

Interactive comment on “Dynamically controlled ozone decline in the tropical mid-stratosphere observed by SCIAMACHY” by Evgenia Galytska et al.

Anonymous Referee #1

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This manuscript addresses the trend in N₂O, and the resulting trend in O₃, which has been observed in the tropical mid-stratosphere (30-35km) on decadal scales by several instruments. The overall SCIAMACHY measurements included here also show this trend in O₃, and the trend in NO₂ which one expects from the dynamical changes which drive the N₂O trend.

The significant contribution that this manuscript makes, is to show that, according to TOMCAT simulations, there is (from 2004-2012) an increase in Age-of-Air (AoA) in the tropical mid-stratosphere (30-35km) during some seasons, and a decrease in AoA during others. This result seems plausible, and offers the interesting possibility of

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changing N₂O (and hence O₃) in this region, while perhaps not changing AoA as much as might otherwise be expected.

While this is quite interesting, the authors have somewhat oversold the conclusion. They can conclude from their model that there is “no statistically significant trend in AoA”, but they cannot say that there is “no change in AoA” (in fact, there is a small overall increase in AoA in their model results). While I have no reason to doubt the model results, their explanations for why the seasonal variation in AoA causes N₂O and AoA change differently do not provide any useful insight. It is, of course, highly desirable to have a better understanding of the N₂O and AoA relationship, but unless the explanations can be greatly improved I would recommend dropping these from the manuscript.

I also have some serious concerns with the presentation of the SCIAMACHY measurements in the manuscript. The authors need to make very clear to the reader that, contrary to the model, they have not found any SCIAMACHY data which shows statistically significant increase in SCIAMACHY O₃, or a decrease in NO₂, during any particular month or season. It is certainly not appropriate that the measurements during the months when the model says that an increase in O₃ or a decrease in NO₂ should occur, and which shows no significant measurement trend, are relegated to the supplement, while at the same time the data during months when the opposite trends occur and the model and measurement trends agree (at least in sign and significance) are shown alongside the model in the main text.

More detailed comments (some of which repeat points from above):

Page 6 line 19 – “Global coverage of SCIAMACHY limb measurements was obtained within 6 days at the equator and less elsewhere.” It’s not clear to me what this means. Perhaps the authors are requiring some maximum distance between measurements. Unless the authors wish to provide a clear definition I would recommend dropping this sentence.

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Page 6 line 24 – “the errors of single measurements are mostly normally distributed and no additional issues with outliers have been reported.” I think this means that there was no need to remove outliers, but if this is the case please say this more clearly. If this is not the case then please rewrite the sentence to better explain what is meant.

Page 6 line 25 – “Consequently, we assumed that the random errors of zonal monthly means could be neglected.” Without knowing at this point how you are using the data it is hard to know whether this is reasonable or not. I would drop this sentence from here and perhaps make the point.

Page 7 – “In the latitudes between 50-60N and within altitude range 15-26 km we applied cumulative eddy heat flux instead of harmonic fit terms. We used ERA-Interim eddy heat flux at 50 hPa integrated from 45N to 75N with the time lag of 2 months.” I am not acquainted with this method. Do other groups do this? Is there a reference? If not, please give some explanation/justification.

Page 13 line 21 – “The absence of AOA changes in considered region . . .” This is a fundamental conclusion of the paper, but it represents an unjustified conclusion from the statistics. One cannot conclude from the absence of statistical significance that “there is no change in AOA”. One can only conclude that “there is no statistically significant trend”.

Figure 9 is particularly interesting.

Figure 12 – “There are no significant changes in SCIAMACHY measurements taken in February (see Supplements Fig. S4), therefore they are excluded from the figure.” One can’t simply include the SCIAMACHY measurements for a particular month per year when they fit the model, and then ignore them when they don’t. The SCIAMACHY NO2 results as shown in the supplement are almost significant at the 2-sigma level (they are certainly significant at 1-sigma) and are in the opposite direction of what the model shows. The easiest solution would be for the authors to conclude that the SCIAMACHY measurements, when plotted as one month per year, simply aren’t up to this, and

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therefore need to be dropped from this figure entirely. The SCIAMACHY results as shown in Figure 2 and 5 certainly do demonstrate the value of this measurements when they are not subsampled as in Figure 12.

Page 19 line 13- This paragraph purports to explain the absence of change in AoA. While it is certainly possible that one could have a change in N₂O and not a change in AoA, this point has not been proven. At the same time, the explanation seems to be simply a complicated statement of the fact that changes in N₂O are governed by changes in upwelling speed, which obviously couple to AoA. Unless the authors can offer some additional insight here I would recommend dropping this paragraph.

Supplement – The notation of SCIAMACHY, TOMCAT, and Insignificant is confusing, since the gray Insignificant lines can be either of the former two. The current notation obscures the important fact that the subdivided SCIAMACHY measurements never show a significant trend in the opposite direction to the overall trend in N₂O and O₃. I recommend using just green, blue, and, if the authors think it is helpful, a dotted version of these colored lines for an insignificant trend.

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