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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Assessment of the interannual variability and impact of the QBO and upwelling on tracer-tracer distributions of N_2O and O_3 in the tropical lower stratosphere" *by* F. Khosrawi et al.

Anonymous Referee #1

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The paper by Khosrawi et al. uses N2O and Ozone data from different satellites for model evaluation and to assess the impact of the QBO on tracer-tracer distributions. The paper relies to a large part on N2O and Ozone data by the Odin instrument. My main concern is that an analysis is performed which discusses subtle changes in a data set which shows rather obviously problems, especially with respect to the N2O values of Odin, showing values above the tropospheric background at 25 km altitudes in the tropics. There have been a number of in-situ aircraft measurements (see e.g. Volk et al., 1996, 1997; Strahan et al., 1999) in this region deploying high preciscion in-

situ N2O measurements. None of these data sets have found enhanced even N2O at 20 km altitude. N2O values higher up are certainly lower due to the photolytical sink of N2O. Further there is no plausible mechanism which could produce large amounts of N2O or transport them to these altitudes. Looking at the in-situ climatology by Strahan et al (1999), I wonder what we can learn about variability of O3 for N2O larger than 300 ppb at 650 K pot. temperature? Most probably that there is a problem with the N2O measurement. Eventhough the climatology by Strahan is somewhat older and N2O values have increased by some 3-4% since, this climatology would suggest values on the order of 300 ppb as a maximum N2O in the tropics at 500 K and certainly significantly less at 25 km altitude. This is also supported by the correlation between N2O and O3 from ACE (Fig. 1) which goes to values on the order of 270 ppb at 650 K and by the correlations shown in Fig. 4. and 5. which do not show such high N2O values (even though they are higher than from the in-situ climatologies). Therefore, I think that the N2O axis chosen as a reference in this correlation study is highly uncertain, which puts the whole basis of this analysis on very shaky ground. My suggestion would be to exclude all tropical Odin N2O data from this analysis, as it seems that the data base is not solid.

In addition to this general remark, here are some specific suggestion, which I would suggest to take into account when producing a revised version for submission:

p.4.l 17: please specify which lifetime is meant (global, local etc.)

p. 5.l 5ff: Ox is mainly destroyed in the high latitudes. The lifetime of ozone itself is shorter in the low latitudes.

p. 14.l3.: I think that the decrease/increase of N2O/O3 is better visible in a simple vertical profile then in this view. . .

p. 14.l.5 I have to admit that I do not understand this statement: what is meant by a set of curves lying in potential temperature bins? These are curves for separated potential temperature ranges.

p.14.l 10 ff: I think that this is actually a bit a comparison of apples with pears. The ATMOS correlations are from one latitude and the variation in N2O and O3 comes from the different altitudes sampled. In the present study the variation in N2O and O3 is caused by the latitudinal variation in sampling. This is something entirely different.

p.16, l 16ff: I do not understand some of the argumentation in this section.

p.17.l11: I suppose that the reasonable agreement refers to the 650 K level.? Or is the agreement between the models meant?

p.19. l 10 ff Further to the general remarks given above, it is not possible from Fig. 3. to distinguish in which year the climatology extends to higher N2O values or has more ozone variablity. In any case I wonder why for the discussion presented here a correlation is chosen, as this could be discussed straight forward using the latitudinal distributions observed (as the O3 is not really used in the discussion). It is interesting to note that these extremely high N2O mixing ratios occur at O3 values on the order of 5 ppm, i.e. values typical of the middle stratosphere.

p. 20.l 4: I do not see why a higher number of observations should lead to a smaller standard deviation.

p. 21 l 22.: I would suggest to sort the data according to the QBO phase and then derive a QBO-east and a QBO-west correlation and see if they deviate in a statistically significant way from each other.

p. 22. L25ff: Isn't this quite trivial? I would expect the maxima of the averaged bins to occur when the maxima in the data occurs.

Section 5.2.2.: I think that one should accept that the observation of large regions of N2O well above the tropospheric background at 25 km altitude is just not a realistic feature. This occurs in no other data set and I find it very hard to imagine a process which produces N2O in-situ in the stratosphere (see general remark above). With respect to the CRISTA N2O data, I would like to point the authors to a paper by Kuell

et al (JGR, 2005) in which the problem of trace gas retrieval from CRISTA data in the tropics is discussed, due to the high uncertainty in the temperature fields. These data are highly suspect and should not be used as a reference. There have been a number of high-preciscion in-situ measurement campaigns in the tropics, which all confirm that there are no N2O values above tropospheric background in the stratosphere. I think the only valid approach is to exclude these data from any further analysis before there has been a thorough validation. I am convinced that these values are just an instrumental artifact.

p. 27. l. 17 ff.: I think that the MIPAS 2003 must be considered an outlier when looking at the entire MIPAS data set (see Fig. 5) and should not be used for intercomparison purposes. Otherwise the 2003 (especially April) data would not be in agreement with the statement that interannual variations are low. Therefore I doubt if they are best suited for a comparison.

Section 5.3.: Taking into account that there are large uncertainties especially with respect to the ODIN N2O data, I wonder if a validation using N2O as one of the coordinates is a very sensible approach. One could actually find differences in ozone between different instruments which could turn out to be caused only by differences in the N2O-axis. I suggest sustaining all findings with respect to ozone by a direct intercomparision, e.g. on eq. lat. - pot.temperature coordinates.

P. 31.l.2.: The Odin values at 25 km are 10 ppb higher than the tropospheric values. Other data indicate, that a substantial fraction of N2O (typically about 15%) should already have been removed photochemically at this altitude. Therefore the difference is much larger than the stated 10 ppb, more in the region of 50-60 ppb.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 22629, 2012.