Author response to review by Joanna D. Haigh

The representation of solar cycle signals in stratospheric ozone. Part II: Analysis of global models" by Amanda C. Maycock et al.

The paper presents an analysis of the responses found in the ozone fields of coupled chemistry-climate models to specification of solar spectral irradiance, how these compare to the ozone fields prescribed in IPCC climate models (and to the signals found in observational datasets). The work is carefully planned and thorough and provides a suitable background against which future work can be planned and studied.

We thank the reviewer for her supportive comments on the manuscript. We reply to her specific points below.

Minor comments

l.44 Tell reader that Maycock et al. (2016) is Part I
Added

l.121 Why only one ensemble member? The modellers have done several to provide you with stats!
We have updated the analysis to use all available ensemble members for the models.

l.179 ref l.671 Matthes (2017) now published (though with a weak explanation for the political expediency involved in the averaging of two datasets!).
Reference updated

l.184 Clarify "up_0.3%": presumably not 0.3% of signal but 0.3% on top of c. 2% (?)
Text has been changed for clarification

l.233 Have you looked at the impact of this choice of AR model (cf none or AR(1))?
Figure R1 below shows the decorrelation timescale for the MLR model residuals. The e-folding time is >2 months in some regions of the middle and lower stratosphere. Figure R2 below is as in Figure 3 of the main text but assuming no AR model for the residuals. The results are similar to those using an AR(2) model, with the main exception found in the polar lowermost stratosphere. Therefore to avoid giving potentially misleading information about the SOR in the polar lowermost stratosphere we have restricted the plots in the revised manuscript to a maximum pressure of 100 hPa.

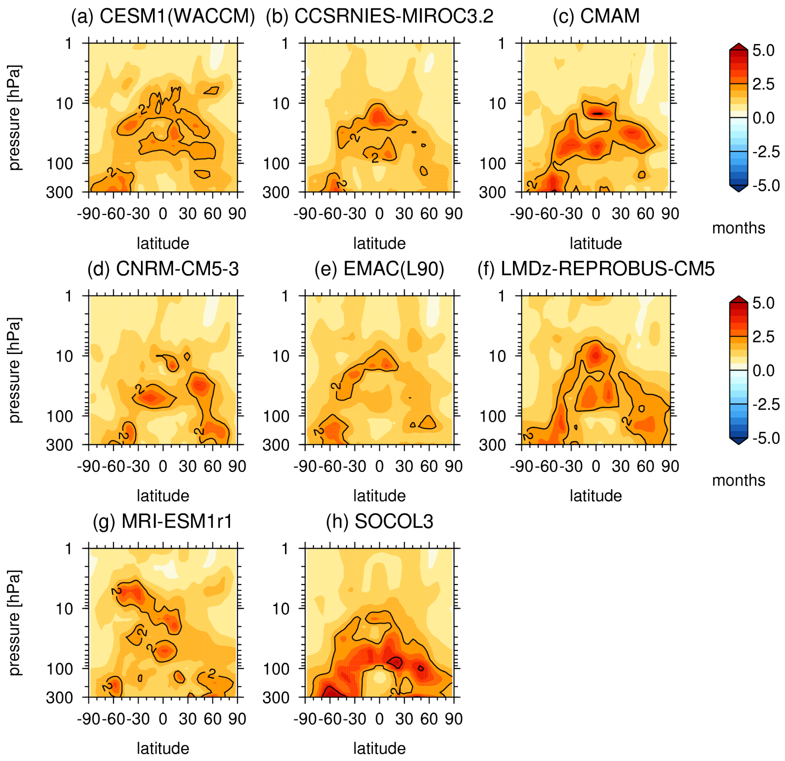

Figure R1: e-folding time [in months] of the ACF in the MLR residuals for the CCMI-1 models.

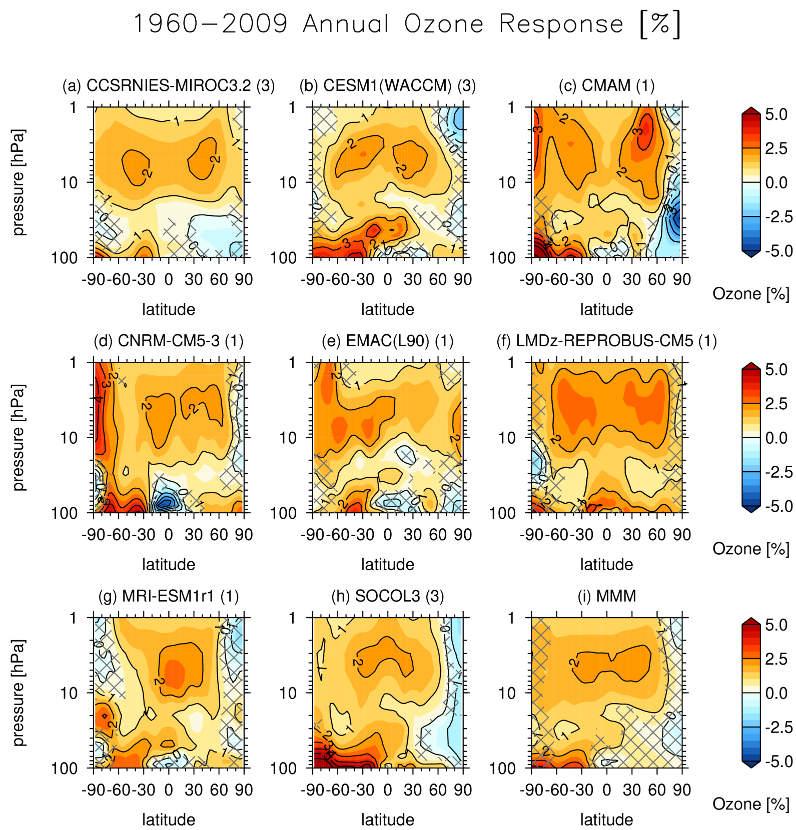

Figure R2: As in Figure 3 of the main text but assuming no AR model.

l.300, l.330 and elsewhere. Comparison to observational results of Part I interesting but difficult to extract from this text.

We have added timeseries of ozone anomalies from two satellite observation datasets described in Part I to Figures 2 and 4 to facilitate the comparison with results from Part I.

l.391-2 Indeed! Can you make any judgement on what is causing these differences between datasets?
l.491-494. Any conclusion on why these models produce a signal in the tropical lower stratosphere?

Both comments relate to the presence or absence of a significant SOR in the tropical lower stratosphere. There are some methodological sensitivities to the robustness of this feature. For example, this is one of the main regions where sensitivity to the choice of AR model is seen. This is particularly found in models where the regression residuals show long autocorrelation timescales in the tropical lower stratosphere (see e.g. SOCOL3 in Figure R1 above and compare Figure R2(h) and Figure 3(h) from the main text). Consequently, the estimated uncertainties in the magnitude of the SOR in the tropical lower stratosphere are larger than in the upper stratosphere (see e.g. Figure 6). In the revised manuscript, we have also altered the approach for accounting for volcanic effects by excluding 2 year periods following eruptions rather than including a volcanic term in the MLR. This also has a modest effect on the SOR in the tropical lower stratosphere in some models (compare Figure 3 in revised manuscript with original Figure 3).

Aside from the above methodological issues, additional analysis (not shown) has been performed on the Transformed Eulerian Mean residual vertical velocity fields for the models that provide this data for the refC1 experiment (CCSRNIES-MIROC3.2, CMAM, EMAC, MRI-ESMr1, SOCOL3). There is no evidence of a significant weakening in tropical lower stratospheric upwelling in the models that show some enhancement in the SOR in this region, as has been suggested in some earlier studies.

The following text has been added to the manuscript:
"One of the CCMI-1 models (SOCOL3) appears to show an enhanced SOR in the tropical lower stratosphere, which is similar in amplitude to that seen in some CCMVal-1 models. However, this feature shows some sensitivity to the choice of autoregressive model in the MLR model probably because the decorrelation timescale for the regression residuals in the tropical lower stratosphere is longer than two months in SOCOL3 and some of the other CCMs (not shown). Further analysis of the Transformed Eulerian Mean residual vertical velocity does not reveal a substantial change in the rate of upwelling in the tropical lower stratosphere in any of the models (not shown)."

l.515 Not sure that you have justified the statement that proper SOR and SSI are needed for solar-climate impacts. Of course I agree with that (!) but you have not discussed climate (troposphere) much at all in this paper, and only used models with fixed SSTs.
This sentence does not claim that proper SOR and SSI are needed to simulate solar-climate impacts, rather it is simply a request to CMIP6 modellers to document the implementation of SSI and the SOR to enable interpretation of the model output after the experiments are finished. For example, a model that does not include any representation of the SOR might be expected to have a weaker atmospheric response to the solar cycle than a model that does include a SOR. CMIP models are often set up in different ways and traceability can be a challenge. This statement in the manuscript is

therefore only intended as an appeal for documentation on how CMIP6 models implement the SOR and SSI in order to interpret model differences once data become available. We have therefore left the text as before.

l.533 A reasonable summary paragraph but it is a bit disappointing that we seem to be no nearer any understanding of the solar signal and the conclusion is just that more data is needed.
The last part of the conclusions has been edited to focus on the new findings of the study. The last paragraph now reads: “Parts I and II of this study have shown that uncertainties remain in understanding the SOR, which present a challenge for including these effects in model simulations. However, given the inclusion of variations in the SOR over the annual cycle, as well as the greater consistency of the amplitude of the SOR with CCM results, CMIP6 models without chemistry are encouraged to use the recommended CMIP6 ozone database in order to potentially improve the atmospheric response to the solar signal. Nevertheless, whatever approach is employed, all CMIP6 modelling groups are encouraged to document the representation of the SOR and SSI in their simulations to facilitate future analysis of solar-climate impacts.”