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

Response to Reviewer 1.

S1. As suggested by the reviewer, we have commented the choice of sunspot numbers as an indicator of solar activity:

Solar activity is estimated in the paper with the Wolf (ISSN, sunspot) numbers, involving the number of groups and the number of spots in each particular group. The number of groups reflects the emerging magnetic field and is an indicator of activity. The number of spots within a group depends on the magnetic field as such and also on the interaction between the magnetic and velocity field. In this paper, we had limited ourselves to sunspot numbers. Reviewer 1 suggests that we could study in the same way Group Sunspot Numbers (GSN, Hoyt & Schatten, 1998) in a further paper. Reviewer 2 (Svalgaard) is more pressing, and therefore we have already undertaken some analyses that we include in the revised paper and that provide comforting responses to both Reviewers 1 and 2 (page 9, lines 22-28). We agree that the study of GSN is worth discussing in this paper (hence a new paragraph, page 18 - lines 4-14 and a new Appendix B page 21). On the other hand, the paper mainly focuses on changes of the irregularity index with respect to smoothing, which leads to evidence of different "QBO" epochs. Therefore, from a pedagogical point of view, we place the graphs with the computation of λ of *ISSN* and *GSN* with different embedding dimensions in the new appendix B and comment them in the discussion. Despite differences in inhomogeneities and potential problems with the two series, the main results are quite similar, excluding the possibility of an artefact due to the choice of an imperfect time series. The irregularity index of GSN exhibits two different regimes with a clear transition in the period 1915-1940. This strengthens the result obtained for ISSN and published in Shapoval et al (2013) and further supports our approach.

The abbreviation WN is changed to ISSN.

S2. We agree with the reviewer that smoothing of the data over 27*k days, where k is large, leaves some traces of the periodicity related to solar rotation, since the duration of the rotation slightly deviates from 27 days. Following the suggestion of the reviewer, we have looked for possible remaining traces of the 27-day signal in the Fourier spectra of the preprocessed data.

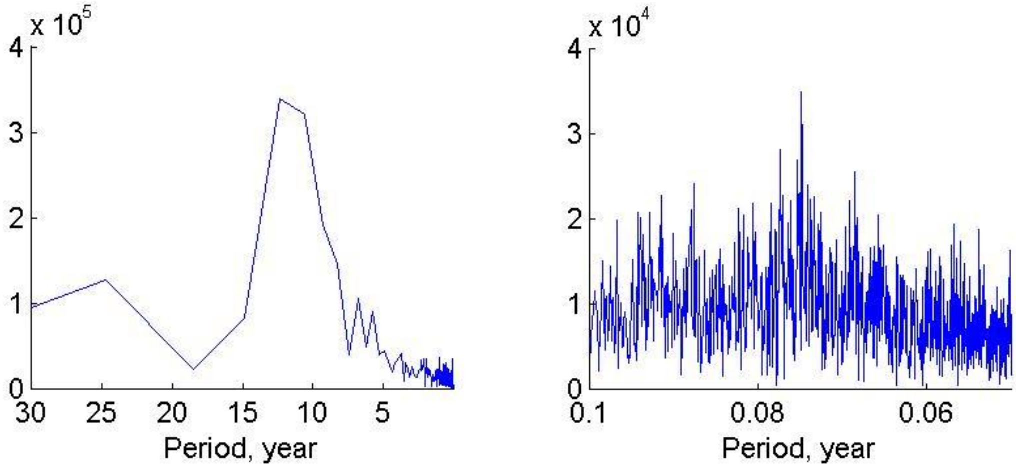

Figure 1. Spectrum (left) of the daily ISSN (1855-1930) and a zoom on the window (right) around solar rotation (27 day) periodicities. Periods rather than frequencies are given on the horizontal axis.

Figure 1 (left) gives a general view of the power spectrum with a prominent peak at approximately 11-year Schwabe cycles. The zoom around 27 days (0.074 yr) shows energy at the solar rotation period (right). Averaging over 162 days largely eliminates solar rotation periodicities, as shown by Figure 2 (left). There is more remaining energy when the averaging is over 648 days. This observation is natural, since averaging (convolution with a boxcar) pins the spectrum down and removes a frequency interval, whose length decreases with increased averaging (properties of the sinc function in Fourier space).

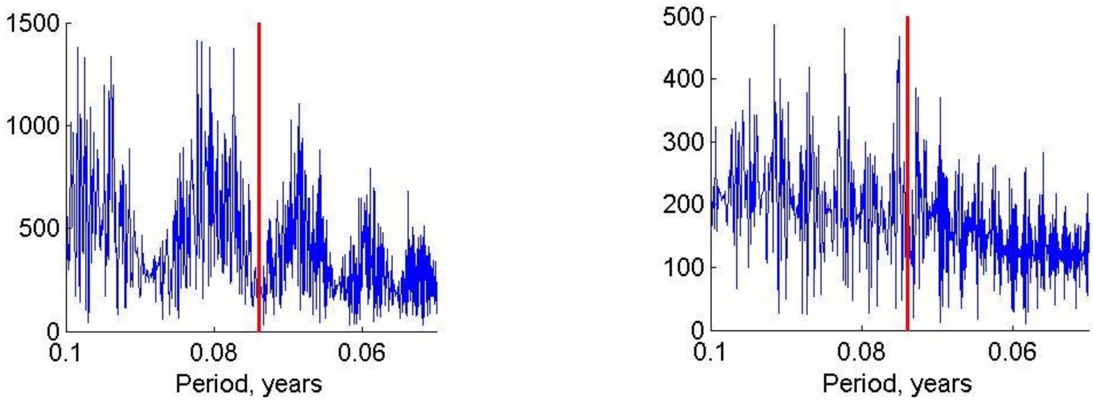

Figure 2. Part of the spectrum of the daily ISSN (1855-1930) averaged first over 162 (left) and 648 (right) days. The red lines indicate 27 day (0.074 yr) period.

In the paper we do not aim at complete elimination of signatures of solar rotation from the data but at their reduction. Their existence does not invalidate our technique. This point is clarified now in the text (page 10, lines 4-8). We have added the reference to Kitchatinov and Olemskoy (2005) and to other papers at the beginning of section 3 (in red).

S3. There are several questions in S3. We answer them separately.

(i) HSV as such are not of a great importance. Our functional λ can achieve its extrema on ascending and descending phases. If it is the case, HSV appear because of a certain similarity between ascending and descending phase. That is why we do not discuss the physics underlying the essence of HSV. On the other hand, we look for a simple time series with properties observed for *ISSN* (section 4). This new paragraph is in the paper on Page 16, lines 18-22.

(ii) When the signal is close to zero (this is the case of Figure 5, but the graph of the model signal is not presented) and the embedding dimension is 1, many points lying at distance 1 are transformed by the translation mapping to points lying at distance 11. As a result, the logarithm of the ratio, log10(11) appears as the value of the irregularity index. This result comes from the very simplicity of the model. Nevertheless, to get reasonable conclusions on sunspot numbers, which are more sophisticated than the simple model signal, we turn from embedding dimension m=1 to m=2.

(iii) As we understand the main questions of the reviewer (in S3) are "why is our λ large at cycle minima, and can we repeat our observation with a simpler tool? Some suggestions of the answers are in the review. Really, an explicit relation between lambda and level of solar activity is not evident. At least, it is *not* a decreasing function of activity. We agree with the reviewer that *ISSN* and its variation are connected to each other. However the coefficient of variation and its simple modifications do not demonstrate the properties of the irregularity index (see also answer to reviewer 3). Introducing the irregularity index, we try to define such a variation, which seems to be what the reviewer asked for. We believe that, because the irregularity index varies (i) in time and (ii) with smoothing, we see new properties of solar activity. Understanding the physics that underlie the changes of patterns of the irregularity index with time will require further research. At the moment, we can neither accept nor reject that "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".

As to QBO, in this paper we emphasize primary changes of the irregularity index lambda as a function of smoothing. 5.5-year oscillations as such are not surprising because they are generated by 11-year modulation (a simple simulation with a sine-curve supports this statement). It is the dependence on smoothing that needs to be explained. Of course, we recognize the possible non-uniqueness of the solution (page 17, lines 14-17); however, we construct a simple model that incorporates the basic features of ISSN. In the framework of this model, there are 5 parameters to be tuned (page 13, lines 14-20). They correspond, in particular, to the activity level, the lifetime of sunspots and the period of the intermediate oscillations. 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 the 600-700 day variations. That is why, as done by other authors when this period range emerges, we link different regimes of HSV to quasi-biennial variations.

As the reviewer mentioned, a jump of the irregularity index to a new level prior to a general change of solar activity can be considered as a precursor. This consideration underlies another paper "Shapoval, A., Le Mouël, J.-L., Courtillot, V., Shnirman, M.: Is a sudden increase of irregularity of sunspot numbers a precursor of a return to low solar activity?" submitted to the Journal of Geophysical Research.

S4. We agree with the reviewer. But Figures 3 and 10 indicate that in the 1930s, lambda for ISSN and *aa* went in opposite directions (a more or less step-like change) to new levels of values (of oscillations), even in spite of the absence of preliminary smoothing. We find this observation of simultaneous opposite changes of ISSN and *aa* important and therefore worth noting in the paper (page 17, line 30 - page 18, line 7; Figure 10; page 19, lines 23-29). However (also as an answer to the other reviewers), we have eliminated most references to *aa* from the abstract, in order to remove emphasis on that topic at present. Since there is a regime change of *aa* in the 1930-s (exactly at the time when the irregularity index of ISSN coming into a new regime), we find it reasonable to suggest that our irregularity index does measure some physical phenomena (and not artifacts reflecting inhomogeneity of ISSN; see also response to reviewer 2). But at this stage we do not have a more convincing physical argumentation.

S5. The reviewer would like more comments to elucidate how the procedure works. We have attempted to improve this, though we do not have all the answers yet to the remarkable numerical results we have obtained. Let us specify a few things further. The irregularity index is likely to coincide with the Lyapunov exponent for chaotic systems. We checked this conclusion for logistic maps that possess a positive Lyapunov exponent. The irregularity index is also meaningful for the description of stochastic processes. The key point is that white noise is characterized by a positive irregularity index, which decreases to zero when the embedding dimension *m* increases. But the theoretical zero-value of the irregularity index as well as that of the Lyapunov exponent cannot be approached in a real computation, since we cannot have infinitely (space-)close points. Therefore, the dependence of the irregularity index on *m* allows one to estimate whether a stochastic component is present in the signal.

Initial smoothing of the signal simplifies the irregularity index and amplifies the importance of the main extrema: by this we mean that originally noisy lambda curves evolve to a simplified quasi-periodical structure with much less noise. They look like quasi-sinusoidal curves, with smooth maxima at solar cycle extrema. This is what we call HSV, since the period (or rather pseudo-period) is about half of the length of a Schwabe cycle. Responses of the *main* extrema to changes of *N* are worth studying by themselves. This is done with parameter *R* that measures the respective mean amplitudes of lambda maxima at solar cycle minima (that we did not expect to occur originally) with respect to those at solar maxima (the ones that might have been expected a priori). In other words, *R* is a measure of HSV (page 11, lines 9-11). A decrease in *R* corresponds to an increase in HSV. The meaning of the *R*(*N*) function is studied with our simple model in section 4. We find that the secondary maxima of the irregularity index nearly disappear with larger smoothing of the signal for the model without intermediate oscillations (Figures 8 and 9). However, if the intermediate oscillations are strong, then secondary maxima (observed at the signal minima) are amplified with a growth of *N* (Figures 6b and 6c).

We are also thankful to the reviewer for the technical comments.

T1. We changed 1867 to 1870 in the description of table 1. It was a misprint. The minimum in late 1866 is not used in the computation of *R*. It is for the correct time values that our *R* values have been calculated and they still stand.

The computation of *R* is straightforward when the signal behaves as a sine-like curve and the minima and maxima are easily detectable (see the curve in Figure 3c as an example). We think that during cycles 11-14 HSV is observed clearly only from the maximum of cycle 11 to the maximum of cycle 12. The computation of *R*, which is equal to 0.67, supports this statement. On the other hand, HSV is easily seen within cycles 11-14 with smoothing *N* equal to 324 and 648. This results in rather similar and relatively large values of *R*: 0.83 and 0.79.

We do not discuss in the paper the left boundary of the regime characterized by amplification of HSV with smoothing, which is difficult to detect. We concentrate on the right boundary, at which lambda changes to an epoch with messy HSV for any value of smoothing.

T2. We unified the notation. The changes are in red.

T3. We wrote an introductory sentence, split the formal description into small paragraphs, each of which determines one quantity, and named these paragraphs. We also added : "With respect to the Lyapunov exponent, in order to determine the irregularity index, we relax the requirement that close points in the phase space must be remote along the time axis (page 7, lines 6-7)."

T4. We re-typed the symbol.

Response to Leif Svalgaard (Reviewer 2)

We are thankful to Professor Leif Svalgaard for his comments and criticism.

We re-discuss inhomogeneity of Wolf numbers on page 17, lines 22-29.

The homogeneity of the *ISSN*-series is a long debated question. Svalgaard (2010, 2012) points to an abrupt increase of *ISSN* in ~1945 and argues that this increase is caused by changes in the measurement rules. The NASA web-site (http://solarscience.msfc.nasa.gov/greenwch.shtml) also notes that the sunspot series is not uniform; abrupt changes occurred in 1941-1942 (sunspot numbers) and 1976-1977 (sunspot areas, not used in our paper). However, our conclusions about regime changes are not seriously affected by such events, because we use ratios (equation 4). Moreover, the date of the ~1930 singularity is remote from 1941-1942 (or 1945); page 18. lines 22-29.

Following the reviewer's suggestion, we have applied our algorithm to the group sunspot numbers (GSN) introduced by Hoyt and Schatten and find that the behavior of the computed lambda corresponds to the regime changes found in our previous paper (Shapoval et al, ApJ, 2013 = P1). Computing the irregularity index of GSN with delay 8 and different embedding dimensions (Figure 1 of this response), there is a clear change in the regime of solar activity, approximately between 1915 and 1940. According to the graphs corresponding to large values of *m* (greater than or equal to 8), one regime of lambda continued during cycles 12, 13, 14, up to the very strong peak of lambda that occurred at the minimum between cycles 14 and 15. Graphs with a smaller *m* (4-8) indicate a possible continuation of the first regime up to the minimum between cycles 16 and 17. The regime change evidenced by Figure 1 exactly corresponds to the regime change exhibited by the irregularity index of the sunspot numbers (ISSN) and reported in P1 (compare Figure 1 and Figure 2). This provides additional evidence that our methodology reveals some "hidden" properties of solar activity and answers the reviewer's concerns. We are grateful for this suggestion and have introduced a new paragraph and figure in the paper to show these strengthening results (strengthening both P1 and the present paper). The new analyses involving GSN are added and discussed in Page 9, lines 22-28; Page 18, lines 8-18; Page 21, lines 17-27 (Appendix B), Figures 12 and 13.

The discontinuity of GSN in ~1880 highlighted by Leif Svalgaard is not based on just a multiplicative factor. Indeed, our irregularity index, which reflects the *ratio* of distances (page 17, lines 26-28) and is conserved under multiplication, suggests a regime change of GSN in the 1870s, not seen in the irregularity index of *ISSN* (Figure 1 vs. Figure 2).

Half-Schwabe variations (HSV) debated in our paper are seen for GSN as such with large embedding dimension (*m* larger than or equal to 16, Figure 1) and partly for GSN first pre-smoothed with 162 and 648 day averaging (Figure 3). However, the change of HSV with smoothing, the main observation of the paper, is absent in the case of GSN. We link this absence to a certain smoothness of GSN.

We retain the term "quasi-biennial" oscillation, since other authors mention QBO when they a 600-700 day variation is involved.

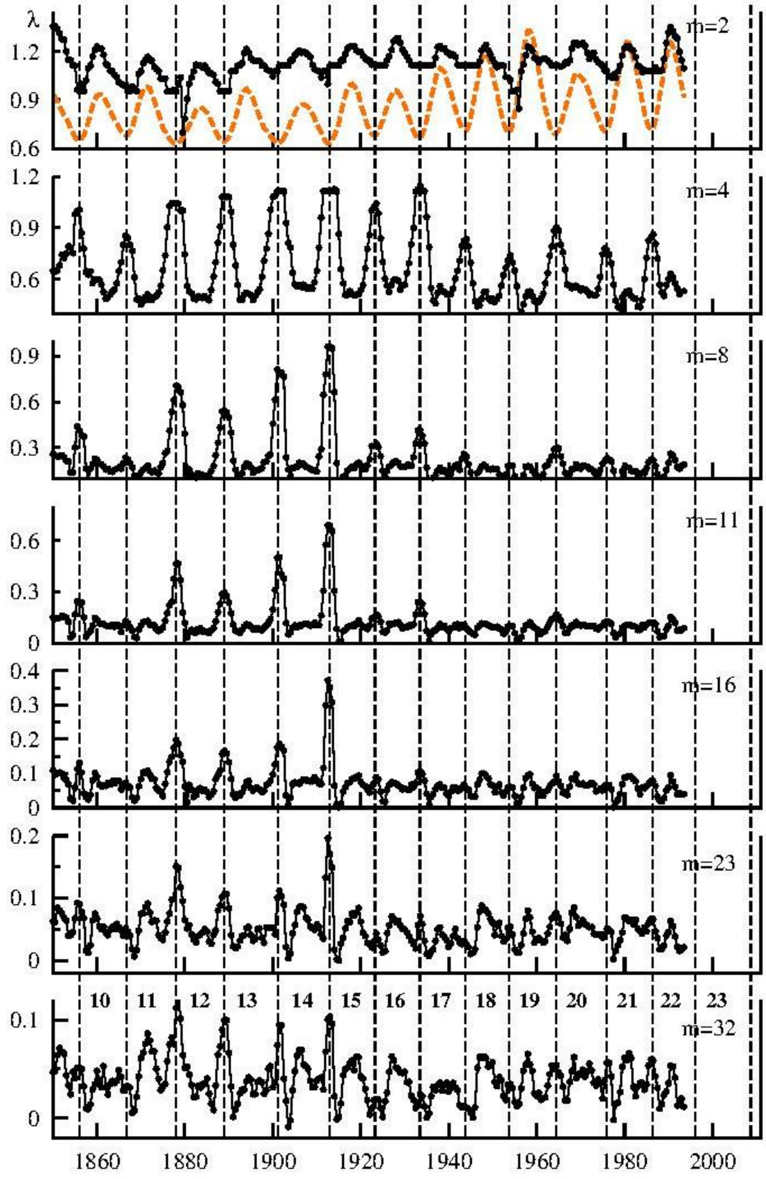

Figure 1. The irregularity index λ computed for GSN within 4-year sliding windows; the embedding dimension *m* is indicated; vertical lines are at solar cycle minima.

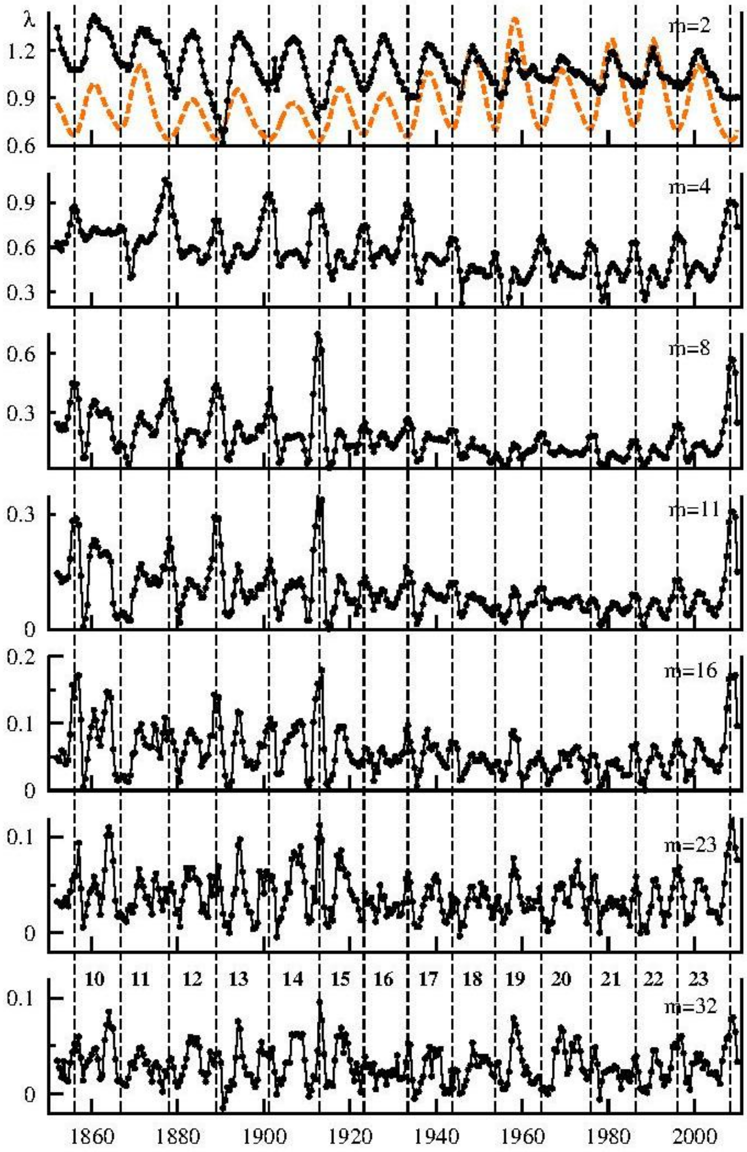

Figure 2. The irregularity index λ computed for *ISSN* within 4-year sliding windows; the embedding dimension *m* is indicated; vertical lines are at solar cycle minima.

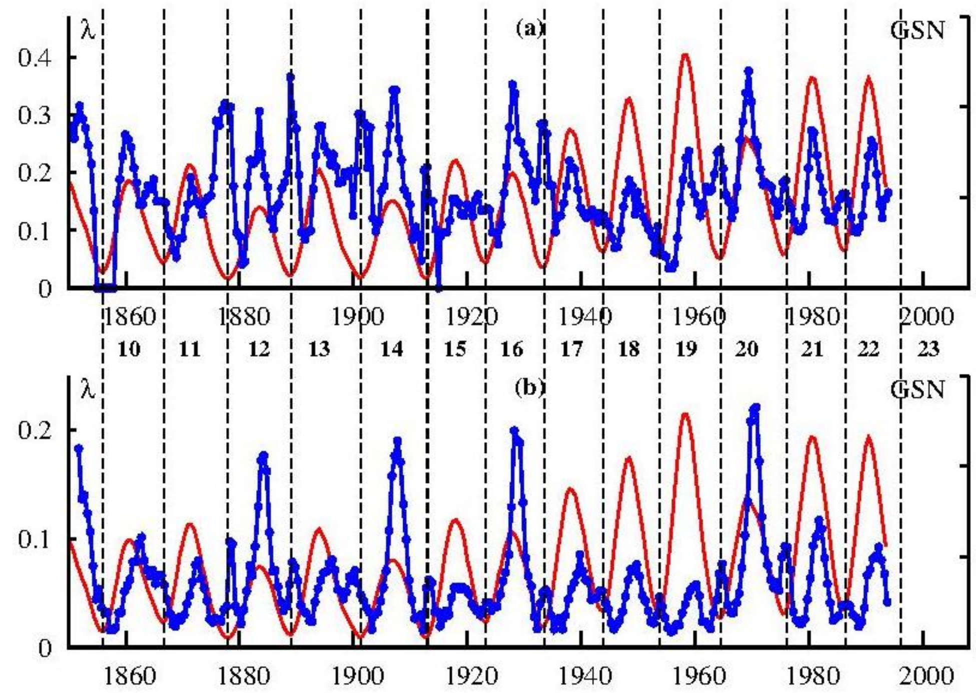

Figure 3. Blue curves: the irregularity index λ computed within 4-year sliding window for GSN averaged over 162 (a) and 648 (b) days; red curves: GSN averaged over 1461 days (4 years); m = 2. Vertical lines are at the minima of 4-y smoothed GSN.

Response to Reviewer 3

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

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.

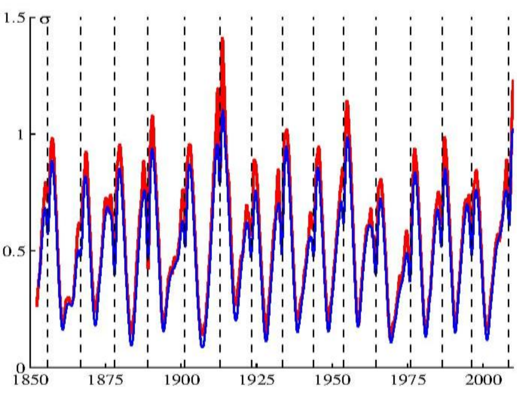

Figure 1. The coefficient of variation σ (standard deviation divided by the mean) of 162 (red) and 648 (blue) day smoothed *ISSN*; it is computed within a 4-year window. The vertical lines indicate solar cycle minima.

3. As mentioned in 2a, appearance of HSV is not surprising. However, depending 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 the significance of the conclusions with the auto-regressive model (this is added in the paper, page 17, lines 18-21).

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.

L. Svalgaard (Referee #2)

leif@leif.org

Received and published: 26 April 2014

Technically, the paper is as it is: application of a sophisticated technique to a nonhomogeneous data set [the sunspot number].

I do have several concerns:

1) the sunspot series is very in-homogeneous, pieced together from data by different observers using different counting methods. This means that it is not easy to separate changes due to observers and due to the sun.

2) as the sunspot data is heavily smoothed it is not really necessary to require that daily values be present. Using monthly values should work just as well and they go back to 1749 [another century]. Especially for an alleged finding regarding long-term regimes, it is important to go back as far as possible.

3) the group sunspot number [GSN, Hoyt and Schatten] also goes back far and has different biases on problems than the International [Wolf] Number and it seems to me mandatory that the GSN also be analyzed with the technique advocated by the authors.

4) there is good evidence that the number of spots per group is not constant in time but has both a solar-cycle variation and [more importantly] an observer-dependent variation, in addition to a possible secular solar variation. So investigating the GSN is important and should be done. Because of these concerns I do not consider the speculation about any long-term

changes in solar behavior to be of sufficient validity to view the finding as establishing a new solar property that need be taken into account in our current understanding of solar activity and its causes. In particular, the relationship with the various 'quasibiennial' variations seems tenuous at best. And the name [biennial] is poorly chosen as the Sun probably does not know about the terrestrial year.

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?