Anonymous Referee #3

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 awkward 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.

We thank the reviewer for reading the manuscript and providing helpful comments and suggestions. We address the raised issues in turn below.

Our answers to comments and suggestions are written in blue and manuscript changes are written in italic type within double quotes ("*like this*").

Section 3 is a case study which follows up on Bossay et al. [2015] study using, in addition to observations, CTM and an ensemble of CCM simulations results. This part allows illustrating clearly, with a concrete case, the effect of internal variability when retrieving the response of ozone to solar forcing at 27-day timescale. Another interesting point of Section 3 is that it allows comparing model and observations and validating the model in light of the very good agreement when comparing the CTM and observational results in 2004-2007. We however agree that in the initial version of the manuscript, Section 3 was not sufficiently motivated. The manuscript has now been revised accordingly (See answer to major comment 1 below).

Regarding the second main concern of the reviewer, we agree that more caution is required when describing results where no signal is found (namely Figure 3 where a power spectrum analysis of raw ozone time series is performed). The manuscript has been revised accordingly.

The description of figure 3 and 4 has been changed in order not to presume of a solar rotational signal:

"The two periodograms of MLS ozone measurements (Fig. 3a and Fig. 3d) reveal no prominent peak in the range of the 20-30 days period, *suggesting an absence of a solar rotational signal in ozone*. More prominent peaks are found at longer periods although they are not consistent between the two periods. The large peak found at the 35day period for 1991-94 corresponds to the yaw-maneuver period of the MLS instrument as described previously (Froidevaux et al., 1994; Hood and Zhou, 1998). Similarly to observations, the periodograms of CTM results (Fig. 3b and Fig. 3e) do also not exhibit a distinctive solar rotational peak; there are some minor peaks between 20 and 30 days and their amplitudes are smaller in 2004-07 than in 1991-94. *The analysis has been repeated at lower pressure-height levels (e.g. 10 hPa, not shown) and led to the same conclusions. Overall, the raw power spectrum analysis of observations and CTM results in the middle and upper tropical* *stratosphere does not allow identifying an ozone signal associated with the solar forcing fluctuations at rotational timescales for the two periods considered here.*

[…]

We further examine the relationship between stratospheric ozone and solar rotational cycle by performing cross-spectrum analysis between stratospheric ozone and F205. *Despite the absence of a solar rotational peak in the ozone power spectrum derived from observations and CTM results*, cross-spectrum analysis should help identifying coherent variability modes between the solar forcing and tropical ozone. Figure 4 presents the vertical profile of the magnitude-squared coherence (hereinafter referred as coherence) between F205 and tropical stratospheric ozone from MLS observations (a and d), CTM model results (b and e) and CCM model results (c and f)."

We also made correction throughout the manuscript to avoid overstatements regarding the ozone response to F205 when this is not so clear.

It is however very clear that there is a response, as the cross-spectrum analysis reveals. In this regard, we now added a new Figure to the manuscript to show the difference in the ozone/F205 coherency between forced and unforced CCM experiments.

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.

Given that our analysis focuses on the 27-day cycle, one would expect 3 years to be sufficient to characterize the ozone response on rotational timescales because it represents about 40 cycles. Our results show it is not sufficient.

The first part of our paper, which considers the declining phases of cycles 22 and 23, actually serves as a case study where we compare observations and model results. It is a follow-up of the study of *Bossay et al*. [2015] which found some intriguing behavior in the response of the ozone to the 27-day solar cycle. Namely, that the correlation between the ozone and the forcing seems stronger when the amplitude of solar rotational fluctuations is small (more details are written in the introduction). We also found in the literature large differences between observational studies. To understand better this apparent contradiction and the variability of the ozone response to solar rotational fluctuations found in different observational studies, we need more than only one realization in order to improve the statistics and better understand the role of other sources of ozone variability. We thus used an ensemble of CCM simulations.

To make the purpose of section 3 clearer, we revised the introductory part as follows:

"As a first step, we follow up on the case study of Bossay et al. [2015] and make use of observations and modelling results comparison to provide a detailed picture of the ozone response to the solar rotational cycle during the declining phases of cycle 22 and cycle 23. We particularly aim to better understand the strong differences in the ozone response to solar rotational cycle found between the two periods. Two configurations of the LMDz-Reprobus chemistry climate model simulations are used, with specified dynamics (i.e. Chemistry Transport Model, or CTM) and in its free running mode (CCM). In the CTM configuration, temperature and wind fields calculated by the model are relaxed towards meteorological analysis; the dynamics is expected to be rather close to the reality, allowing direct comparisons with satellite observations for evaluating model chemical processes and its relevance to our study. In the CCM configuration, an ensemble of simulation is performed. Comparing the CCM ensemble results to CTM and observations during the declining phases of cycle

22 and cycle 23 allows to understand better the effect of internal dynamical variability on the ozone response. As a second step, we take advantage of the ensemble of CCM simulations and its large statistics 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."

We also revised the introductory of section 3. part as follows:

"In this section, we analyse the ozone response to the solar rotational cycle over the declining phase of solar cycles 22 and 23 in the observations and in the CTM and CCM model simulations. The analysis presented here follows up on Bossay et al. (2015) observational study. In particular, we aim to assess the model performances, understand better the differences in the results between the two solar declining phase periods and highlight the importance of internal dynamical variability."

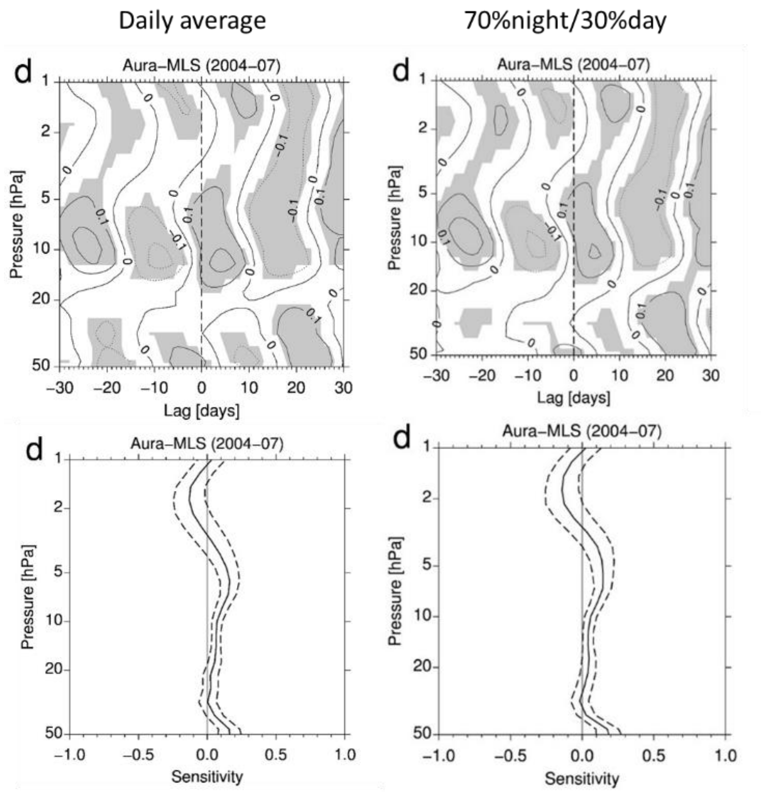
2. Perhaps the response diagnosed from MLS is intended as validation for your model simulations. In this case, why ignore the 9.5 years of MLS/Aura observations that have been taken since 08/2007? As you later find using model simulations, a 3-year period does not give results that are very robust and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

In light of the observation/CTM results comparison in 2004-2007, we argue that it is rather convincing as validation: model and observations show almost the same results on the several diagnostics we applied. We also performed extra analysis over the 2003-2005 period to compare with Sukhodolov et al. [2017] which also reveal very consistent results. This is now clarified in the Section 3 of the manuscript.

In our study, it is very important to use short periods as it is what was done in previous efforts. We can thus compare our results more easily and provide explanation on the different discrepancies that have been found in these previous studies.

Further note that the prolonged solar minimum after 2007 which lasted for almost 5 years has an exceptionally weak 27-day component. As a consequence, it is not the most appropriate period to examine ozone response to 27-day solar flux variations. There is again another maximum from 2011-onwards that could be investigated and we did it but it does not bring any further elements. In this regard, we do not intend to extend our CTM and CCM ensemble simulations by ~10 years that would turn out being very expensive.

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).



We tested the influence of an uneven distribution of night/day time measurements (see figure above). To do so, we reproduced the 2004-07 analysis Aura-MLS data but by resampling the data with the ratio of 70/30 of night/day time measurements and compared it to the daily average (which has a ratio of roughly 50/50). The results are fairly similar, indicating a limited diurnal effect on the unbalanced day/night sampling. Note that we repeated the analysis by considering only nighttime or daytime measurements (not shown) and did not find any significant difference.

The ozone diurnal cycle issue is now mentioned in section 2.2:

"Furthermore, the ozone diurnal cycle becomes important in the upper stratosphere, so that the results may be affected by the imbalance in daytime and night-time measurements used to construct daily time series. This issue will be discussed in section 3.2."

The following paragraph has been added in section 3.2:

"As mentioned in the section 2.2, the results based on UARS-MLS measurements may be affected by the imbalance between night and daytime sampling due to the ozone diurnal cycle becoming significant above 2 hPa. To test the influence of the ozone diurnal cycle, we repeated all the analysis performed in this section by mimicking an irregular sampling over the period covered by Aura-MLS (i.e. 2004-2007). Each day, ~800 ozone vertical profiles of the Aura-MLS instrument are evenly retrieved in the tropics [20S-20N] at two fixed local times: one at night (~0142 LST) and one during daytime (~1342 LST). We initially build the ozone timeseries using daytime measurements only (1095 days in total). Among these 1095 days, we selected N days randomly where daytime measurements were replaced by nighttime measurements. We then repeated the spectral, correlation and regression analysis. The procedure was performed for various values of N, from N=100 (i.e. 91% of daytime measurements) to N=1000 (i.e. 9% of daytime measurements). The results (not shown) revealed almost no dependence to N, suggesting that the diurnal cycle has a small effect on the ozone solar rotational signal."

Regarding the Li et al (2016) paper, the problematic is quite different. The orbit of SBUV drifts relatively to the diurnal cycle on the same timescale as the solar signal which is investigated (decadal). So, in this case, there is an artificial decadal fluctuation simply created by the drift which aliases the "real" decadal signal of solar origin. This type of problem does not apply in our case, though.

In UARS-MLS, the yaw maneuver creates an artificial periodicity of 36 days in zonally average data. Furthermore, the non-fixed local time measurements may introduce spurious variations in the temporal evolution of the daily zonal mean (due to the diurnal cycle) in the upper stratosphere. This may partly affect the observed signals in the upper stratosphere for the period 1991-94.

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?

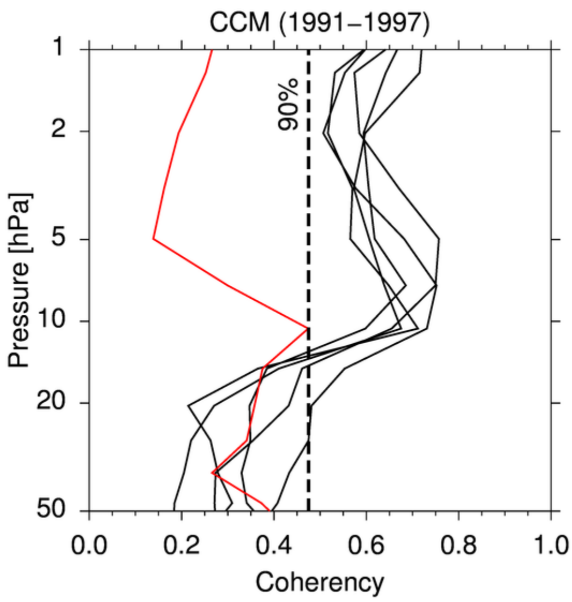
Active regions are not always located at the same longitude on the Sun. Furthermore, the Sun's rotational period depends on the latitude (i.e. differential rotation). For these reasons, we do not expect a thin peak at 27 days, but a rather broad peak near 27 days. Wavelet analysis of solar forcing time series (Figure 3c) shows that solar forcing fluctuations are strong in the band 20-30 days, hence it is not shocking to see a large patch of coherency between 20 and 27 days. The most convincing evidence of a link between the solar flux variations and the ozone response is the high coherency between both: it means that the UV forcing and ozone vary together.

See also answer to the next comment (#5) for further justification.

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).

The CCM realization with constant solar flux is performed in the CCM configuration from 1/1/1991 to 30/11/1997. To be fair, we compared the coherency in this experiment with the coherency in the five individual ensemble members over the same period. Below, we plotted the vertical profiles of the spectral coherency around 27 days between the F205 and ozone for the (dashed) constant solar experiment and (solid) the five individual ensemble members. It is clear that although the spectral coherency varies among the different ensemble members, the results are extremely similar above the 10 hPa level and very robust. If we remove the forcing, there is no coherency signal (dashed line). Given the very consistent results between the five different ensemble members, we are confident in saying that the coherency signal is due to the forcing and is not fortuitous. We now modified the manuscript as follows.

We added the following Figure:



"Figure 5. Vertical profile of the mean squared coherence between ozone and F205 averaged between 22 and 30 day periods and calculated for the time period 1991-1997. The black lines correspond to the results of individual ensemble members (five in total) and the red lines to the results of the experiment forced with constant solar forcing. The vertical dashed line indicates the 90% confidence limit."

We added the following part in the main text:

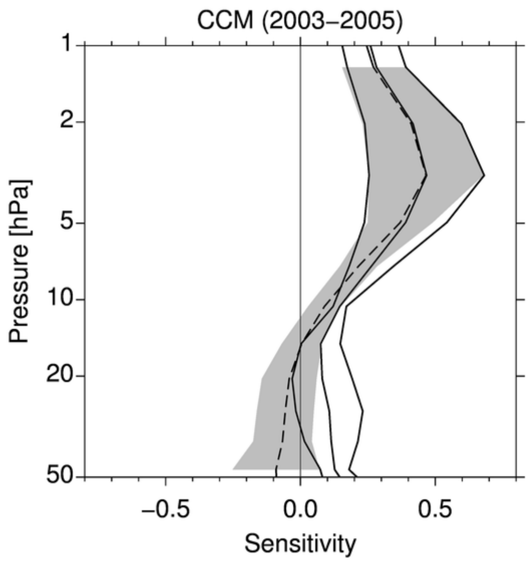
"To further test the robustness of the coherence signal, we perform an additional CCM simulation for the period 1991-1997 where the solar forcing is kept constant by using fixed (i.e. climatological) photolysis rates during the model simulation. Results are shown on Fig. 5. Below 15 hPa, the different experiments show no significant coherence between ozone and solar flux. Between 15 and 1 hPa, all forced experiments (black lines) reveal a similar and significant coherence signal while for the constant solar forcing experiment (red line), the coherence is weak and within the range of randomness. The absence of significant coherence found in the constant solar experiment confirms that the coherence found between F205 and stratospheric ozone is not fortuitous and primarily originates from photolysis processes"

6. I am having trouble reconciling Figure 4c, f with Figure 6 and 7. Figure 4 indicates largest signal at ~10 hPa and near zero signal at ~0.3 hPa while Figures 6-7 indicates the opposite. Even though sensitivity (Fig 6-7) is different from absolute response (Fig4), these do not appear to be consistent. Please explain.

Please note that:

1/ none of our analysis goes up to the lower mesosphere (i.e. 0.3 hPa). We will however assume that the reviewer meant 3 hPa.

2/ in the upper stratosphere, there is not near zero coherency signal at 3 hPa in Figure 4c and f (the signal is just weaker and only in Figure 4f).



On Figure above, we plotted the ensemble mean and spread of the ozone sensitivity to F205 index over the period 2003-2005 to compare our results with those of Sukhodolov et al. (2017) (see also answer to reviewer #2). The ozone is shown at optimum lag (where the correlation maximizes, solid) and zero lag (as in our analysis throughout the paper, dashed). This analysis shows that - particularly clearly when we examine the optimum lag sensitivity profile – the sensitivity does not maximize at 10 hPa, but it corresponds to the pressure level where the sensitivity average value is the most stable (as revealed by the strongly reduced standard deviation). Hence, coherency results and sensitivity are consistent.

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.

We insist on this conclusion at many places in the manuscript (abstract, results part, conclusion). The various corrections in the manuscript also allow insisting further on the importance of the internal variability.

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.

We changed the sentence to:

"*Furthermore, the life span of a single satellite instrument is generally shorter than (comparable to in some cases, e.g. MIPAS-ENVISAT, MLS-Aura) one solar cycle*"

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

The term nonlinear has been removed.

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.

We now provide more details on the fact that the photolysis separate look-up table is calculated every day from daily NRLSSI solar flux. We added the following sentence:

"*A separate photolysis look-up table is calculated every day using the daily NRLSSI as solar input*"

We also now provide more explanation on the effect of neglecting UV variations associated with the 27-day rotational cycle on heating rates. The following paragraph has been added:

"*Note however that the direct effect on heating rates generated by UV variations associated with the 27-day rotational cycle is neglected: i.e. daily changes in the spectral irradiance are not considered in the CCM radiative scheme. As a consequence, part of the thermal and dynamical responses to the 27-day rotational cycle and hence their effect on ozone (through transport and temperature dependent chemical reactions, as described above) are missing. The impact of this approximation on our results seems to be small though, as discussed thereafter (sections 3 and 5). Note also that on timescales of the 11yr cycle, Swartz et al. (2012) found that their photolysis-only simulation captured almost all of the solar cycle effect on ozone.*"

Finally, we added O_2 in the list of radiatively active gases.

4. (Figure 3) There is a mismatch between the level shown in the figure (~3 hPa) and the level where you see a response in Figure 4 (~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.

The pressure level at ~3 hPa level is the one we used throughout the manuscript as it corresponds to the level where the maximum sensitivity response is found (not only in our study but also in many other observational and model based study). Note that we repeated the power spectrum analysis at 10 hPa and similar conclusions were reached. This has been now clarified in the main text:

"*The analysis has been repeated at lower pressure-height levels (e.g. 10 hPa, not shown) and led to the same conclusions*."

Regarding the fact that the stronger coherency signal is found at 10 hPa while the maximum sensitivity is found at 3 hPa is explained in the detail in the answer to major comment 6.

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.

We now changed the text as follows:

"*This illustrates the difficulty in detecting solar rotational signals in the observations, as well as in a single ensemble member over these 3year periods*."

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.

There is a signal found in the CTM in the lower stratosphere (below 10 hPa). This signal is however not found in observations. We reformulated the text as follows:

"*The fact that the correlation signal in the middle and lower stratosphere (below 10 hPa) is found in the CTM but not in the observations may partly arise from the large noise present in the MLS-UARS ozone dataset at these altitudes (not shown)*".

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.

We reformulated as follows:

“This is further supported by the apparent inverse relationship which is found between the F205 index variance (Fig. 8b) and the ozone sensitivity variance (Fig. 8d)”

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)”.

We modified the following sentences:

“The phase lag vertical profile between the ozone response and the solar forcing was found to be negligible at about 40 km and gradually increasing/decreasing, below/above that altitude”

“After removing outlier values, 85% and 93% of the 1095day ozone time series of the periods 1991-94 and 2004-07, respectively, are kept for the analysis”

“Given that the amplitude of the rotational cycle increases with increasing solar activity, one may thus expect minimum and maximum sensitivity during 11year solar maximum and minimum phases, respectively.”