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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Sensitivity of the tropical stratospheric ozone response to the solar rotational cycle in observations and chemistry-climate model simulations" *by* Rémi Thiéblemont et al.

Anonymous Referee #1

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The manuscript discusses the responses of the stratospheric ozone response to the solar irradiance variability on the Sun rotation cycle time scale. The authors analyzed the satellite observations by MLS instrument and results obtained with LMDZ model in free running and specified dynamics modes. The subject of the manuscript is appropriate for ACP. Despite many attempts to characterize ozone response to solar irradiance variability several aspects of the problem still remain open. The manuscript is well written and structured, the figures and explanations are clear. The conclusions about the dependence of the ozone response uncertainty on the signal strength and necessary numbers of cycles could be of interest for the scientists working in this area. There are,

however, some flaws which do not allow me to recommend immediate publication.

Major issues

1. It looks like the authors intentionally omitted the direct effects of the solar irradiance on the heating rates and temperature. After careful discussion of how important this process for the correct representation of the time lag between ozone and solar UV in the introduction they completely excluded this processes using not correct arguments. The importance of this process is clear even if we put aside the dynamical consequences of the direct radiative heating. I recall the importance of the direct heating was demonstrated in the recent paper by Sukhodolov et al., 2016.

2. It is also not clear why the authors used 3 year period to evaluate ozone response from the observation and model while they show that 3 year time period does not provide statistically robust results (uncertainty only below 50%). This should be somehow explained to the readers. The choice of the number of ensemble runs is also doubtful in the light of the obtained results. If the ensemble run is crucial it would be logical to estimate how many ensemble runs are necessary to reach some kind of convergence. Moreover, the obtained results with free running CCM will be more convincing if the analysis of the CCM runs without solar rotation variability is added.

Minor issues:

1. Page 7, line 1: It is clear since 2004 that two bands scheme cannot be used for the simulation of the atmospheric response to solar irradiance variability. It was confirmed in 2010,2011 and 2014. Several modifications of different complexity are known.

2. Page 7, line 17: I do not understand how climatological temperature can be used in CCM. Please, elaborate. By the way the reference to TUV is confusing. If I am not mistaken in the cited 1999 paper the tropospheric version was described. Maybe it is better to use recent intercomparison of the codes where TUV showed excellent performance relative to reference models. I do not also understand whether or not

daily NRL solar irradiance was used for the photolysis rate calculations.

3. Page 7, line 21-24: I recommend to check carefully this sentence. I do not recall such a bold statement in the cited paper.

4. Page 12, line 9: I think this statement is not completely correct. In general, the magnitude of the rotational cycle depends on non-homogeneity of the dark/bright features distribution, which can be very small for very high level of solar activity.Fig.8 shows for example rather small variances in 2002, while the solar activity level is high.

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