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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 #2

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This is an interesting and very well written study on solar 11-year signatures in different atmospheric parameters (in the troposphere, stratosphere and lower thermosphere) based on model simulations with the UM-UKCA model. The paper does not present any spectacular new results on atmospheric effects of solar variability at the 11-year scale, but it is an interesting contribution to the field and should eventually be published in my opinion. An important aspect of the study is the fact that two different analysis techniques (i.e., a composite analysis and multi-linear regression) are applied and the differences in the results are studied and discussed. I ask the authors to consider the

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Discussion paper

following comments:

Page 2, line 15: “The spectral distribution of solar irradiance is commonly referred to as the spectral solar irradiance (SSI).”

I disagree, this is not the correct meaning of solar spectral irradiance. SSI has the units W / m2 / nm, i.e. spectral irradiance. It is the power of electromagnetic radiation per unit area and per spectral interval. SSI at a certain wavelength can also be determined or calculated without considering the spectral distribution of the entire spectrum.

Page 5, line 29: “In the Fast-JX photolysis scheme, the change in partitioning of solar irradiance . . .”

I find the phase “change in partitioning” a little misleading, because it’s not only the partitioning that’s changing. The overall TSI changes as well.

Page 5, last line: “At pressures less than 0.2 hPa, i.e. where photolysis rates are calculated using the look up tables, the 11-year solar cycle variability is reflected in the TSI change only, with no modulation of SSI.”

I don’t really understand this statement. If TSI is changed, then SSI (in a given spectral interval) must change as well. You probably mean that the spectral distribution of the solar irradiance spectrum is not changed, right? See also my comment on the meaning of SSI above.

Page 6, line 22: “.. but a sparse horizontal sampling (Soukharev and Hood, 2006; Hood et al., 2015; Tummon et al., 2015).”

I think it’s more appropriate here to cite one of the original instrument or algorithm papers, rather than papers that “only” use the data. Sparse geographical coverage was always known to be a disadvantage of solar occultation observations.

Page 7, equation 5 (and the equation in the supplement): The choice of the offset and trend terms does not make sense to me. The offset is just a number, right? Why does it have to be represented by a product of two numbers. This is not necessary and only makes things more complicated. I doubt that the function is implemented in this way in your fitting routine – this would not lead to stable results. Also, the trend term trend(t) is simply "t", right? If yes, then it should be written that way.

Page 7, line 16: "(here applied 5 times)"

Is there a specific reason, why this filter was applied 5 times?

Page 8, line 14: "However, unlike the yearly mean TSI timseries that forces the model, the timeseries chosen here is that originally recommended for the CMIP5 models"

How does this choice affect the results? Ideally, the same solar proxy time series should be used. Please add a brief (qualitative) comment on the expected impact (probably very small).

Same line: "timseries" -> "timeseries"

Section 3: It would be good to show a sample result of the MLR analysis (fit and residual). I have no reason to doubt that the method works well, but it's always good to see a fit example.

Page 10, line 17: "According to the postulated . . ."

I think this sentence is incomplete.

Page 11, section 3.2.3: This section focuses more on the (few) similarities between ERAI and the model simulations. However, looking at Figs. 3 and 4 the obvious aspects are the significant differences for both T and the zonal wind response. They should be mentioned/discussed as well.

Page 12, line 14: I suggest replacing "The lower altitude of the ozone response" by "The lower altitude of the maximum ozone response"

Page 17, line 10: "we find that the total column ozone responses derived in various regions are somewhat higher for MLR than for composites,"

Any ideas on the causes of this behaviour?

Page 17, line 28: "observational records such as ERAI"

Can one really call ERAI an observational record? It's certainly different from the "pure" observational records such as the SAGE II O3 data set.

Page 21, line 18: "Some differences (although not statistically significant) are found in the troposphere and in the tropical lower stratosphere."

Did the paper really show that the differences are not statistically significant? Some signatures are statistically significant in one analysis, but not in the other. What does this imply in terms of the statistical significance of the differences?

Figure 2, caption and title of panel a): "heating rates response" -> "heating rate response"

Page 43, Table 1, lines 3 and 4: Both lines list the same spectral interval (320 – 690 nm). Is this intended? If yes, the exact meaning of these two lines (and their difference) is not clear to me.

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