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Atmospheric Measurement Techniques
Discussions

Interactive comment on "Moving Lomb-Scargle Periodogram: A way to identify time-varying periodicities in unequally spaced time series of OH* temperatures" *by* Christoph Kalicinsky et al.

Anonymous Referee #1

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Report on manuscript AMT-2019-332 Moving Lomb-Scargle Periodogram: A way to identify time-varying periodicities in unequally spaced time series of OH* temperatures, by Christoph Kalicinsky et al.

This paper presents the application of the LS periodogram analysis, ("Moving" LS periodogram) as a method to identify variations in unequally spaced OH temperature time series, and the calculation of the FAP as a method to test the significance of the peaks obtained in the LS periodogram analysis. However the use of "moving" periodogram is not new in studies of airglow variability in general, neither in OH airglow variability in particular. Then, although the authors describe the necessary mathematical method-

ology and later they apply it to a particular set of OH airglow data the authors forget to discuss many airglow studies that have previously used this mathematical tool for variability analysis. Then I can not recommend this work for publication until this is resolved.

General comments:

As the authors comment in the manuscript, airglow data have many gaps (day-night periods, moon-periods, weather...) and airglow variations are not constant, there are some features "more stable, repetitive and stronger" and other "more unstable and smaller...". There are different papers (quite a few) that have been devoted to study airglow variability (at different temporal scales, gravity waves, planetary waves, tides, seasonal variations...) by using time series analysis sliding a temporal window over a data set. This should be clarified and mentioned in the text.

Moreover, airglow studies dealing with periodogram calculations have also needed a method to distinguish the significant peaks, it is to say (real) peaks from the noise and in this sense, levels of confidence that a peak be a signal have been used in airglow studies that should also be mentioned (signals well above of the noise level, probability that a peak (z) above a level (z0) be false, false alarm probability function, FAP=1-(1-Prob(z>z0)ˆNi or confidence level...). However, as the authors point out, one of the difficulty to evaluate the FAP is to find the number of independent frequencies "Ni" of the data due to the non-orthogonality between different frequencies, by that "Ni" is usually calculated by fitting the FAP equation using different bootstrap simulations of the data set.

In the present manuscript the authors analysis the number of independent frequencies in different samples to find an analytic expression for this parameter (Ni). They find that Ni increases linearly with the length of interval (T), Ni=slope(f_rang) x T, but the slope for each frequency range analysed (f_rang) also follow a linear relation with the range of frequencies (slope=m(f)+b), obtaining an analytic expression for Ni, Ni= (m(f)+b) x

T. Finally in section 4.2, they use this relationship to find the number of independent frequencies of a set of OH measurements to easily calculate the FAP at one level.

I think this paper may be accepted for publications, but although the paper properly presents the necessary mathematical tools, and the enough set of simulations to find a mathematical expression to easy calculate the number of possible independent frequencies necessary to evaluate the FAP, the paper does not mention some of the works that have been carried out in the studies of airglow variability by using periodogram analysis + moving window + significance test (including FAP). In this sense:

1)The title of the paper should change because it seems that a "new" method "moving periodogram" is "first" applied in OH airglow studies, and that is not true.

2)The introduction should be improved to give appropriate credit to previous work in airglow variability studies using periodogram analysis.

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