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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Separating the role of direct radiative heating and photolysis in modulating the atmospheric response to the 11-year solar cycle forcing" *by* Ewa M. Bednarz et al.

Anonymous Referee #2

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The manuscript attempts to disentangle the contributions from direct radiation and chemical effects of the solar irradiance. The authors applied the chemistry-climate model (CCM) UM-UKCA to simulate the steady-state atmospheric state for the solar maximum and minimum conditions. To separate the role of different processes and estimate the linearity of the overall response, the authors performed three pairs of experiments switching on only the direct influence of solar irradiance on radiation (RAD-ONLY), photolysis (PHOT-ONLY) and both (INTERO3). Several similar studies have been published before. The novelty of the manuscript consists of the application

of more sophisticated model and the results concerning the non-linearity of the two considered processes during the southern hemisphere cold season. This conclusion is important for the community because it emphasizes the necessity of the interactive ozone treatment in the climate models. However, this important conclusion is also a weakest part of the manuscript because the identification of the responsible mechanisms is not convincing. This issue should be clarified before the publication of the manuscript, otherwise this important conclusion will not be fully appreciated.

Major issues

1. The chain of physical/chemical processes leading to the weaker polar vortex during SON is not convincingly presented. From the presented results, it is more or less clear that the story should start before the winter time. The gradient in heating rate for the PHOT-ONLY case should related to ozone gradient. It cannot be dramatically different from RAD-ONLY case, because the ozone increase inside polar vortex due to enhanced solar UV should not be large. During SON the obtained gradient in ozone is high, but I think it is rather related to dynamical processes during late winter. This dynamically induced increase of the ozone in the stratosphere produce strong heating rate gradients during SON and produce further suppression of polar night jet. Thus, the triggering process is not identified leading to weak understanding of the obtained results. I do not know which process can be involved, but I think the authors should try hard to find it.

2. The linearity of the atmospheric response to radiation and chemical processes was discussed in several previous publications. Maybe it is better to concentrate on discovered no-linearity in the southern hemisphere and makes the description of the annual and tropical mean responses shorter.

Minor issues:

1. Figure 1: Statistical significance is missing on panel d).

2. Section 4.1: I recall the contribution of radiation and ozone effects were analyzed in Forster et al. (2011). Maybe it makes sense to mention this paper?

3. Page 10, Section 5: I think it is pointless to carefully compare the results of time-slice model runs with permanent solar max/min conditions with observations and try to explain the difference.

4. Page 15: The explanations in the last paragraph are too vague and not instructive to my taste. These processes definitely exist, but it is not easy to illustrate how they work.

5. Page 21, line 10: I wonder how it is possible keep this paper under review for already 3 years. It seems something is wrong with it. I would not site unpublished papers, because the results could be wrong.

6. Page 23, line 3: I do not see clear time line of the changes. It looks like triggering mechanism is missing.

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