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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

Interactive comment on "The effect of the solar rotational irradiance variation on the middle and upper atmosphere calculated by a three-dimensionalchemistry-climate model" *by* A. N. Gruzdev et al.

Anonymous Referee #4

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General comments

The manuscript describes the response of the gas composition and temperature in the atmosphere from the ground up to 100 km to the 27-day variability of the solar irradiance simulated with the chemistry-climate model HAMMONIA and its comparison with the results obtained from several satellite observations. This subject is highly relevant to the scope of ACP. In comparison with the majority of the relevant publications the authors present the results for a wide number of atmospheric species and also analyze the atmospheric response for extra-tropical area. The main thrust of the manuscript is the application of the high resolution spectral techniques to extract the solar signal from the model output and comparison of these results with more traditional approach based on the linear correlation in time domain. Overall, the authors confirmed previously published estimates concerning the simulated sensitivity of the ozone and temperature to the solar irradiance variability. The results obtained with the spectral analysis are rather new and have potential implications for the analysis of the short-term as well as long-term (decadal scale) response of the atmosphere to the solar irradiance variability. The applied scientific methods and assumptions are valid and clearly outlined. The obtained results are sufficient to support the conclusions, however the authors did not present absolutely convincing arguments to proof the deficiency of the traditional linear correlation analysis. The numerical experiments with the model are clearly described and can be readily repeated by other scientists. The previous publications about the subject of the manuscript are properly credited. The manuscript is mostly (except section 5.7) well written and structured. Therefore, I suggest publication of the manuscript with modest revisions, which are, described below.

Major issues

1. As far as I understood the authors applied artificial sinusoidal solar forcing with the same properties for all considered wavelengths. While it is acceptable for the sensitivity studies it is not in agreement with observational data which show much more complicated behavior (e.g., Krivova et al., Reconstruction of solar UV irradiance in cycle 23, Astron. Astropys., 452, 631–639, 2006). The application of the real solar irradiance variability instead of artificial can have some implications for the comparison between the spectral and linear regression approaches as well as for the comparison of the obtained results with observations. In fact it is not clear how to apply in this case the spectral technique proposed by the authors. Some comments on this issue should be added to the text.

2. The Figures 12 and 13 are too difficult to read because they are overloaded. I suggest to divide Figure 12 and 13 into 4 figures. In the first pair only the model results

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can be shown, while for the second pair I suggest to show some selected observations and the results obtained from the linear regression analysis, because this approach was used for the observation analysis. Accordingly, the section 5.7 which is not clear in the present version (see minor issues) should be improved.

3. The time-slice experiment with HAMMONIA model (Schmidt et al., 2206) showed that there is a clear tropical temperature response ˜1K to the decadal scale solar irradiance variability near the stratopause. The magnitude of the temperature response presented in the manuscript is much smaller and only marginally significant. This obvious contradiction can be also seen from the other model and observation data analysis. To understand the causes it would be useful to analyze the instantaneous solar heating rate perturbations. If there is a clear response to the 27-day solar irradiance variability then the problem is only with a proper detection of this signal. All applied methods (spectral and linear regression analysis) are based on the assumption that the spectral properties of the response are similar to the spectral properties of the forcing. This assumption works fine for the quantities with very short relaxation time (e.g., hydroxyl and ozone in the upper atmosphere), but it seems that this approach does not work for the temperature which leads to the underestimation of the signal. Then two possible alternatives can be considered: (i) the spectrum of the response is different from the forcing and (ii) there is a process which transforms the absorbed energy to the vertical motion instead of local heating. I understand that this problem is extremely difficult to address, but some comments or ideas how it can be done should be added to the paper.

Minor issues

1. Abstract, line 12: It should be briefly said what does non-linear behavior mean in this particular case.

2. Page 1114, lines 24-25: This sentence should be checked and clarified. I believe, during a solar maximum the variability of the solar irradiance is comparable to the

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annual mean difference between solar maximum and minimum.

3. Page 1114, line 26 and page 1115 lines 1-2: These statements should be supported by the appropriate references.

4. Page 1117, lines 13-16: This sentence should be moved to the conclusions.

5. Page 1122, line 1-4: The Lean’s data are not directly derived from SOL-STICE measurements. It would be better to reformulate this sentence.

6. Page 1124, lines 16-18: Does it mean that the molecular diffusion in the mesosphere plays important role for the decadal scale solar signal. Some clarification and estimations of the molecular diffusion intensity are necessary.

7. Page 1124, lines 26-29: This sentence is not supported by the results. For the ozone the difference between Fig.2a and 2b is obvious and proves in a way the presence of the 27-day response.

8. Page 1127, lines 25-27: The coherence for the temperature is hardly visible and the values of the coherence are difficult to read. Would it be possible to redraw Figure 6 to better illustrate the statement?

9. Page 1130, lines 15-19: It would be interesting to know the coherence for short lived species like hydroxyl.

10. Section 5.7: I suggest to define the method of sensitivity calculation from the spectral analysis at the beginning of the section (or even earlier in Section 3) . In the current version of the manuscript the analysis of the results appears before a very brief explanation how these results were obtained.

11. Section 5.7: it is not clear why the authors compared the seasonal sensitivities for the spectral method with non-seasonal sensitivities for linear regression method. Taking into account substantial difference between seasons, it does not help to judge how good is the agreement.

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12. Section 5.7: lines 4-18: This paragraph is not clear. For example, there is a big difference between spectral and linear regression estimates in the ozone sensitivities around 40 km and it cannot be explained by the two author’s arguments because the 27-day signal in ozone is clearly visible here (see Figure 2) and its magnitude is not small. How to explain this difference?

13. Section 5.7: I could not find in the text what happens if the spectral approach is applied to the analysis of no forcing case. This case is absent in the Figures 12, 13.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 1113, 2008.

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