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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #2

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The authors aimed to understand the negative ozone change seen in the middle tropical stratosphere, and in doing so made the link that increases in NO2 as a result of dynamical changes were causing the loss of ozone in the region of focus around 30-35 km. However, they were not able to link this to a statistically significant change in the age of air, which is also an interesting result. Nevertheless, the importance of understanding multiple chemical and dynamical drivers in the stratosphere is highlighted and the authors present interesting results and raise questions worth investigating further.

However, my concern is that some of the points made, and hypotheses, are not well supported by what is presented, or the authors are not explicit and careful with how

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Discussion paper

they present results (e.g. correlation coefficient, below). I think this work is useful, and should be published, but changes are needed to make it explicitly clear what (i) can definitely be said from the observations, model and comparisons, (ii) what are the hypotheses the authors are putting forward, and (iii) what are the clear open questions that need to be addressed in future.

Comments: 1. I am in agreement with the other referee that the non-significant, even opposite signal (and sign of trend) in February, though non-significant (in the supplement), is not addressed head on. Data is often messy and difficult to deal with especially when comparing with a model, and should be presented front and centre even if there is a contradiction or lack of evidence to contend with. This actually requires a deeper discussion, because if the model disagrees with the data in the sign of the trend (and it appears consistent between NO_2 and O_3 in February in the supplementary materials despite the non-significance) then that raises questions that need to be highlighted (for example, is it a model or an observational problem?). I won't labour on this point further, or repeat points raised by the other referee, as the other referee has spent quite some time on points related to this.

2. Page 4, L20-23: is this relationship specifically in the 30-35 km tropical region of the study (see comment 2 below).

3. Page 4, L26: actually I would argue that the decrease Kyrola et al., 2013 found was up to 6-8% at its core (Fig. 16), which is more in line with that quoted for Gebhardt et al 2014. However, the core of the negative region in Gebhardt et al., 2014 is upward of -18% (Fig. 8). Could the authors be clear in what they mean here since I believe the -10% refers to the 20S-20N (Fig 7) profile; since the authors focus in on +/-10 deg. latitude region, the higher value seems more appropriate but then the estimate in this manuscript is almost 2x smaller. I assume, though perhaps the authors should check, this difference is due to a different time period and set of regressors used? At the very least please be explicit about what region the numbers represent and are comparable to the region focused on in this manuscript.

4. Page 8, L4-9: I am not sure I agree that it is consistent to ignore the monthly autocorrelation when using all months. It seems to me consistent not to use it for single-month (i.e. Jan only, etc) estimates (since there should be no autocorrelation between the months 12 months apart) and to indeed consider autocorrelation for the full time series since that is typically the case if they are next to each other in a continuous timeseries. This is only reasonable if you can state explicitly that there is no change in the significance - does considering it have an effect on your conclusions?

5. Page 9, L14: the inference the authors make from Fig.3 is that chemistry has little impact on the 30-35 km tropical region; for O3 and N2O I think this is reasonable. But for 3d-f it seems that in the box, NO2 is roughly split 70/30 or maybe 50/50 in the peak positive change. So it isn't clear to me if this statement is fully backed up by the plot (or perhaps its a non-linear interaction?). Please could the authors comment on this, perhaps with values.

6. While Fig 4a. shows a combined non-linear shape, it appears that the anti-correlation (linear slope for each level) reduces with higher altitude, being almost flat at 35 km (green). Why does this happen? Does this indicate that the mechanism proposed is no longer operating as efficiently in the upper part of the box?

7. Fig 6, 10, and all discussion related to the Rˆ2 statistic: this is very confusing and needs to be stated explicitly and correctly. Rˆ2 is formally the "coefficient of determination", which can be the square of, but not same as the "correlation coefficient". Further Rˆ2 can only range from 0 to 1, while the correlation coefficient can range from -1 to +1. Please check all instances of this and be correct in its usage; in many places this is confusing and leads the reader to have to try and work out what the authors mean.

8. Page 16, L5: 0.6 is an arbitrary threshold; please state this explicitly.

9. Fig. 11: is this also integrated over 10S-10N?

10. Page 19, L4-5: Is this a hypothesis or a demonstrable fact? I do not understand

why it is a limitation of the measurements, given the description earlier of the limb observations being well-distributed in the tropics and the period being considered is the same for the model data. If the effect is demonstrable, then this would provide good evidence the model is correct and why we don't have to worry about the insignificance and/or inverse correlations. If it is a hypothesis, please state explicitly this is the case.

11. Page 19, L16-22. I'm afraid I found this explanation difficult to follow. Please rewrite to be clearer. Is the summary that the N_2O "changes do not cancel in the yearly average" because photolysis has an affect that AoA is not impacted by?

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