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Atmospheric Chemistry and Physics Discussions

ACPD

Interactive comment

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

Anonymous Referee #2

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Overview of study

The authors look at how the ozone (mainly stratospheric) changes in response to solar cycle activity (not including many secondly solar effects, such as high energy particles). The use CMIP5 and 6 data, and compare with observations, mainly characterised in part 1 of these papers.

I find the study comprehensive in its analysis, but not particularly novel in terms of the science, and certain not novel in terms of increased scientific understanding. The study essentially regresses out the solar signal from ozone in climate models, which has been done to death. I appreciate a lot of work has gone in to applying it to a new

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Discussion paper

data set, but I can see little advancement in scientific knowledge in what is done. The authors final summary seems testament to this, where their conclusion is essentially ‘we need more data’. The scientific analysis is far from rigorous as well, with statistical significance very rarely performed, in unclear for the figures where it has been done.

Concerns (major)

- The novelty to the study. Many studies have done very similar things to this study. Some of these studies are cited in the main text, but it is often not clear to a reader who is unfamiliar with the literature just how similar these studies really are. In many cases reproducing very similar figures. The authors need to be clear up front what is new here, and attribute all the repeated information to the correct papers.

- The statistical significance in this study. This is very poor, and often non-existent. The authors need to be clear about what their significance test is, what it is showing, and most importantly, they actually need to do some significance testing for most of the plots. In some plots, different significance tests will be needed for each panel. i.e. Figure 3, a different test will be needed for the individual model, as for the MMM. I am not even sure if the MMM has significance in the current study?

- Regression methodology. The methodology the authors use has no measure of uncertainty in the basis functions. This is a fundamental problem, because some of the basis functions have a good deal of uncertainty associated with them. The authors should add this uncertainty in to better reflect the uncertainty in the final result. The regression method they use is cited as from Maycock et al, but in reality, it probably has roots in far earlier solar-regression studies (see my first point, of giving due where it is deserved, even if methods/results are slightly different). I expect the authors arguments to not including uncertainty in the basis functions will be ‘it has been done multiple times before’, and cite a number of studies. However, this does not mean it is correct, unfortunately. A feel at some point, one of these studies need to include these basis uncertainties, at the very least to show that it doesn’t make a difference (although

I expect that it does).

- Anomalies. Often the authors use anomalies of variables, rather than the absolute variables. It would be good to see who real values of the data. I realise this can not always be done, but in some figures, for instance Figure 4, this would be very informative. Anomalies often make things look better!

- There is a lot of focus on the CMIP6 ozone data set. But seems to be absolutely no citation to documentation on this data set. I note that the creators of the data set are not authors on this paper, and perhaps some of that lack of knowledge is reflected in the text. Is there a CMIP6 ozone paper coming out? Should this current paper be kept out of publication till that exists? This should certainly be true if there is any overlap.

Concerns (minor)

1. Line 1: This alone would not fully capture the response

2. The SOR seems a little strange in this context, because it is not obvious (until later) that the SOR is not a ‘set thing’, it is only known within uncertainty bounds (and so different CCM give different SORs).

3. Line 9: . . . ozone databases’ – at this point it is not clear if the ozone data basis is the prescribed ozone, or simulated ozone from a CCMI.

4. Line 11 Make clear that you refer to historical period ozone

5. Line 13: weak compared with what?

6. Line 76 – a citation is really needed here (see major concern).

7. Line 89 – what time frequency is this data?

8. Line 94 ‘transferring’ – please revise this word.

9. L124. Why only 1 ensemble member? Please repeat with all of them. You need to capture the uncertainty.

10. Page 5 (top): This seems very similar to Hood et al, please state that.

11. Line 37: 5 x 5degree. This is not normal, why has the interpolation taken place?

12. Equ 1: Please cite where this came from originally.

13. Equ 1: Do the authors have any views on the breakdown between the long terms solar response, and the 11-year response?

14. Figure 1: Surely these QBO signals are just from one model? These will change.

15. Line 218-219: 'better proxy' not convincing to me. Please cite a paper that compares these.

16. Line 220-225: This section on the volcanic signal is vague. The data sets the authors use are not long, in fact some figure just use ~30 years of data. Very short for regression. Volcanic signals will cause issues in the regression, and it is not clear the authors have dealt with the properly (nor in Part 1).

17. Line 240. I think the authors need to show the autocorrelation plots to the reviewers, so we can assess this evidence. I agree they probably do not need to go in the main text.

18. Section 2.3: please describe more how this model fits into the wider models of CMIP5. Then we can assess suitability.

19. Line 255-260: This is worrying that the lower signal might not be so well captured.

20. Figure 2: Please just plot the SAGE2 and SBUV observations on this plot.

21. Line 291-295: power spectra would be useful here.

22. Line 350-358: This is an important point the authors make. Are you saying this is a drawback of the CMIP6 ozone data set? Please expand on your recommendations here.

23. Figure 3: What is the significance test here? Does the MMM have significance?

24. Figure 3: Why are tropospheric values masked out?

25. Figure 4. Colors very similar

26. Figure 5. Is there any significance on here? At this point (analysis of figure 5-9), I do not believe it constructive to have an in-depth review, because the significance is mainly missing, or hard to understand. You are interpreting potentially small signals compared to the noise.

27. Line 438-440: I think this is wrong, the SSTs do not constrain the upper tropospheric temperatures this much!

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