vertically down from the stratosphere to the surface, overlapping the downdraft regions." There is indeed a clear PV maximum discernible in Figure 3, at about mid-tropospheric levels. But one might argue that this

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is diabatically produced PV, due to condensational heating, which therefore is not of stratospheric origin. This option should clearly be discussed in the manuscript.

Response : We thank referee for the suggestion. Now we have incorporated in the revised manuscript.

â€” Some of the formulations are not careful enough. For instance, (P19313,L25) “The potential temperature contours indicate the presence of unstable atmosphere at this location”. I do not see any sign of an unstable air column. Possibly, what is meant is that the stability is reduced?!

Response : Corrected in the revised manuscript.

Minor concerns: - P19309,L3: There is no need to repeat at several places that it is a very severe cyclonic storm’. The text seem here, and other places, a little repetitive.

Response : Corrected in the revised manuscript.

- P19309: I am not familiar with ozone analysis. What does “1% linearity” mean?

Response : The surface ozone analyser was calibrated by applying known mixing ratios of ozone in the measurement range (i.e. 0 to 70 ppbv). The calibration constant is fixed based multipoint calibration (i.e. based on linear fit between know values of ozone and those measured by the analyser). The analyser shows linear behaviour, however it deviates from linearity by $\pm 1\%$. Thus, ozone mixing ratios may have deviation of $\pm 1\%$ of its measured value. Note that our ozone measurement range is from few ppbv to 30 ppbv, $\pm 1\%$ correspond to 0.3 ppbv which is very small in the context of observed enhancement (i. e. ~ 10 ppbv).

- P19309,L23: “This data is also used to do the qualitative analysis of stratospheric air.” What do you mean with ‘qualitative analysis of stratospheric air’?

Response : Sentence is removed from the revised manuscript.

- Section 3. The last paragraph of section 3.2 would better be placed in the introduction

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to the whole section 3. It applies to both cases and already refers to the images in Figure 1.

Response : Following the reviewer’s suggestion, we have moved this sentence to the whole section 3.

- Section 4: I strongly suggest to rename section 2 into “campaign details and data” (or something in this direction). Then, the description of section 4 could be added to this new section. At the moment, section 4 with all the technical details of the NWP simulation looks rather out of place. It distracts the reader from the physical discussion.

Response : Following the reviewer’s suggestion, we have moved the section under ‘Numerical Simulation’ to the section 2.

- Figure 1: Mention in the caption what the blue star refers to!

Response : Added in the figure caption.

- Figure 2: Describe in caption, what the mean ozone profile refers to. It’s discussed in the text, but not in the figure caption.

Response : Added in the figure caption.

- Figure 3: The position of the height-latitude cross-section could be shown in Figure 1.

Response : It is very difficult to accommodate this information in Fig.1 (a) as it is already very crowded. We hope reviewer will kindly accept not to incorporate this particular suggestion.

Reference

David, L. M., and Nair, P.R. 2011.: Diurnal and seasonal variability of surface ozone and NOx at a tropical coastal site: Association with mesoscale and synoptic meteorological conditions,116, D10303, doi : 10.1029/2010JD015076.

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Stohl, A., Wernli, H., Bourqui, M., Forster, C., James, P., Liniger, M. A., Seibert, P., and Sprenger, M., 2003.: A new perspective of stratosphere-troposphere exchange. *Bull. Am. Met. Soc.* 84, 1565-1573.70, doi: <http://dx.doi.org/10.1175/BAMS-84-11-1565>.

Interactive comment on *Atmos. Chem. Phys. Discuss.*, 15, 19305, 2015.

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