

Interactive comment on “Influence of solar activity on hydrological processes” by J. Pérez-Peraza et al.

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The following comment is made by Bernd Kromer (bernd.kromer@iup.uni-heidelberg.de) and Holger Braun (Holger.Braun@iup.uni-heidelberg.de).

The manuscript 'Influence of solar activity on hydrological processes' by Perez-Peraza et al. discusses a possible relation between solar activity and the water volumes of two lakes in Mexico and Europe, resp., by means of correlational and spectral analysis methods. Since the field of sun-climate connections tends to be highly polarizing we would like to comment on this paper in order to support a well-balanced formation of opinion concerning solar influence on the climate system of the Earth. Our comments relate to A) the structure of the paper and the data presentation B) the applied methods C) the discussion and interpretation of the presented results.

A) Structure of the paper / Data presentation

1. It is most desirable to present the full set of annual data as used in the paper (the sunspot surface, the level of Lake Tchudskoye, the level of Lake Patzcuaro), as graph and as table, or reference.
2. The last two decades of data (~1985 to today) are not presented in the paper. If these data are available, they should be included in the analysis.
3. The graphs suffer from several deficiencies. The vertical scale is often missing, so the dynamics of the spectra cannot be assessed. In fact, without appropriate labelling some figures seem to be almost incomprehensible.

B) Methods

1. The manuscript does not provide any information on the statistical significance of any spectral feature claimed to be seen in the data sets. However, it is widely recognized that in time series analysis of meteorologic data the key question is that of detectability of purported periodic signals. Without an estimation of the statistical significance of the results, the scientific contribution of the manuscript appears to be quite limited.

Over the past two decades a number of freely available packages have been developed which provide estimates of confidence limits in the detection of periodic signals (e.g. SSA [<http://www.atmos.ucla.edu/tcd/ssa/ssa-reference.html>] or Spectrum [<http://www.palmod.uni-bremen.de/~mschulz/#software>], see also the review of Ghil et al., 2002). Thus no major practical obstacle exists in accessing the relevant computer programs.

2. For the solar series the strong 11 yr spectral component comes at no surprise (figure 2), but the crucial question is if the weak (and possibly smoothed?) spectral features of the lake level curves are significant.
3. In figure 2 the lake level spectra are shown for various shifts between the sets of data series. How does a shift enter in the calculation of the power spectra, i.e. what is

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the meaning of the statement 'shifts between the sets of the data series' in the figure legend?

4. In the manuscript it is stated that figures 1b and 1c demonstrate a cycle of secular nature (p615, 1.). However, a detailed discussion of this claimed cycle is not included. Given that figure 1b is based on first differences, where the trend has been removed (and accordingly figure 2b shows low power at long intervals), and that the limited length of the time series (~100 years) would allow to track only one secular cycle, the significance of the claimed 80-90 yr cycle is highly questionable. Again only a confidence analysis would show if the claimed secular signal is statistically significant.

5. In the manuscript it is mentioned that 'the reliability of the estimations was controlled by applying the spectral analysis methods to the test data' (p. 614, bottom). From the manuscript it is not clear how this was done. In fact it seems to be questionable that the test data, e.g. the velocity of the wind in the Baltic Sea, can give any information about the reliability of the presented relation between solar variability and the levels of the two lakes.

6. It appears daring to compare lake levels of a site in Mexico to one in central Europe. The former would be considered sensitive to ENSO in the first place, whereas the central European site would probably be affected by the NAO rather than by ENSO. It would thus be of prime concern to eliminate false correlation by carefully removing dominant regional signals in both data sets. In fact, visual comparison of figure 8a and 8b appears to show mainly opposing (long-term) and non-related (short-term) features.

7. It is stated that the comparison of the spectra shown in figure 11 'with those of the lakes (figures 10a and 10b) reveals an absolute coincidence in the presence of the oscillations' (p. 620, bottom). Strictly speaking, figure 11 shows the solar spectrum, with a strong 11 yr signal and a shoulder at ~0.3% of the peak amplitude, assigned to a 4 year signal, whereas figure 10 shows spectra and cross spectra among the two lakes only. A 4 year signal in the Mexican lake could probably be attributed to

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ENSO, and it would not seem to be very surprising to find similar frequencies in a more NAO-dominated European lake. Thus the cross-correlation of the Mexican lake to the European lake could be spurious unless the significance can be shown. Support for such an interpretation also comes from the absence of common features in the cross amplitude spectrum other than the 4 years signal (figure 10c), possibly caused by the dominance of the ENSO signal.

C) Discussion / Interpretation

In the manuscript it is stated that the 11 year cycle is 'present in the water volume through analogous oscillations of the earth climate' (p. 616, bottom). Furthermore, the authors conclude that the water volume variations of Lake Tchudskoye show 'statistical significance with periods of 2.6, 11.2, 22 and 80-90 years' (p.621). It is precisely the significance of the cycles in the lake level oscillations which is not investigated in the paper.

Moreover, a possible relation between the 11 year solar cycle and climate is far from being accepted in the literature and still remains to be demonstrated beyond doubt up to today. The references as cited in the paper are chosen in a highly selective manner. A simple internet search shows a wealth of reviews and exchanges, some of them demonstrating that seemingly convincing correlations do not hold if longer intervals are considered, or are obtained only by selective treatment of data (see e.g. the 'classical' review of Pittock, 1978 and 1983, or the discussion of the findings of Friis-Christensen and Lassen, 1982 by Laut, 2003, among many, many more).

With these comments we do not want to dismiss studies in the literature where manifestations of the solar 11 year cycle in atmospheric parameters have been demonstrated, predominantly in the stratosphere. Yet the field of solar-terrestrial climate connections has evolved enormously over the past two decades, leading to high standards in the levels of data quality, statistical treatment of time series, and interpretation of the mechanisms behind the proposed links. We find that these criteria are not applied in the

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paper of Perez-Peraza et al.

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