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Interactive Comment

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.

Anonymous Referee #3

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In this manuscript, Shapiro et al. reported a 27-day solar-cycle signal from the Microwave Limb Sounder (MLS) measurements of mesospheric hydroxyl radical (OH) near 80 km over tropics. Their conclusions were based on two arguments: (i) They found a peak in the "normalized" power spectrum of OH time series during 2004−2005 when Solar Cycle 23 was mid-way of the declining phase but not during 2008−2009 when Solar Cycle 23 reached the end of the solar minimum; (ii) OH anomaly is positively correlated and in phase with solar Lyman-α intensity (Ly-α). They did very good in supporting the above arguments by showing further that (iii) The "normalized" power spectrum of H_2O anomaly during 2004−2005 also shows a much stronger peak than that during 2008 2009 and (iv) H_2O anomaly is negatively correlated with Ly-α (proba-

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bly with 6−7 days phase lag) and hence the OH anomaly, as expected from standard photochemistry.

Their Argument (i) that the 27-day solar-cycle signal being present only during 2004−2005 may provide some confidence; their Argument (ii) that OH being anticorrelated with H_2O is also a nice one. However, in order to convince the reader that they did find the signal that satisfies Arguments (i) and (ii), more statistical tests will be helpful. Shapiro et al. may want to consider the suggestions below. They might have already done some of these statistical tests, but showing them explicitly in the manuscript will be extremely useful.

1. 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.

2. 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.

3. 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

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may want to perform a hypothesis test, where the raw power spectrum is tested against a red noise spectrum (Mann and Lee, 1996).

4. 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 spectrum has a very large uncertainty of $\pm$100% (Press et al., 1992, Sect. 13.4). FFT is also subject to serious spectral leakage. Some improved spectral estimation 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.

5. 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.

6. 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.

7. 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

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can also be seen for H_2O 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.

8. The authors defined 2004−2005 as their "solar maximum" in the abstract. However, the solar cycle may behave asymmetrically between an inclining and a declining phase. They may want to state more clearly that this was "a solar maximum period during a declining phase."

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