Atmos. Chem. Phys. Discuss., 11, C15891–C15896, 2012
www.atmos-chem-phys-discuss.net/11/C15891/2012/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Signature of the 27-day solar rotation cycle in mesospheric OH and H_2O observed by the Aura Microwave Limb Sounder" *by* A. V. Shapiro et al.

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Received and published: 7 March 2012

"Bandpassed time series are only selectively shown; only August 2004−December 2006 has been shown (in Fig. 2) to display the phase relationship. If they were to use the time series during 2008−2009 as a control case of null solar-cycle response, then beside the power spectra for 2008−2009 shown in Fig. 4, they may also want to show the bandpassed time series during 2008−2009 (either in Fig. 2 or in a subpanel). I'd also prefer showing all available data from 2004 to 2011, so that they can see how their correlation coefficient, the phase lag or other metrics goes with the declining phase of the solar cycle."

We would like to thank the referee for this comment as it forced us to consider the solar minimum period in more details and as we think improve the paper. We have added a new figure which shows the comparison of the solar irradiance and OH for the solar minimum period (2008-2009). The figure is attached. Working with this figure we found out that the period from November 2008 to November 2009 is not the best for the analysis of the null solar cycle response as it includes the end of 2009 when some solar activity is observed. Therefore we remade our analysis using the period from August 2008 to August 2009 as a minimum solar activity.

"Continuing from Point 1, the authors did not talk about how sensitive their results are to the bandpass window 20−35 days. The latter may be important to demonstrate the robustness of their signal."

We remade the figures 3 and 4 using unfiltered data that did not change substantially our results. We suppose this can be considered as an evidence that the filtering method used in the paper is sufficiently robust for our tasks.

"Were the power spectra shown in Fig. 4 derived from filtered time series? The authors did not state clearly. If the answer is NO, then the results are great. If the answer is YES, then the reader may not be able to tell whether they were filtering a pure (white/red) noise or a weak/strong signal in a noisy background. Filtering should be done only after they establish the statistical significance of the 27-day signal from raw time series (i.e. without any filtering). For example, the authors may want to perform a hypothesis test, where the raw power spectrum is tested against a red noise spectrum (Mann and Lee, 1996)."

We thank referee for this interesting comments. We remade the figures 3 and 4, using unfiltered data. The new figures still clearly show that the power spectra for the period of the high solar activity are substantially stronger than the spectra for the solar minimum.

"Continuing from Point 3, the authors used the Fast Fourier Transform (FFT) to estimate power spectra. However, it should be noted that the FFT power spec- trum has a very large uncertainty of ±100% (Press et al., 1992, Sect. 13.4). FFT is also subject to serious spectral leakage. Some improved spectral estima- tion methods offer a way to tell whether a spectral peak is a genuine signal or just a numerical artifact of noise. For example, Welch's method (Welch, 1967) is a simple extension of FFT where the raw time series is subdivided into smaller overlapping segments, each of which FFT is applied to obtain a “subspectrum”. If the 27-day signal is real and significant (whether it is correlated with the sun or not), then a majority of the subspectra should have a peak at 27-day."

The Welch method which was advised by the referee splits the data in the overlapping segments and calculates periodograms for each segment. We would be glad to use this method but the length of the time series used in our paper does not allow us to implement it.The number of points used for our paper time series is less then 400. As the number of segments recommended for the method is 8 after division the length of the segments will be about 90 points, which include only about 3 rotational cycles. Such small number of the covered cycles does not allow us surely extract the rotational cycle signature from the data. So this method does not fit our analysis but as we found it very interesting we will obviously use the Welch method in the future works.

"Again, did the authors perform the correlation analysis in Figs. 5 and 6 with filtered time series? Correlation analysis is known to be statistically biased if the input time series have been filtered (Coughlin and Tung, 2006). The authors did not explain how the statistical significance was estimated either. They may want to re-make Figs. 5 and 6 using regression coefficients instead and derived their significance as suggested by Coughlin and Tung."

Coughlin and Tung, 2006 use the regression analysis for the extracting solar signature from the data. The described method is very interesting however this technique cannot be applied in our case. The time series used in our paper are complicated by many different atmospheric effects. For some of them we even do not know regression pre-

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dictors. Besides the unfiltered data sets include seasonal component which is deleted from the solar data. If we have added the seasonal component to the solar data it would lead to the high correlation between the OH (H2O) and the solar irradiance. However this would be due to the correlation between their seasonal cycles.

"Their Fig. 4 shows “normalized” spectra, but the authors did not define what the “normalization” was. Conventionally, spectral power should have a unit “variance per frequency” [see, Eq. (6.26) of D. L. Hartmann’s lecture notes]. A correct normalization is important for a valid comparison between two power spectra. The authors may want to state clearly what their normalization was."

The spectra were normalized to the maximum of the strongest spectrum for the periods from 20 to 35 days. This description is added to the paper text.

"The zero lag (as emphasized in their abstract) appears to be conditional. As shown in their Fig. 2, top panel, the OH time series is first out of phase with Ly-α until January 2005 and in-phase afterwards until October 2005, back to out-of-phase and in-phase again in February 2006. Similar phase-lag variations can also be seen for H2O in the central panel. The authors did not explain this non-stationary phase difference. My personal experience is that a non-stationary phase difference between a filtered time series with a training index (i.e. the filtered Ly-α) may imply that the former may be a filtered time series of red/white noise. The authors may want to first show that the filtered time series is not from noise. Or did they already have a theory to explain this non-stationary phase difference? If YES, then they may want to say a few words to convince the reader."

As it is seen from Fig.2 the rotational solar cycle in August-December 2004 and in 2006 is not as strong as in 2005. It can lead to the weakening of the water vapor photolysis influence on the OH production and strengthening the role of the OH production by non solar mechanisms. OH produced by the latter way does not have to show any 27day solar cycle and could be qualified as a filtered noise. The decrease of the correlation is especially strong in the end of 2004. One of the possible explanation can be that the easterly SAO phase which characterized by the O_3 downward transport is observed in this period and it could lead to a strengthening of the OH production by $O_3 + H \rightarrow OH + O_2$. Partly this mechanism was described in the paper. We have broadened the explanation.

"The authors defined 2004–2005 as their “solar maximum” in the abstract. How- ever, the solar cycle may behave asymmetrically between an inclining and a de- clining phase. They may want to state more clearly that this was “a solar maxi- mum period during a declining phase.”"

We absolutely agree with the referee and applied this comment.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 28477, 2011.

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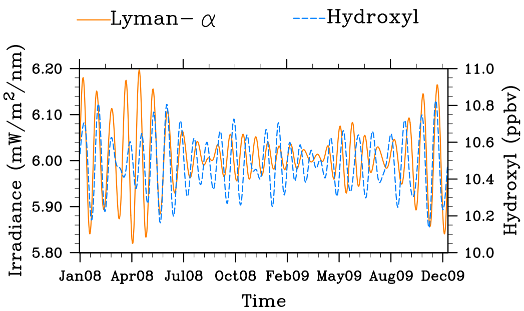

Fig. 1.

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