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Comment on acp-2021-663

Anonymous Referee #2

Referee comment on "A Single-Peak-Structured Solar Cycle Signal in Stratospheric Ozone based on Microwave Limb Sounder Observations and Model Simulations" by Sandip S. Dhomse et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-663-RC2, 2021

In the paper „A single-peak-structured solar cycle signal based on Microwave Limb Sounder observations and model simulations" by Dhomse et al., the authors employ a multivariate regression on 16 years of ozone observations from MLS to derive the solar cycle signal. The same analysis is performed on model experiments with a chemistry-transport model driven by ECMWF re-analysis data, using different data-sets of solar spectral variability. They find a clear solar cycle signal with a single-peak structure and significantly higher amplitude than in previous estimates based on other data-sets, and they argue that this is due to a combination of higher sampling rate of the MLS data compared to, e.g., solar occultation instruments used in previous studies, and to less aliasing with other modes of atmospheric variability, in particular stratospheric halogens and volcanic forcing. They also find really excellent agreement between the solar cycle signals derived from observations and all model results shown, suggesting that all forcing data-sets used can be used in studies of solar-climate interactions. This is a careful analysis of an interesting and promising data-set concerning a very interesting (if difficult) question, and the paper is generally clearly structured and well written. However, there are two points that I think should be addressed before final publication. First of all, while the point raised about less aliasing with volcanic forcing and stratospheric halogen loading appears plausible, the fact that 24 fitting factors were needed for the QBO signal suggests to me that an aliasing with the QBO is possible, and this should be investigated / discussed in more detail. Second, both MLS observations and model results show consistent solar signals in the lower stratosphere with a distinctive latitudinal structure. This appears unlikely to be due to direct solar forcing, and more likely due to a dynamical feedback which would be implicitly included in the model results due to the use of dynamical fields from reanalysis data. This could be investigated simply by doing the same multivariate analysis on a model run with constant solar forcing which already exists, and I would urge the authors to do this. These points are discussed in more detail below, as well as a few more minor points.

Lines 100 – 102: I have been wondering here about the justification of using a chemistry-transport model driven by re-analysis data. Superficially, this could be understood to mean that the solar cycle signal derived from the model experiments is purely the chemical response of the atmosphere. However, any potential dynamical feedback in the atmosphere is implicitly contained by the use of the re-analysis data, and this will obviously also affect the ozone fields, by transport and by its dependence on temperature. But the reverse feedback, from the ozone fields to dynamics via radiative heating is suppressed to some extend by the use of prescribed temperatures and wind fields. Does that mean that the ozone fields and model dynamics are not fully consistent with each other? I'm not quite sure, but would have liked a discussion of this somewhere. On the other hand, using the same dynamical situations with and without variable solar forcing provides the interesting possibility to separate chemical responses from dynamical feedbacks. I'll come back to this later on.

Lines 155-162: Looking at Figure 1, it seems obvious that the QBO provides the largest source of ozone variance in most pressure layers, probably much larger than the comparatively small solar cycle signal. Considering that you fit 24 different QBO terms, and that the residual of the fit appears to be several percent, in the same order of magnitude, or even larger than, the solar cycle signal of 1-4 % that you derive (see Figure 2) – how confident can you be that the solar cycle signal is not affected by aliasing with the many QBO terms, or their superposition? Please add some analysis/discussion on this point. You could, e.g., show a comparison for the amplitudes of the different terms (QBO, linear term, SCS, volcanic), for the pressure levels given in Fig. 1, compared to the residuals.

Line 201-207, discussion of differences between regression analyses of the different model experiments, Figure 3: again, the largest mode of variability appears to be the QBO signal. I would expect that this is very similar in all model experiments, and that results of D_SFix would be very consistent with the other model experiments here. Why not include those in the Figure? On the other hand, there are quite significant differences between model results and observations in 31 hPa and 100 hPa. Can you discuss where those derive from? As model results A_NRL, B_SAT and C_SOR are nearly identical in these pressure levels, so presumably not due to any chemical solar cycle signal. To highlight the impact of the solar cycle, you could show the differences A_NLR-D_SFix, B_SAT-D_SFix, C_SOR-D_SFix on these pressure levels as well.

Lines 208 – 222, discussion of Figure 4: I had to look very carefully at figure 4 to ensure that the results from the different data-sets, and particularly MLS and NRL2, are not identical. The agreement of the patterns of positive/negative SCS between observations and model results is really striking. However, you mention in the text that results are not statistically significant everywhere. Could you a) describe how statistical significance was derived (e.g., by explaining the errors shown further up), and b) somehow mark regions of significance in the figures? Also, you show in Figure 3 that the model results at 31 hPa and 100 hPa are virtually identical, but the regression results shown here are not – why is this the case? Does the fact that the model results are nearly identical mean that there is no chemical solar cycle signal, or just that the chemical solar cycle signal is identical in the lower stratosphere? This is not possible to see from the results shown, but could be obtained by comparing to results of the model run D_SFix. So again – could you add results of D_SFix and differences, to Figure 3 as suggested above? Anyway I would expect that any solar cycle signal in the lower stratosphere is not a direct response of the chemistry, but due to dynamical feedbacks which the model experiments implicitly consider by using the re-analysis data. This could be tested by performing the same multivariate analysis of the solar cycle signal on the model run D_SFix; any statistically significant SCS signal derived from this must be due to dynamical feedbacks. My expectation would be that this looks very much like the other model experiments in the lower stratosphere, but shows no (significant) solar cycle signal in the upper stratosphere and mesosphere, where this is probably due to direct response of the chemistry. I would also not be surprised if the strong signal in the Southern high latitudes was a dynamical feedback (via vortex strength). So – please perform the same analysis on the model experiment D_SFix, and show / discuss results in Figures 3 and 4.

Minor points:

Line 16-19: “compared to earlier estimates” – I don’t disagree with this sentence, but struggled with it nevertheless. I think the important differences are, on the one hand, much denser sampling of the observations by MLS independent of solar illumination, that is, also covering high; on the other hand, observations during a different time-period with (possibly) simpler background conditions leading to less (obvious) aliasing with other terms of variability.

Lines 155-156, figure 1: you could provide the correlation coefficient as a measure of the quality of the fit. As the multivariate regression is essentially a multi-linear regression, Pearson’s correlation coefficient is well suited for that.

Line 163, Figure 2: what is the meaning of the error bars? Are the derived from the error covariances of the multivariate regression, or from the variance within the sample, or both? Please explain.

Line 166-167: You could / should discuss this around the error bars you provide for the MLS based SCS: In 35-45 km, the MLS based SCS are significantly higher than the previous results, with the results from previous estimates outside the error range of the MLS based SCS; the best agreement is observed for HALOE SCS, which is just at the edge of the lower error bound of the MLS SCS. In 50-56 km, MLS SCS and HALOE SCS agree within error bounds, but are significanty lower than the SAGE II SCS. In 20-30 km, all data-sets yield consistent results within the error bounds of the MLS SCS.

Line 167 – 168: “A key feature is that the MLS SCS … is almost twice as large as any other satellite-data based SCS reported in the past” … considering that this was derived over a comparatively weak solar maximum, this result is somewhat surprising.

Line 173-174: Additionally to the sparse sampling, the solar occultation instruments only measure during a very specific time of day, sunrise and sunset, while MLS measures independent of solar illumination. This is probably not important in the lower and mid-stratosphere as ozone does not have a significant diurnal cycle there; but could it affect results in the mesosphere and uppermost stratosphere, where a diurnal cycle evolves?

Line 185-187: the enhanced stratospheric aerosol also leads to lower ozone values, which will have an impact on the regression results I guess.

Line 188-190: agreed that SSI changes could have been different from earlier solar cycles, but would you expect a larger amplitude of the solar cycle signal for the weaker cycle?

Line 198: “is most probably due to …” I would formulate this a bit more carefully. Maybe “is likely due to …”?

Line 200: why not do the same regression analysis as well for D_SFix? This would enable you to separate the purely chemical solar cycle signal from the implicit dynamical feedback contained in the use of re-analysis data.

Line 218-219: for instruments depending on solar illumination, high latitudes are naturally difficult, in particular as they would certainly miss polar night.

Lines 223 – 225: again, how are the error bars derived ? And – all model results seem to agree with MLS within error bars over the whole range shown (though that is difficult to assess in the figure), so the differences should probably not be overinterpreted.

Figure 6 – can you change “plev” on the y-axis to “pressure (hPa)”?

Line 251: … significant variations “of the” ozone difference patterns …

Line 264-265: just as a suggestion for the future – would it be possible to include other instruments measuring independent of solar illumination into the analysis, e.g., MLS/UARS, MIPAS/ENVISAT, SMR/ODIN?