Nonlin. Processes Geophys. Discuss., 1, C50–C54, 2014
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Nonlinear Processes in Geophysics
Discussions
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Interactive comment on "Can irregularities of solar proxies help understand quasi-biennial solar variations?" *by* A. Shapoval et al.

Anonymous Referee #1

Received and published: 14 April 2014

Specific comments

S1) I have doubts about the suitability of the sunspot number in its present form. It is an interesting contradiction that its importance is tremendous in long-term studies although its physical meaning is really ambiguous. The problem is that the numbers of sunspot groups and the individual sunspots are indicators of two different physical processes which are mixed in the present form of the time series. The number of groups is an indicator of the activity level i.e. the amount of the emerging field, whereas the number of spots within the groups depends on the fragmentation of the flux ropes which is a matter of interaction between the magnetic and velocity fields. This is one of the motivations to revise this time series. One possible solution is the Group Sunspot Number (GSN) compiled by Hoyt and Schatten. I do not suggest to repeat the procedure with

GSN in the present paper but it may be worth considering in a further analysis and the ambiguous background of the recent sunspot number might be mentioned here.

By the way, the official name of the sunspot number is recently International Sunspot Number (ISSN), I would recommend using it. The following address also recommends the correct reference to it:

http://sidc.oma.be/sunspot-data/SIDCpub.php

in the following way:

SIDC-team, World Data Center for the Sunspot Index, Royal Observatory of Belgium, Monthly Report on the International Sunspot Number, online catalogue of the sunspot index: http://www.sidc.be/sunspot-data/, 'year(s)-of-data'

S2) The pre-procession (Third section, Data analysis) has not been executed in P1. One should obviously get rid of the signal of the rotation which is an observational effect but the oldest active regions live as long as about three rotations. If after their decay another AR emerges at the same location its contribution is not an observational artifact but a component of the non-irregular behavior of the sunspot activity. See among others the paper of Kitchatinov and Olemskoy (2005, Ast.Lett. 31, 280) about the active longitudes, (by the way, their rotation rate is different from that of the Carrington frame). The question: is it possible that a N=162 smoothing leaves some signatures of the rotation in the time series? This could be checked with a power spectrum. I have the impression that smoothing with high N may be an exaggeration. A brief explanation is welcome.

S3) P1 reports a half-Schwabe variation (HSV) emerging in the irregularity index by computing with m=5. The high values at activity minima are conspicuous and also the difference between the years before and after the thirties, this is also reported in this paper. However, there may be an interesting relationship between the values of lambda and the activity levels at minima. In P1 Fig.3 the lambda is high at the very weak activity minima prior to the 1930s but it drops at minima after the 30s where the minima are not as weak as earlier. The best example is the last minimum prior to cycle 24, this was an extremely inactive minimum, sometimes several weeks passed without observable spots and the minimum-lambda was high again (is this a signature of a new regime?) . This is not so obvious in the present paper because of the averaging but the trend is similar. My question: is it possible that the behavior of lambda is just a consequence of a variation which is recognizable without any irregularity analyses? Furthermore, the maxima of lambda at activity maxima and minima may also arise because of the broadest latitudinal extension of the activity at these phases which might cause higher irregularity (?) . It is conspicuous that Fig.5 of the recent paper does not exhibit a variation of the lambda peaks at activity minima presumably because the model does not contain a modulation of the minimum levels. A comment on the mentioned (or any further) alternative explanations would be welcome. I dont claim that the explanation with QBO cannot be correct, I just conjecture that there may be a more simple interpretation which could be checked more easily and directly. Can the authors exclude it?

S4) I do not know whether the comparison with the aa-index can convince the readers. It has two distinct components, the coronal holes are the sources of recurrent disturbances for several rotations (Bartels) and if the 27-day signal is not filtered out from the time series then it may more strongly predominate the regularity than in the sunspot dataset because it is definitely an observational effect whereas sunspots may repeatedly emerge at the active longitudes which is a physical effect. The other component of the variations is the series of eruptive events, this is expectedly even more irregular than the simple sunspot emergence. Moreover, the solar impacts are also modulated by a further observational effect, the semiannual variation (Russell-McPherron, Rosenberg-Coleman) so the aa is more contaminated with known non-solar regularities than the sunspot index. The lambda of aa-index has definitely minima at activity minima (Fig.10), presumably because of the regular signal of the coronal holes in the poloidal phase. Is this possible? The pre-procession and the irregularity curves of the

sunspot index and aa index are different but the authors guess: "...but the same singularity in solar behavior could be at the origin of both." This does not seem to be a corroboration, just a conjecture, it deserves a more convincing argumentation.

S5) By reading the text I was wondering what is the answer to the question of the main title, in other terms, what is the heuristic potential of the irregularity analysis in this case. We use a time series, the sunspot index, which is a Sun-as-a-star parameter disregarding many relevant details, we carry out two sophisticated procedures (the analyses of irregularity and autoregression) and draw a conclusion about the role of the mid-term variations. The procedures are similar to a black box. For instance the reader do not see the role of the averaging although it is an emphasized conclusion that the R parameter increases with growing N in the time interval 1870-1910. What is the meaning of an N-dependent R? What is behind the m-dependence of the lambda-fluctuation? Some comments would elucidate how the procedure works.

Technical comments:

T1) Table 1 and Fig.3 do not seem to support the claim that R increases with increasing N between 1870-1910. For N: 162-324-648 parameter set R is: 0.67-0.83-0.79. I scrutinized Fig.3 and I have the impression that lambda_min at N=162 is 0.19 rather than 0.23. If so, R=0.75 at N=162, thus the series R is 0.75-0.83-0.79 for increasing N. This seems to be a mere fluctuation. It is also disturbing that the start of this earlier interval is indicated earlier than 1870 in Fig.3 and the plot is not in accordance with the data of Table 1. Please, check the data and the plot.

T2) There are apparent contradictions between the definitions of R, please check the use of greek letters: capital-Delta and small-delta and the possible reciprocal versions of R by comparing: i) page 167 last but one paragraph, ii) the caption and marks of Fig.3 (including lambda_mid) and iii) the caption of Table 1 (where the definition of delta_S_max is also suspicious).

T3) Please, consider a more straightforward description of the irregularity index. Section 2.2 contains too many indexes with their combinations, and variables. It can remain as it is but at the beginning a brief summary could enlighten the train of thought to facilitate the reading.

T4) A simple typological remark: I was embarrassed to see "theta" instead of the Euler's number (e) in the first equation in 2.1.1, and it turned out only in larger zoom that it is really "e", I would prefer a different font here and in section 4.1, third row for clarity.

Interactive comment on Nonlin. Processes Geophys. Discuss., 1, 155, 2014.