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Nonlinear Processes in Geophysics
Discussions
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Interactive comment on "On the intrinsic time-scales of temporal variability in measurements of the surface solar radiation" *by* M. Bengulescu et al.

Anonymous Referee #2

Received and published: 13 July 2017

The authors perform a careful EMD and Hilbert analysis of four sets of surface solar irradiance data. The analysis methods are clear, concise, and carefully executed. The authors find a statistically significant IMF with a period of $\sim$1yr and higher-frequency components that they term 'weather noise'. These results are described clearly in the abstract and conclusions sections, however, I believe sections 5.1 and 5.2 of the discussion sections may, in some respects, be misleading. I believe the paper is of sufficient quality to warrant publication. The figures are informative and well described in the text. However, I have a few suggested modifications that I believe would improve the paper prior to publication.

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Primarily I believe it is important to establish the signal/noise status of the components before discussing their physical origin i.e. sections 5.3 and 5.4 should be placed before sections 5.1 and 5.2. These sections then question the validity of linking the various components to features observed in solar data e.g. the discussion of the high-frequency components with solar rotation, which appear to be due to noise and the dyadic properties of EMD.

Along the same lines in Section 5.3 it is stated that ‘unambiguous interpretations of QBO-like components seems to be out of reach’ and yet the authors still discuss the possibility that it could be related to the solar QBO.

If the authors insist on including this discussion I believe the terrestrial QBO should also be mentioned as this also has a well know impact on weather on Earth, such as the severity of winters, which would also affect cloud cover. However, it is my opinion that the authors should either not try and make any conclusions concerning the QBO or at least stress that with the current analysis they cannot be sure that this is a real signal.

Finally with regards to the QBO I believe that the link between galactic cosmic rays and cloud coverage is still highly debated and so I would either remove the comment concerning this or refer to papers concerning the debate.

Minor comments: P7, line 7 ‘Unlike in the Fourier decomposition, the amplitude is not a constant, but rather a time-dependent function.’ Fourier decomposition doesn’t imply the amplitude is constant it just doesn’t provide any information on the time variation of the amplitude.

P10, l24: ‘the only minor difference being the slightly extended range for TAT of 200 days as opposed to 300 days for the other two’: 300 for TAT and 200 for the other two?

P13, l27: containt -> contain

P15, l18 – Is there a way of quantifying how far from unity \omega must go before the

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null hypothesis can no longer be rejected?

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