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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 #3

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The paper uses models and satellite observations to investigate the response of tropical stratospheric ozone to short-term solar UV variations due to the 27-day solar rotation. This is a topic that has been investigated in a number of previous studies but is still not resolved; therefore an updated study will be of interest to journal readers. The stated goals of the present paper are to "(i) assess the influence of the solar cycle phase on the ozone sensitivity to the rotational cycle and (ii) quantify the time window required for a robust estimation of the ozone sensitivity".

While the paper does address the stated topics, the focus is drawn elsewhere by awk-

ward organization and extraneous material. The authors do not make it clear why they include Section 3, which is longer than the section addressing their goals. Section 3 does not contribute to the focus of the paper and, in my view, does not contribute to the understanding of the solar response.

My most serious concern about the paper is in regard to the implicit assumptions in the text. At a number of places, the authors have assumed that a solar response is present even when they do not see a signal of it in their analysis or that the response is larger than what they find from the analysis. The text then describes why and how the signal has been masked. This is dangerous; if you do not find a signal of a response, the first explanation should be that there is no response. Even if processes that might mask a signal are present, it is not appropriate to conclude that this masking is the reason for not finding a signal that is "known" to be present based on prior assumptions. A more appropriate way to say it would be: if there is a response, it is too weak to detect.

Major Comments

1. As indicated above, I did not see the purpose of Section 3. Since you consider two fairly short 3-year periods, the analyses do not have any bearing on the questions raised about variations of the response with timing within the 11-year solar cycle or the dependence on the length of the analysis period.

2. Perhaps the response diagnosed from MLS is intended as validation for your model simulations. In this case, why ignore the 9.5 years of MLS/Aura observations that have been taken since 08/2007? As you later find using model simulations, a 3-year period does not give results that are very robust and so is not convincing as validation.

3. It seems that both satellite data (in Section 3) and model output (throughout) are zonally and latitudinally averaged over each day, including both day and night. There is a mention of local time issues (Section 2.2) but you then decide not to use any local time or day/night information in your analysis. There is evidence that this should be considered: 1) why else would the MLS/UARS ozone variations show a prominent

peak at the yaw period?; 2) it is known that local time variations in the response of ozone in the upper stratosphere to solar variability are not negligible (e.g. Li et al., Earth and Space Science, doi:10.1002/2016EA000199).

4. Although some spread should be expected, if I see a response peaking at 22 days (as you show in Figure 4a), I would automatically assume that it has no relation to the solar rotation. The signal in that particular panel is near zero at 27 days. It seems shaky to interpret it as driven by the solar flux variations. Can you provide more justification for your interpretation?

5. ("additional CCM simulation where the solar flux is kept constant") Since you show that the CCM responses vary considerably between realizations, a single simulation is not a very useful. Alternatives would be to perform additional realizations and/or to perform a similar case (fixed solar flux) with the CTM, particularly for a longer period (>10 years).

6. I am having trouble reconciling Figure 4c, f with Figure 6 and 7. Figure 4 indicates largest signal at $\sim$10 hPa and near zero signal at $\sim$0.3 hPa while Figures 6-7 indicates the opposite. Even though sensitivity (Fig 6-7) is different from absolute response (Fig 4), these do not appear to be consistent. Please explain.

7. Your conclusion (p. 15, l. 18) that "the differences mostly originate from the dynamical variability" is an important one that should be brought out more prominently.

Specific Comments

1. (p. 2) "the life span of a single satellite instrument is generally far less than one solar cycle" This has been true for a few but just as many last for a time span comparable to a solar cycle.

2. (p. 3, l. 24) Why "nonlinear"? Any dynamics will affect ozone.

3. (p. 7) The description of simulated solar flux variation was confusing. You imply that the variations are included in your photolysis lookup table but I could not tell exactly

how. Is the table recalculated every day? Is there a separate table for each value of your solar flux parameter? Please be explicit. Also, the impact of the solar variation on heating rate is not clear. Reading between the lines, I guess what you mean is that the heating will respond to the increased or decreased ozone but that, in the heating part of the calculation, the solar flux is kept constant. Is that what you mean? And one other comment: O2 should also be included on your list of radiatively active gases.

4. (Figure 3) There is a mismatch between the level shown in the figure ($\sim$3 hPa) and the level where you see a response in Figure 4 ($\sim$10 hPa). Perhaps this is related the mismatch between the SAGE/SBUV results, which contributed to the conclusions in the Hood paper you cite to choose the level of maximum response, and other data and models (e.g. see discussion by Dhomse et al., 2016). Also, a better label for the area where you see a signal would be middle stratosphere, not upper.

5. (p. 9, l. 9) "This explains why . . ." This could be the explanation but, as you show later, you are not using enough data to determine a robust signal. It would be safer to say that "This could contribute etc.". Since you cannot see a signal in the observational analysis, it is not appropriate to assume that the response is there but the signal is masked. There may not be a response.

6. (p. 10, l. 28) "The absence of correlation signal in the middle and lower stratosphere in the observations is consistent with the large noise present in the ozone dataset at these altitudes" As in the comment above, this is misleading since it implies that there is an ozone response but it is masked. You have not shown that a response exists.

7. (p. 13, l. 10) "overall anti-correlation" All I see is that there is a period when F205 variance is low and sensitivity variance is high. The curves are otherwise not related and, even in this period, do not follow a similar evolution. Coincidence of one perturbation is not enough to deduce anti-correlation.

Editorial comment

“increasing (decreasing)” and similar construction is grammatically incorrect and very confusing, especially since elsewhere you use parentheses in their legitimate use to define or clarify, e.g. “solar forcing index (F205)”.

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