Nonlin. Processes Geophys. Discuss., 1, C239–C244, 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.

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Received and published: 8 June 2014

We are thankful to the reviewer for useful comments. A response to these comments, including the list of performed changes, is given below.

Please, see the copy of this response as a pdf-file with figures and appropriate formatting in the supplement. The revised version of the paper is uploaded to the journal web-page as a separate comment (AC C221).

1. We agree with the reviewer that the role of index aa in the paper is less than announced in the abstract. We have therefore eliminated references to aa from the abstract.

A discussion of the efficiency of prediction algorithms is outside the scope of this paper. We simply mention more clearly now the rather good prediction results claimed by several authors. Following the suggestion of the reviewer, we cite the review by Pesnell (2012) of on-going cycle 24 and two reports of the proximity of cycle-to-cycle characteristics to random walk (Love & Rigler, 2012 and Choudhuri & Karak, 2012) on page 4, lines 11-16.

2. We agree with the reviewer that the irregularity index defined in the paper and variation of the sunspot numbers (in the sense of variance) have something in common. Nevertheless, details are rather different.

2a. Appearance of HSV (5.5 y oscillations) is due to modulation of the Schwabe cycle (11 y). The changes in HSV properties need to be explained. In our earlier paper (Shapoval et al, 2013 = P1), we analyzed the evolution of HSV with time and found a regime change in 1915-1930. In the present paper, we primarily study the changes of HSV as a function of smoothing (denoted by N in the text).

2b. The irregularity index lambda reveals a transition between two patterns prior to a general increase of solar activity. Le Mouël et al (Le Mouël, J.-L., Shnirman, M.G., Blanter, E.M.: 2007, The 27-Day Signal in Sunspot Number Series and the Solar Dynamo. Solar Phys. 246, 295 – 307) found such a transition for a functional reflecting properties of the 27-day signal. At the moment, we are not able to simplify our irregularity index to somewhat following the reviewer's description of sigma/mu. The coefficient of variation (sigma/mu) as such is displayed in Figure 1 of this response. It exhibits HSV, but there are no traces of any regime change.

Figure 1 is placed here in the pdf-version of this comment.

Figure 1. The coefficient of variation sigma/mu (standard deviation divided by the mean) of 162 (red) and 648 (blue) day smoothed ISSN; it is computed within a 4-year window. The vertical lines indicate solar cycle minima.

3. As mentioned in 2a, appearance of HSV is not surprising. However, depending

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on the computation parameters, HSV could appear but also could not appear. In our model, we see that raw data (small c, Figure 7a and 7b) do not possess HSV after averaging over either 162 or 648 days. Smoothing the signal by simply increasing the parameter of the Poisson random variable (larger c,) we observe first separate rare (Figure 7c) and then global (Figure 7d) HSV. When a signal with separate rare HSV (Figure 7c) is changed by introduction of intermediate modulation, HSV is no longer symmetrical for N=162 and N=648: HSV becomes clearer with an increase of N (Figure 6b and 6c). We recognize that our modeling approach of ISSN is not unique (page 17, lines 14-17). In the framework of the model there are 5 parameters to be tuned. They correspond, in particular, to the activity level, the lifetime of the sunspots, and the period of the intermediate oscillations (page 13, lines 14-19). Tuning them one by one (sections 4.2.2 and 4.2.3), we are able to generate a transition from increasing to decreasing HSV as a function of smoothing only with the parameter that reflects the strength of 600-700 day variations. That is why we link different regimes of HSV to quasi-biennial variations. Authors who identify a signal in this period range are naturally led to suggest a link with quasi-biennal variations, which unfortunately does not mean that the sense of causality or the physical link have been identified and understood (yet).

4. We do not mention aa in the abstract any more. The irregularity index of aa is computed to show its increase in the 1930s, at the moment of the increase of the irregularity index of the averaged ISSN. Since the aa-series correspond to a physical measurement, whereas the meaning of the Wolf number series is ambiguous, the time coincidence of the lambda-changes supports the relevance of our tool and it is natural for us to suggest that the same singularity in solar behavior could be at the origin of both co-eval, correlated changes (page 17, line 30 - page 18, line 7).

5. In contrast to a standard statistical analysis, we could not introduce a reasonable null hypothesis in order to test significance. Instead we check the stability of the observed phenomena with respect to the parameters determining the irregularity index and test

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the significance of the conclusions with the auto-regressive model (this is added in the paper, page 17, lines 18-21).

Anonymous Referee #3 Received and published: 4 May 2014 1. Abstract is too long and could benefit from removal of many details. In reading the abstract, it is unclear if all the discussion is about sunspot number or aa or both. Yes, they say that both are considered in the first sentence, but the reader might be reminded of this important parallel a few sentences later, just to make sure that this is clearly understood. Also, the last sentence of the abstract is confusing. To say that the irregularity index of WN can be linked to the quasi-biennial oscillation (QBO) doesn't seem right. As the sentence is written, it sounds like the authors are saying that QBO might cause irregularity in sunspot number (WN), but I think they mean the opposite, that irregularity WN might cause the QBO. Introduction (page 159, line 12), I don't know that anybody can accurately predict that Schwabe solar cycle. Indeed, several recent papers have reviewed the many prediction methods (both physics based and phenomenological). In particular, some recent papers describe the cycle-to-cycle change in average sunspot number as indistinguishable from a random walk. The authors should review the literature on this subject, starting with a google of recent publications. 2. The authors report the identification of a half-Schwabe cycle, maybe with period 5.5 years, in which the sunspot number is most "irregular" at solar cycle minimum and maximum. Their analysis of Lyapanov exponents and imbedded dimension, etc. Is not something I'm familiar with, so let me ask a simple question. Could this "irregularity" in sunspot number be simply measured in terms of a normalized variance in the data? I say "normalized" because it would need to be normalized by average sunspot number in order to tease out effects (as reported in this manuscript) occurring at sunspot maximum and minimum. So, maybe sigma/mu would be the formula (calculated with independent non-overlapping intervals and without smoothing). This would, I think, be more intuitive, and it is certainly easy to calculate. If this shows something similar to a "half-Schwabe" cycle, then the follow-on question for these authors would be: Why use a complicated mathematical method to show something that sigma/mu also shows? 3. I wonder whether or not the reported half-Schwabe cycle is an artifact of relative uncertainty in sunspot number count when the absolute number is small. This uncertainty might result in large sigma/mu. At the same time, at sunspot maximum, large sigma/mu might result from higher levels of solar activity (giving greater relative variance). I note that some of the formulae in the manuscript involve logarithms. Would this represent an appropriate normalization for sigma/mu as might be described in terms of log-normal statistics? 4. The manuscript is really mostly focused on sunspot number; the geomagnetic index aa is kind of given short shrift. What is not clear, to me, is whether or not the half-Schwabe cycle is significantly present in the aa data. Looking at Figure 10 I see some anomalies in the (blue) irregularity index for aa, but these are not nearly as pronounced as they are in Figure 1 for sunspot number. Still the authors seem to report some consistency. I don't see it, however, and I would, therefore, like to encourage the authors to exercise a bit of skepticism about the consistency between the sunspot and aa results. If, after such an exercise, they still find consistency, then tell us more about it. 5. Normally, in a statistical analysis, one consider the "significance" of reported results by comparison against a null hypothesis. How would significance be assessed for an analysis using Lyapanov exponents and imbedded dimensions, etc.? Can the skepticism inherent in such an approach please be considered here? 6. And, finally, I think this manuscript would benefit from focusing on just one thing, the possible existence of a half-Schwabe cycle. I find the QBO discussion to be overly speculative and distracting from the main point of the manuscript. Are all the figures necessary? Can the presentation be presented in more succinct terms?

Please also note the supplement to this comment:
http://www.nonlin-processes-geophys-discuss.net/1/C239/2014/npgd-1-C239-2014-supplement.pdf

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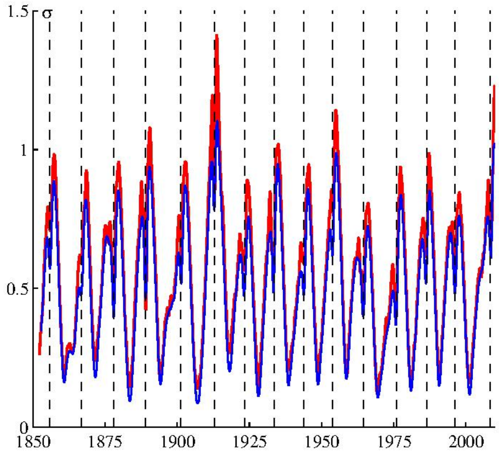

Fig. 1.

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