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Atmospheric Chemistry and Physics Discussions

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

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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.

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

l.121 Why only one ensemble member? The modellers have done several to provide

you with stats!

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!).

l.184 Clarify "up∼0.3%": presumably not 0.3% of signal but 0.3% on top of c. 2% (?)

l.233 Have you looked at the impact of this choice of AR model (cf none or AR(1))?

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

l.349 et seq. Useful comment on effect of QBO simulations.

l.391-2 Indeed! Can you make any judgement on what is causing these differences between datasets?

l.425 Good point re seasonality.

l.491-494. Any conclusion on why these models produce a signal in the tropical lower stratosphere?

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.

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.

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