RESPONSE TO REVIEWER 1

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.

We thank the Reviewer for the positive review and constructive comments that have improved the manuscript. Our replies to the individual comments are shown below in blue.

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.

We have changed the manuscript (both the abstract and main text) to make it clear that we investigate the response to the amplitude of the 11-year solar cycle forcing using an idealised timeslice setup. We have also changed the title to "… to the amplitude of the 11-year solar cycle forcing".

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.

We agree with the reviewer that our explanation of the winter mechanism is more speculative than for the spring one; we have tried to stress that in our manuscript (see end of the last paragraph in Sect. 6.1 of the old manuscript version, which reads: "The details of this sensitivity are, however, difficult to diagnose using our experiments and this hypothesis should be subject to further examination") and we are sorry to hear we failed to convey this message more clearly. We have analysed the monthly evolution of specific quantities to examine the seasonal march of the signal, but identifying clearly and confidently the initial trigger is not easily possible as the monthly mean results are fairly consistent with each other. A more confident attribution of the initial triggering process responsible to the SH dynamical response in PHOT-ONLY would involve performing more specifically designed sensitivity simulations, which is beyond the scope of this manuscript. We do, however, think that our results at present constitute an important motivation for investigating the role of solar-induced ozone feedback in more detail, as it is a subject that has not been thoroughly acknowledged in previous literature.

We have now changed the manuscript, as to make this even clearer: we have toned down some of the statements about the mechanism responsible for the winter response, and stress that our suggestions/hypotheses should be followed up with further sensitivity experiments. Also, we include a discussion of an additional potential triggering process, i.e. the role of zonally-asymmetric ozone heating in modifying the wave-mean flow interactions. Evidence of the role of such ozone heating in modulating the NH polar vortex has been shown in the literature (e.g. Nathan and Cordero, 2007, Kuroda et al., 2007, 2008, McCormack et al., 2011, Silverman et al., 2018). It is plausible that the increased ozone levels in PHOT-ONLY have a similar effect in our study, with the zonally-asymmetric component of ozone heating being most important in early

winter (as opposed to the zonally-symmetric one in spring described in Sect. 6.2 of our manuscript due to increased ozone levels at high latitudes).

We have also investigated the interannual variability in the August zonal wind anomaly, and we include the histogram below to the Supplement and refer to it in the text. As shown in Fig. R1 below, the integrations suggest that it is both the mean behaviour and the extremes that shift, although longer model runs would be required to distinguish better differences in the distributions, especially in their tails.

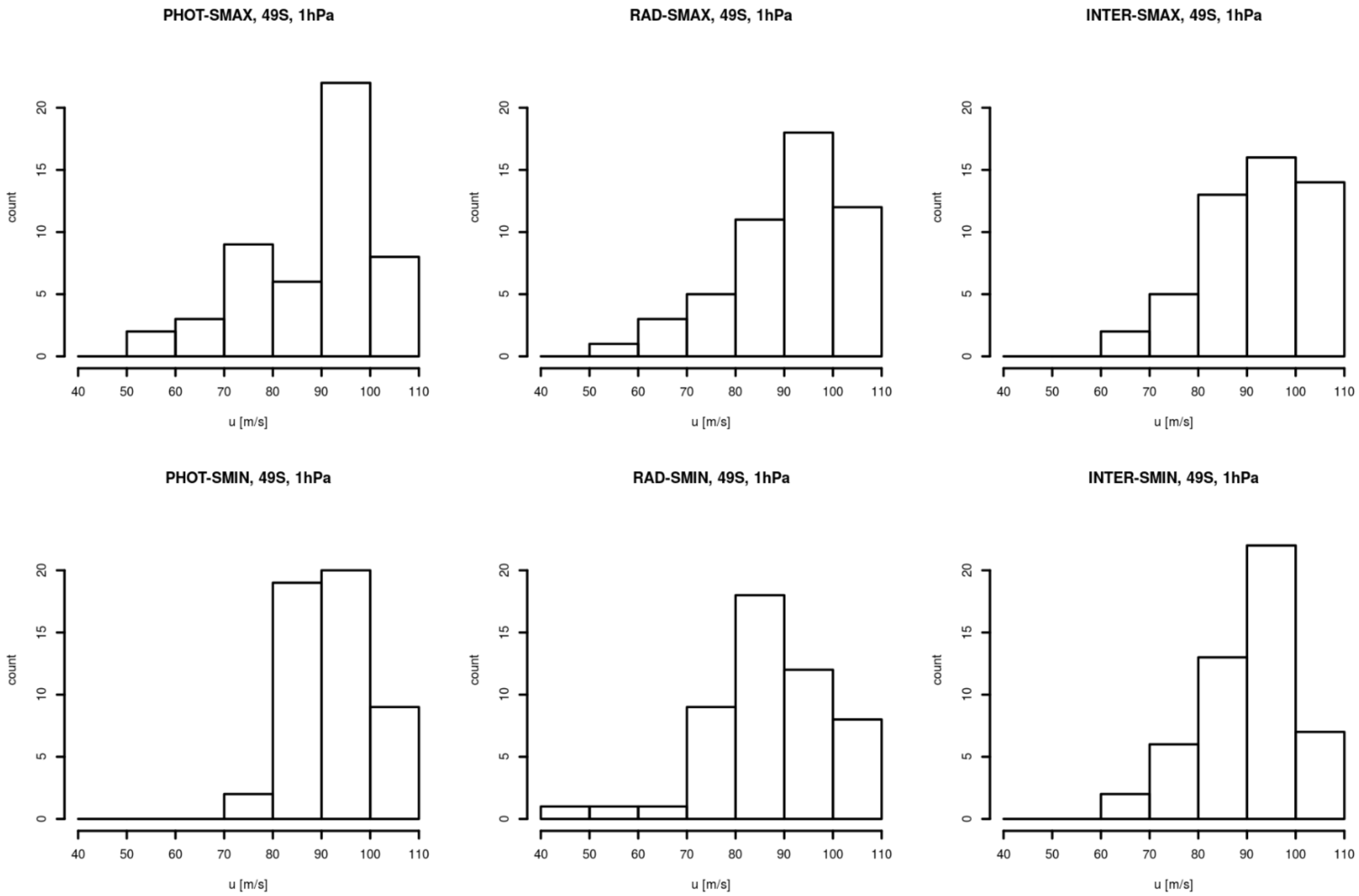

Figure R1. Histograms of August monthly zonal mean zonal wind [ms^{-1}] at 1hPa and 49°S in the model experiments for (top) SMAX and (bottom) SMIN. The panels show (left) PHOT-ONLY, (middle) RAD-ONLY and (right) INTERO3, respectively.

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?

This is an interesting suggestion. The fact that SSTs are prescribed and fixed in the experiments diminishes substantially the bottom-up response as only land temperatures can adjust. Hence,

the mechanisms for a bottom-up response to solar forcing which have been discussed in the literature will largely not be active here, e.g. a response in the tropical Pacific SSTs and links to the Walker and Hadley circulations. We note that the yearly mean SMAX-SMIN zonal mean temperature changes simulated in the troposphere are very small (Fig. 1). To remove entirely any bottom-up response would require us to fix land temperatures, which is very difficult to implement in the HadGEM3 model. Hence, we cannot rule out a potential role for a bottom-up influence, although the analysis of the experiments points to this being less important than the top-down influence from the stratospheric changes.

The radiation code takes TSI and partitions it into the shortwave radiation bands; hence it would be difficult in this model to keep TSI fixed whilst altering the distribution of solar energy across the UV part of the spectrum.

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.

We have changed the abstract in line with our response to the Reviewer's main comment 2 above.

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

We have included a couple of examples, i.e. the resolution of the radiation scheme and the height of the model top.

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.

We have added a citation to Petrick et al., 2012; Kuroda et al. and Shibata, 2006, Kuroda et al., 2007; and Kuroda and Deuschi, 2016.

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

This sentence now reads: "Unlike in Bednarz et al. (2016), however, the model version used here does not include the coupling of stratospheric aerosols with the radiation and photolysis schemes."

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.

As per the response to the Reviewer's main comment 1, we have now rephrased the text to clarify the scope of the model experiments and their design.

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.

See the response to the Reviewer's main comment 3. We also note that the use of fixed TSI in the radiation scheme is not straight forward to implement in our model at present as the change in partition of solar spectral irradiance over the shortwave radiation wavelength bins varies as a function of TSI.

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.

Considering the standard error associated with the RAD+PHOT response defined as a square root of the sum of squared standard errors associated with each RAD-ONLY and PHOT-ONLY responses, we find that the confidence intervals (±2 standard errors) around the RAD+PHOT and INTERO3 overlap. Therefore, the difference of these responses is not significant in a strict statistical sense. We now state that in the manuscript.

We note, however, that combining the errors associated with each of the RAD-ONLY and PHOT-ONLY responses by construction leads to broader confidence intervals than it is the case for each individual experiment pair alone, since each is affected by internal variability. Hence this is a more difficult criterion to pass.

Nevertheless, the yearly mean temperature difference between RAD+PHOT and INTERO3 in the SH high latitudes shown in Fig. 1d does largely exceed ±2 standard errors of the INTERO3 response. We have added this to the manuscript.

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)

Indeed – this is partially what we refer to when noting possible contributions from interannual variability in that sentence.

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 %.

We have added this to the manuscript.

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.

We have changed this sentence to: "There is some overestimation of the summed response compared with the control case; this illustrates that stratospheric ozone concentrations are controlled by a range of photochemical processes, thereby resulting in a complex dependence of the SMAX-SMIN ozone response on the associated temperatures, incoming wavelength-dependent solar radiation as well as any resulting changes in ozone columns above."

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.

The SH dynamical response diagnosed in the ensemble of transient runs described in Bednarz et al., 2018 consists of a poleward shift of the SH polar vortex in austral winter and its weakening in spring (not shown). As this behaviour could result from the difficulty of separating the solar cycle response from the effect of other time-varying drivers, e.g. GHGs and/or ODSs, we refrain here from making a comparison between these idealised timeslice runs with all forcings except solar held fixed and the transient experiments with varying GHGs, ODSs, SSTs, sea-ice and stratospheric aerosols.

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.

We have now added the climatologies.

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?

See the response to the Reviewer's comment 2. We now include the histogram shown above to the Supplement, and we refer to it at the end of this paragraph.

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

As it was the case with the yearly mean SH high latitude temperature response in Fig. 1d, the ±2 standard error confidence intervals around the RAD+PHOT and INTERO3 responses overlap. Thus, the difference of these is not significant in a strict statistical sense. We now state that in the manuscript. We note that the differences between RAD+PHOT and INETERO3 in Fig. 5 and 6 are nonetheless largely big enough to exceed the confidence interval (±2 standard errors) around the control INTERO3 response.

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?)?

We have added this to the manuscript.

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.

Please see our response to the main comment 2.

P16,L6. Instead of "primary driver", I would rather say that it's considered as the "initial driver".

We prefer to stick to saying 'primary driver' as to indicate that this driver is usually considered as the main, if not the only, driver of the solar response during the whole dynamically active season.

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.

As described in the manuscript, the non-additive nature of the temperature and wind responses must reflect contributions from dynamical processes which could be part of a non-linear response, as we discuss, and/or with some contribution from internal variability. We do point out that the magnitude of the non-additive component of the temperature and zonal wind response in JJA (Fig. 5) is relatively small here.

RESPONSE TO THE REVIEWER 2

The manuscript attempts to disentangle the contributions from direct radiation and chemical effects of the solar irradiance. The authors applied the chemistry-climate model (CCM) UM-UKCA to simulate the steady-state atmospheric state for the solar maximum and minimum conditions. To separate the role of different processes and estimate the linearity of the overall response, the authors performed three pairs of experiments switching on only the direct influence of solar irradiance on radiation (RAD-ONLY), photolysis (PHOT-ONLY) and both (INTERO3). Several similar studies have been published before. The novelty of the manuscript consists of the application of more sophisticated model and the results concerning the non-linearity of the two considered processes during the southern hemisphere cold season. This conclusion is important for the community because it emphasizes the necessity of the interactive ozone treatment in the climate models. However, this important conclusion is also a weakest part of the manuscript because the identification of the responsible mechanisms is not convincing. This issue should be clarified before the publication of the manuscript, otherwise this important conclusion will not be fully appreciated.

We thank the reviewer for the positive review and constructive comments that have improved the manuscript. Our replies to the individual comments are shown below in blue.

Major issues

1. The chain of physical/chemical processes leading to the weaker polar vortex during SON is not convincingly presented. From the presented results, it is more or less clear that the story should start before the winter time. The gradient in heating rate for the PHOT-ONLY case should related to ozone gradient. It cannot be dramatically different from RAD-ONLY case, because the ozone increase inside polar vortex due to enhanced solar UV should not be large. During SON the obtained gradient in ozone is high, but I think it is rather related to dynamical processes during late winter. This dynamically induced increase of the ozone in the stratosphere produce strong heating rate gradients during SON and produce further suppression of polar night jet. Thus, the triggering process is not identified leading to weak understanding of the obtained results. I do not know which process can be involved, but I think the authors should try hard to find it.

Please see our response to the Reviewer’s 1 main comment 2.

Also, we note here that our hypothesis about the role of shortwave heating rate gradient in winter is based on the altitude of the maximum gradient change, which differs between PHOT-ONLY and RAD-ONLY due to heating rate differences in the tropics (See Fig. S1a of the old Supplement)

2. The linearity of the atmospheric response to radiation and chemical processes was discussed in several previous publications. Maybe it is better to concentrate on discovered no-linearity in the southern hemisphere and makes the description of the annual and tropical mean responses shorter.

We do try to concentrate on the SH dynamical response in our manuscript, however we think that some description of the annual and tropical mean response is useful here as (i) it shows that as far as the aspects of the stratospheric response to solar forcing already discussed in other studies our UM-UKCA response is not contrastingly different, (ii) we note a few points less frequently discussed in the context of the solar cycle before, e.g. the role of different chemical cycles for the solar-cycle induced ozone response, or the fact that while the SW heating rate response in PHOT-ONLY is higher that RAD-ONLY in the upper stratosphere, the corresponding temperature response there is lower (thereby illustrating the contribution of longwave heating rate change and any indirect dynamical processes in determining the tropical temperature response to the 11-year solar cycle). We have nonetheless attempted to shorten this section.

Minor issues:

1. Figure 1: Statistical significance is missing on panel d).

See our response to the same point raised by Reviewer 1 above.

C22. Section 4.1: I recall the contribution of radiation and ozone effects were analyzed in Forster et al. (2011). Maybe it makes sense to mention this paper?

Forster et al. (2011) is a stand-alone version of Chapter 3 of SPARC (2010), and we cite this study in the manuscript.

3. Page 10, Section 5: I think it is pointless to carefully compare the results of time-slice model runs with permanent solar max/min conditions with observations and try to explain the difference.

We agree but we do nonetheless make a brief comparison here in order to put our model results into context.

4. Page 15: The explanations in the last paragraph are too vague and not instructive to my taste. These processes definitely exist, but it is not easy to illustrate how they work.

Please see our response to the Reviewer's 1 main comment 2.

5. Page 21, line 10: I wonder how it is possible keep this paper under review for already 3 years. It seems something is wrong with it. I would not site unpublished papers, because the results could be wrong.

We have removed this reference.

6. Page 23, line 3: I do not see clear time line of the changes. It looks like triggering mechanism is missing

Please see our response to the Reviewer's 1 main comment 2.