Author response to second review Anonymous Reviewer 2

Overall the Authors have done many corrections on their paper, and have addressed most of my minor concerns satisfactorily. Some of the responses to major concerns are not satisfactory, especially regarding the novelty of the analysis. My overall conclusion is that while I do not necessarily agree with some of the implementations of methods, there is nothing fundamentally wrong with the analysis either (it is just not as rigorous as it could be). But, the paper provides only a small incremental advance to the field, and is not what I would call novel. As such, I do not believe it warrants publication in this journal.

I will not go through all the major point responses from the author, only the one regarding novelty, which the authors state is summarised in the introductory section with 4 points. I will take the 4 points separately, and explain why I believe the paper is only an incremental advance.

We respectfully disagree with the reviewer's resolute view that the manuscript is not sufficiently novel for publication in Atmospheric Chemistry and Physics. Our justification for this position is given below in blue.

1. To provide an update to previous CCM studies by analysing the SOR in CCMI-1 models.

Hardly ground breaking science. All that has been done here is using very similar methods to very similar data, and unsurprisingly getting very similar results. The previous similar studies were only a couple of years ago – models haven't advanced that much in two years.

The solar-ozone response in CCMs (CCMVal-1) were first analysed more than 10 years by Austin et al. (2008). CCMVal-2 results were briefly described in SPARC CCMVal-2 (2010) report but were never published. Hood et al. (2015) analysed a very limited subset of CMIP5 models (3 independent models that included a realistic representation of solar variability). The progress in the general performance of CCMs is evident if we compare CCMVal-1 and CCMI for other measures such as long-term ozone trends (e.g. Dhomse et al., 2018). Therefore, it is important and new to document the solar component of their performance. Getting the same results is not a problem. It just confirms that the solar response is robust. It is new and should be done, because otherwise one could not say whether or not the response to solar variability in the models is the same a decade after Austin et al. (2008). We have amended the Introduction to make this point clearer.

2. To evaluate the SOR in three pre-calculated ozone databases for climate models from CMIP5, CMIP6 and Bodecker et al.

Yes, but this analysis has already been performed for two out of these three datasets, so this is wildly overstating the novelty. It is new for CMIP6, but again, see my response to point 1….

The extracted solar-ozone coefficients from CMIP6 are a valuable dataset for other modeling projects. For example, the coefficients described in the study have already been used in PMIP4 in order to have a consistent representation of the solar-ozone response between PMIP4 and CMIP6 (Jungclaus et al., 2017; Bader et al., in preparation). It is therefore important that this paper documents the CMIP6 solar-ozone coefficients that are being used by other parts of the community. The coefficients are now being published along with the manuscript as a dataset (https://doi.org/10.5518/348), which is available to other researchers. The comparison with CMIP5 simply helps to put the latest dataset into the context of previous work. The citation to the ozone coefficients dataset has been added to the manuscript.

3. To compare CCMs and ozone databases with satellite observations from Part 1.

But this is exactly what Hood et al, 2015 did. They compared satellites with CCMs, and with nonchemistry models. Clearly seen in their figures and analysis. As Hood is an author on this paper, perhaps Maycock and Hood can discuss this and present an answer. At the moment, I see very little novelty (other than, yet again, the application to slightly different data sets, but you've already covered that in points 1 and 2).

Hood et al. (2015) did not include the latest satellite datasets. They used SBUV VN8.0 and SAGE II VN6.2; the latter dataset has been shown to have significant limitations in its representation of solar variability (Maycock et al., 2016; Dhomse et al., 2016). We use the latest version SAGE VN7.0 extended to the recent past for comparison with the models. This point has been added in the Introduction. If the results are similar to previous findings it is not so important, because we did not know this would be the case a priori.

4. To perform atmospheric model experiments….

This is certainly the most novel aspect of the paper, and in my view by far the most useful part. Model responses to the simulated atmospheric response (even with the same ozone prescribed) are widely different though, so for this to be a robust analysis, the experiments should be performed in at least one other model.

We show that in a CMIP5/6 model that the changes in solar ozone response have a first order effect on the simulated temperature response to the 11 year solar cycle. This result could vary slightly from model to model, but basic radiative transfer says that imposing a solar-ozone response that is almost half the amplitude in the upper tropical stratosphere will result in a smaller temperature response, so there is no reason to suspect that the model is inconsistent with that effect. The CMIP6 solar ozone coefficients are now being published with the manuscript (see point 2), and are therefore available to other groups to perform their own model experiments to check this result.

References

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