Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2018-129-RC1, 2018

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Simulating the atmospheric response to the 11-year solar cycle forcing with the UM-UKCA model: the role of detection method and natural variability" *by* Ewa M. Bednarz et al.

Anonymous Referee #1

Received and published: 16 March 2018

This is another in a series of model simulation studies of the atmospheric response to 11-year solar forcing. It considers only one model (the UM-UKCA model) but does an extremely thorough and careful statistical analysis of the model simulations (three 45-year simulations) for comparison to a $\sim$ 38-year meteorological reanalysis data set (ERA-Interim). I wish I could say that this study sheds new light on how solar variability influences climate. Unfortunately, as discussed further below, despite the careful analysis of the model and observational data (combined with an excellent review and referencing of previous work), limitations of the model itself, which is representative of

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many existing chemistry-climate models, preclude any such progress.

Revisions are requested in response to the following main comments and other more minor comments.

Main comments:

(1) The abstract and summary (section 7) make little mention of the strong disagreements between the model-simulated responses of stratospheric ozone, temperature, and zonal wind to 11-year solar forcing and that derived from observations (here, SAGE II and ERA-Interim). These disagreements (e.g., Figures 3, 4, and 5 for the annual mean response for the 3 simulations combined together) are so strong that there is no chance that the model can simulate realistically the solar-induced climate response. Instead of drawing this obvious conclusion, the abstract and summary (and most of the last half of the manuscript) concentrate on investigating the roles of detection method and internal model variability in affecting (in relatively minor ways) the calculated model response to solar forcing. While the latter aspects are of academic interest, they pale in comparison to the overall failure of the model to simulate the stratospheric response and, hence, any derivative consequences for the troposphere. The reader is left with the false impression that the model does a reasonably good job of simulating the solar response and that any differences with observations can be attributed to uncertainties in reanalysis data sets and possible aliasing of observations by volcanic and ENSO forcing.

(2) One fundamental problem with the model is its failure to adequately simulate the observationally estimated 11-year solar response of the upper stratosphere and lower mesosphere. Such a simulation is essential for initiating a strong zonal wind response near the time of winter solstice, which propagates downward and poleward later in the winter, ultimately leading to a tropospheric response (Kodera and Kuroda, 2002). The ozone response is too weak in the tropical upper stratosphere (dropping to 1 percent

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by 45 km; Figure 5a). Other models (see, e.g., Figure 1 of Hood et al., 2015) do better (about 2 percent at 45 km) but still fall short of that estimated from SAGE II data (about 3 percent). This shortfall of the simulated ozone response is at least partly responsible for the too-weak model temperature and zonal wind responses near the stratopause evident in Figures 3 and 4. At least some other climate models do a much better job of simulating the temperature response at these altitudes (see, e.g., Figures 4 and 5 of Mitchell et al., QJRMS, 2015) but I did not notice any mention of this in the text. Possible reasons for the poor ozone simulation are discussed only briefly in section 3.3.2 on p. 12. One factor that is not mentioned is the very coarse resolution of the 6 spectral bands evident in Table 1. The first spectral interval includes the entire UV region from 200 to 320 nm. As mentioned at the bottom of p. 9, the broad spectral band in the UV also has negative effects on the model's shortwave heating scheme and therefore its ability to simulate the full magnitude of the temperature response in the upper stratosphere and lower mesosphere. All of these problems combined together inevitably prevent the model from simulating realistically the downward propagating solar-induced dynamical signal. It is these deficiencies that should be emphasized in the abstract and summary sections for a fair assessment.

More minor comments:

(3) The model produces essentially no tropical lower stratospheric response of ozone and temperature, which contrasts with that derived from SAGE II and ERA-Interim data (Figures 3 and 5). This is probably mainly because of the weak modeled upper stratospheric response, which leads to, at most, a weak perturbation of the tropical upwelling rate via the Brewer-Dobson circulation. However, it may also be partly because the model does not have a coupled ocean; it only uses prescribed sea surface temperatures, etc. Recent work shows that there is strong coupling between the tropical lower stratosphere and troposphere, which will not be adequately simulated in a model

with no coupled ocean. For example, observational analyses have recently shown that the stratospheric QBO influences tropical convection and the Madden-Julian oscillation (Yoo and Son, GRL, 2016; and references therein). If the same is true for solar forcing, positive feedbacks from the tropical tropospheric response may have the effect of amplifying the tropical lower stratospheric response in ways that would not be simulated in a model with no coupled ocean and no MJO. Please at least mention with references in the revised manuscript introduction this new evidence for coupling between the tropical lower stratosphere and tropical tropospheric convection.

(4) With regard to the above noted evidence for an influence of the stratospheric QBO on the tropical troposphere, have the authors investigated whether such an influence can be simulated in the UM-UKCA model? Shouldn't such an investigation come first since many more QBO cycles are available for a robust statistical analysis? Presumably the model cannot simulate this since it has no coupled ocean and therefore, probably, no MJO. If not, then some discussion should be added to the manuscript about this deficiency of the model and whether it could affect the model's ability to simulate as well the 11-year solar forcing. Also, please mention in section 2.1.1 that the model is not able to simulate the MJO if this is the case.

(5) The font chosen for printing most of the manuscript text is difficult to read.

(6) P. 5, section 2.1.2. Is the resolution of the model's SSI forcing daily or monthly? This is not clear from Figure 1 after 1979 where large fluctuations are present near solar maximum. In other words, does it only simulate the 11-year component of SSI variability or does it also simulate the 27-day component? The latter could have some non-negligible effects on the simulated 11-year atmospheric response since extrema of the 27-day cycle can differ significantly from the mean. While it is true that TSI and UV proxies such as F10.7 correlate well on the 11-year time scale as shown in Figure 1 (see text on p. 8, line 18), this is not at all true on the 27-day time scale. Also, F10.7

does not correlate adequately with the actual solar flux near 200 nm on the 27-day time scale. So, if daily resolution is used in the future, you would need to use the actual SSI near 200 nm as your solar variable.

(7) The monthly analyses for the Nov. to April season in section 4 are useful for showing that the model only simulates a strong zonal wind response in November centred near 60N latitude whereas observations indicate that the zonal wind response is initiated at much lower latitudes in the subtropics and continues on with downward and poleward propagation through the winter. The same high-latitude bias of the 11-year zonal wind response is seen in most or all climate models. No clear explanation for this bias has been advanced but it could be related to the ozone response, which is much larger in the SAGE II observations in the tropical upper stratosphere than it is in most models.

(8) Most of the manuscript beginning with section 4.2 could be criticized as being either over-analysis of a deficient model or an investigation of issues that are mainly of academic interest. Can the authors find ways to delete or at least shorten some of this material?

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