Review of the manuscript “Separating the role of direct radiative heating and photolysis in modulating the atmospheric response to the 11-year solar cycle forcing” by Bednarz et al.

Reviewer: Rémi Thiéblemont

This paper examines the thermal and dynamical responses of the tropical and Southern Hemisphere polar stratosphere to changes in solar irradiance using sensitivity experiments of the chemistry-climate model UM-UKCA. The aim of the paper is to separate the effect of the photochemistry and radiation module (that are artificially separated in models) on the solar signal and to explore the linearity of the stratospheric response due to the photochemistry and radiative module contributions. They found that the response is linearly additive in the tropics but not in the polar region and proposes mechanisms to explain the non-linearity in the polar region.

The issues that the paper is addressing have long been debated and the results of the paper constitute an added value toward a better understanding of the impact of the solar variability on the stratosphere (and thus potentially on climate) but also on the importance of model design on the representation of the solar variability-induced effects. The main findings of this paper are novel and constitute an interesting scientific contribution in my opinion. I find the manuscript also well-structured and well written. However, I have some concerns with some interpretations which seem to me somewhat speculative since I don’t find that they are convincingly supported by the results. Some statements should hence be tone down unless additional analysis (or experiment) are carried out. Therefore, some revisions of the paper are needed before I recommend it for publication in ACP. I also have several questions for the authors. Please find the details of my comments below.

Main comments:

1/ Given that all results and conclusions of this paper are based on timeslice experiments performed under permanent max or min solar conditions, I do not find that it is appropriate to claim that the study investigates the “… atmospheric response to the 11-year solar cycle forcing” (title of the manuscript). This is misleading for readers and should be formulated differently in the title, the abstract, but also everywhere in the manuscript where required (at several places). It could instead be mentioned that the study investigates the atmospheric response to constant changes in solar forcing that correspond to the amplitude of the 11-year solar cycle. Or something like this. Note however that I fully understand the arguments and agree with the benefit of using timeslice experiments instead of transient experiments in this paper.

2/ Presently, the mechanism that is proposed on Fig 8 is not clear to me. In particular, the paragraph and analysis describing the mechanism in link with the changes in wave activity (P15,L24-P16,L2) seem speculative in my opinion. For instance, the only actual significant signal in the wave activity diagnostics (S3 and S4) is seen during 2 months over the July/August/September period for the PHOT-only experiment. The other experiments do not show statistical evidence of changes. What the analysis reveals is that the PHOT-only experiment shows an increased wave activity entering the stratosphere (S3) and increased westward forcing of the mean flow in the upper stratosphere by wave breaking (S4). This is associated with an acceleration of the stratospheric overturning circulation which brings more ozone to the polar region. This could come from background changes in the stratosphere (due e.g. to the changes in the SWHR gradient as claimed but that may come also from other processes), but also to changes in the wave excitation in the troposphere (see comment 3/). Attributing these wave changes to the SWHR gradient is to me not yet supported by robust evidences. Although I understand that making additional extended analysis may not be easily feasible or wanted, you may consider examining the monthly evolution of the wave activity (amplitude, propagation, …), Brewer-Dobson circulation, wind, SWHR,… to explore the seasonal march of the signal: such analysis may help identifying more clearly some causality. You may also consider examining the refractive index to see if the SWHR changes affect the propagation conditions of wave. Finally, I think that it may be interesting to examine more in details and possibly show how the inter-annual variability behaves for these various quantities. Are the changes in PHOT-only the result of a few years with an "extreme" behavior - for instance possible SSWs in the Southern Hemisphere - or rather the result of more permanent/continuous changes. As it is claimed that the initial source of perturbations in wave activity start from the changes in the SWHR in winter in the tropical region (where the perturbed vertical profile should not experience too much inter-annual variability), I would expect the changes in winter to be rather continuous. The mechanism in spring is much clearer (more ozone in polar region => changes in SWHR gradient, etc) but largely depend on the winter circulation perturbation that is presently not easy to understand.

3/ In light of my previous comment, I wonder if some of the identified changes between the PHOT-ONLY vs. RAD-ONLY & INTERO3 may not partly come from the fact that (if I understood correctly the experimental design) PHOT-ONLY MAX/MIN pair has a constant TSI-induced heating (since the radiation module solar forcing is fixed) while this is not the case for the two other experiments since between MAX & MIN conditions, the TSI-induced changes are considered in the radiation module. Could that lead to some bottom-up residual influence (even if the SSTs are fixed) and contribute to some of the identified differences? Is there a way to diagnose this? Do you think that this could have an influence?

Specific comments:

P1, L21-24. As described in comment 2, I am not convinced yet that the SWHR gradients in winter play an important role.

P2, L26-28. Please give one or two example of "…specific aspects of model design" to make the issue more concrete.

P3, L11-12: Indeed, the SH is less studied than NH, but there are clearly more studies than the one cited here (e.g. Petrick et al. (2012, JGR), see also numerous studies of Yuhji Kuroda and co-authors (the most recent by Kuroda was published this year in JGR)). Please cite some.

P3, L25: the "main" or the "only"?

P3, L21-P4, L7. These 2 § are misleading since they leave the impression that transient simulations are performed. For instance, it is mentioned that HadISST are used and an 11-year forcing is implemented while it's not really the case. The authors should make clear from the beginning that they do idealized experiments that look at SMAX-SMIN conditions of the amplitude of the 11-year solar cycle. Presently, it is too confusing in my opinion.

P4, L23-24 "The third pair, PHOT-ONLY SMAX/SMIN, is analogous to RAD-ONLY SMAX/SMIN, but the solar cycle forcing is included exclusively in the photolysis scheme while constant TSI and SSI are used in the radiation scheme.” As mentioned in the main comment 3/, could the TSI change between SMAX and SMIN in the RAD-ONLY also be responsible for the observed difference in the signals? Would it not have been an option to keep always the TSI constant? Note that this would have also made the use of the same fixed climatological SST prescribed for SMAX and SMIN more adequate in the case of the RAD-ONLY and INTERO3 experiments.

P6-7, section 3 and Figure 1. It would be relevant, I think, to calculate also the statistical significance of the difference between the RAD+PHOT & INTERO3 responses (panel d of Figure 1). This would strengthen the results and help motivating the analysis that are carried out later on in the paper.

P8, L20-24. That is very interesting to notice. Could that be due to the extraction method too used in the case of transient experiments and reanalysis and e.g. the difficulty to separate the solar signal from contributions of other variability factors? (see e.g. Chiodo et al., 2014, ACP)

P9, L15-16. It appears from Figure S1 that Chapman dominates over NOx. This could be clarified in the text by giving the contribution of each e.g. in %.

P9, L27. The “small overestimation” is also statistically significant at the 2-sigma level near the peak at ~36 km. That means that even in the tropic, the RAD+PHOT contributions are not exactly linearly additive. This is I think still important to highlight and it shows that the complexity of the system needs to be accounted for, even in regions where we usually believe that the response is simple. Of course it’s not major, but still worth mentioning.

P10, L19-23. What about the comparison with the results of Bednarz et al., 2018? Does the comparison between the timeslice and transient experiments help to get further understanding of the opened issues listed here? As you refer to this comparison previously in the manuscript (P8, L20-24), it may be worth looking at it again here.

P10, L26-30. It may be relevant to add the climatologies to the plots (as black contours on the background similarly to Fig. 1d). That would help to better visualize the jet strengthening and eddy driven jet displacement.

P11, L4-7. Did you also look at the inter-annual variability? Is there a difference between the different runs? Are there SSWs–like perturbations (even though they should be rare in SH) that may be responsible for the SH easterly anomalies of the PHOT-Only experiment?

Figures 5 and 6. Similarly to Figure 1d, I think that the statistical significance of the differences could be relevant to show here.

P15, L19-20. Please indicate the altitudes of the peak (lower mesosphere, upper stratosphere is somehow vague). Where in the mesosphere does SWHR peak in RAD-Only (is 60 km the maximum?)?

P15,L24-P16,L2 & schematic on Fig. 8. As explained in comment 2/, that paragraph is not clear and too speculative to me. Please consider either making further analysis to support the present discussion or just tone down.

P16,L6. Instead of “primary driver”, I would rather say that it’s considered as the “initial driver”.

P16, L32-P17,L3. The arguments in this paragraph are not really convincing to me, since despite the fact that SWHR gradient are additive in JJA, the temperature and wind responses are not.