Nonlin. Processes Geophys. Discuss., 2, C222–C233, 2015
www.nonlin-processes-geophys-discuss.net/2/C222/2015/

Nonlinear Processes in Geophysics
Discussions

Interactive comment on "Search for the 531 day-period wobble signal in the polar motion based on EEMD" *by* H. Ding and W. B. Shen

H. Ding and W. B. Shen

wbshen@sgg.whu.edu.cn

Received and published: 17 June 2015

Response to Referee#2 (Prof. King) Referee#2 ### The authors present a new analysis of historic polar motion time series in order to see if they can resolve an apparent debate on the existence or otherwise of a 531d (∼18month) signal. Their aim is not to understand its excitation but confirm its existence, with application of the Empirical Mode Decomposition Method (EEMD). The paper is quite well written although the clarity could be improved through some editing. I was a little surprised to see this paper submitted to this journal rather than J Geodesy where the readership is more obvious - nonetheless I see EEMD has been published recently in a geodetic context in this journal. Response: Dear Prof. King, Thank you for your careful

and insightful review. We tried our best to revise the manuscript according to your comments, and have made the following overall changes to the manuscript in light of your comments: (i) We made numerous editing changes to further improve the wording as you have suggested. We also corrected some typos in the whole text. These are all highlighted. (ii) We added a new Figure 2 to explain the EEMD process, and make it be self-contained. And a new Table 2 is added as you suggested. So the orders of the figures and tables as well as the citations of their corresponding numbers in the text were corrected in the revised manuscript. (iii) We re-plotted the old Figures 1, 3, 4 and 7 as you suggested (which correspond to the Figures 1, 4, 5, 8 in the new version). (iv) According to your suggestion, we used the errors of x-/y-components in the time domain to combine with the RMS noise to re-estimate the error bars of the values listed in Table 1. We also used the 1980-1994 time series to compare with the 1978-1994 time series as you suggested. Because the EEMD is based on the waveform to filter different signals, the effect of the time-domain noise is not significant (see the following Figure A1), so we still use the 1978-1994 time series for the purpose of comparing with the results of Carter (1981 and 1982), but we added relevant notes about this context according to your useful suggestions. Specific replies to the review are stated as follows.

##
I have one major remark - the C04 time series is used by the authors as their primary product. This product is generated from a combination of historical optical observations before 1980 and geodetic VLBI since 1980. At Jan 1, 1980 the precision of the time series improves by about an order of magnitude and this is indicated by, for instance, a very large reduction in the formal errors in the C04 file. For some background: I have analysed these time series with a Kalman Filter-Smoother in the time domain (see King and Watson GJI 2015) although not for the purpose of looking at 531d signal - indeed I did not estimate parameters at this period previously, although I did include time-variable Chandler Wobble in the Filter. This paper prompted me to include it also, allowing a signal at 531d to vary in amplitude and phase in both x and

y. I confirm that any signal at 531d is no larger than estimated with a conventional amplitude spectra (no more than 25mas in each of x and y). There is evidence of a drift in phase of the 531d signal, especially in X, and increasing amplitude for the period since 1980 (10-20mas to 20-30mas). The signal before that is∼10mas in my analysis (see Figure 1 attached). My analysis makes me conclude two things. First, that the authors should repeat their analysis where they make the clearer distinction at 1980; treating data across this boundary makes use of heterogeneous data and should be avoided without first testing the effect of it. The dat before 1980 could potentially contain various aliased signal as a result of the limited number of optical stations in the analysis, and its seasonal variation due to cloud cover. I have not looked at the literature closely on this, but I think the authors must consider this heterogeneity in their analysis. Since 1980 I think the data is very high quality and should be able to be used as the authors intend. Second, it is not clear to me how the phase drift I detect would affect the authors' analysis, or if indeed it is an indication of a modulation of the kind they are discussing. I leave this to the authors to consider. The method of the EEMD is not sufficiently clearly described and be self-contained. For instance, it is not clear why IMF5 and IMF6 are differenced to interpret the 531d signal. It is also not clear why a proper vector difference (considering the actual phase) is not done. The authors must also consider temporal correlation in noise. At present they consider background noise with RMS, which assumes white noise. This almost certainly underestimates noise. The approach to estimating the amplitude spectra is not described and more rigorous uncertainty estimates are needed.

Response: Thanks for your very useful comments. Your results about the 531dW obtained by using the Kalman Filter-Smoother are very useful for us, and they can also be considered as a support for our research. In this study, our phase results of 531dW can only be considered as the phases of three time-points (namely the initial phases); however, your results even show all of time-varying phases of the 531dW. To be honest, we still don't know how to use the Kalman filter to obtain those results, but in the future we will try to learn this and try to understand the phase drift of the

531dW as you detected. We have added some relevant discussions in the section 4 'Discussion and Conclusion'. At first, we found that previously we made a mistake about the estimation of RMS noise level. The noise level lines shown in Figures 3d-3f (in the new version) correspond to correct values (see the following Figure A1âĂŤa re-plot for Figure 3d, taking it as an example), but we read some wrong values (we are negligent, and we are sorry for this!). The errors of the time series before and after the 1980s do have some differences (see Figure A2), and comparing with our improved (correct) estimated RMS noise level (see the new Table 2 in the revised text), generally, they can be considered consistent with each other (the formal errors for 1995-2013 are only about 0.1mas, while the RMS background noise level is about 5.5mas, the latter is even higher than the former; this can be understood because all un-relevant signals in target frequency band can be considered as 'noise'). Hence, based on the correct (estimated) RMS values (in frequency domain), we re-estimated the error bars for the estimated values listed in Table 1. And in Lines 181-183 we explained which method (AR method of Chao and Gilbert 1980) we use to obtain those estimations. Given the scope of this paper, we don't try to repeatedly explain the details for the AR method (which will need a lot of space), but provided three references for the details (see the literatures added in the new version). In addition, according to your comments, we added a new Figure 2 to explain the IMFs after using EEMD, and added more relevant explanations about EEMD in Sections 2 and 3. See Lines 99-107 and Lines 171-174. And we consider the reason that IMF5 and IMF6 are differenced should be the demodulation feature of EEMD (see Line 151). And in the revised manuscript, we used the vector differences to calculate the amplitude's differences between the IMF 5 and IMF 6 just as you suggested. See Line 217 and Table 2.

Figure A1. A re-plot for Figure 3d (in the revised manuscript) to show we have read some wrong values for the RMS noise level.

Figure A2. The formal errors of the EOPC04 time series (obtained from IERS).

About the starting time of the second sub-series, we have used a new time spanning:

1980-1994 as you suggested; and the IMFs of the x-components of the 1978-1994 time series and the 1980-1994 time series are shown in Figure A3 (see below). Although the errors of the time series before and after the 1980s are different (see Figure A2, generally, they are consistent with the results from the frequency domain), Figure 3A shows that the EEMD method is not very sensitive to this (namely the temporal correlation in noise will not significantly affect the results from EEMD). This is due to the fact that the EEMD is based on the waveform of the time series to make the signals with different periods to different IMFs. Hence, in order to compare with Carter (1981and 1982), we still use the 1962-1977 and 1978-1994 time series, but we have added relevant notes about your results in the 'Introduction' part and relevant explanations about the time series, see Lines 158-162.

Figure A3. The IMF 5 (a) and IMF 6 (b) of the x-components of the 1978-1994 and 1980-1994 time series, respectively. Note that the difference at the starting time of IMF 6 of the 1980-1994 time series (as indicated by the arrow) is just due to the end-effects of the EEMD (although it has been improved, this effect cannot be completely removed).

##
Otherwise, the authors did not spell check the paper before submission! Please do this- I will not point them all out. the English needs a thorough proof read. On p649 at least two of the papers cited as background work are abstracts from AGU. This is ok for me, as it is not fundamental to the work here, just the background and nice for those analysts to be recognised. Response: Thank you very much for your kind and laborious corrections. We fixed them one by one as you suggested, and modified some other spells. And for the following 'Specific remarks' part, all of those comments are responded, and we have modified the corresponding expressions one by one (Highlighted in the revised manuscript).

##
Specific remarks: Abstract: series is mis-spelled twice as seires; "while cannot" -> "but

cannot" P648 L21: "researchers *have* suggested" L23: some *have* considered. Again L24. P649 L4: the periods here are given without proper introduction L7: "band *of the CW* was .." L25. the remarks re the work of C Bizourd is not appropriate. None of their results are published (if they are, then please cite them). If discussions with them or their unpublished presentations helped, add them to the acknowledgements P650 L6: please add a citation to this statement or demonstrate it here L9: "dyadic" may benefit from a definition L25: I don't think IMF was defined yet P651 L8: Step 4 is unclear in that is it the r1 that is used in the iteration? If not, then could you recheck the description since it is not really clear. Change "let" to "letting" Response: Thank you for your very careful review and useful comments, we have modified all of those, Please see the highlighted parts in the revised manuscript. In addition, about the word 'dyadic', here we referenced it from Flandrin et al. (2004); in this paper, Flandrin et al. used the numbers of the zero-crossing of each IMFs to turn out that 'EMD acts essentially as a dyadic filter bank resembling those involved in wavelet decompositions'. However, EMD (and EEMD) can be simply understood a filter which is based on the waveform of the time series. Given that the 'dyadic' is not very easy to be understood, we delete it in the revised manuscript.

##
L18: Step 3 here cross refer to earlier step 3 which I think this step is meant to modify —Thank you for you careful review. The two "Step 3"s have different meanings, the first Step 3 (in EMD) is used to obtain a IMF, and it is contained in Step 2 in EEMD. To describe more clearly, we replaced the "steps" in EEMD by "procedures". In addition, we added some more explanations, see Lines 99-107.

##
P653 L1: on *the* original —Thanks, we have modified this, see Line 142 L2: used *on* these —Thanks, we have modified this, see Line 143 L9: the phases are opposite - do you mean IMF5 is opposite IMF6? I suggest remove "but" and start a new sentence. —Yes, thanks for your comment, we have modified this, see Line 150. L10:

what does "according to the characteristics of EEMD" mean? —Thanks, we have modified this by using ‘according to the Equation 1’, see Line 151. L11: the reason why IMf5 and IMF6 are differenced (the differencing is not clear here in the text, but in infer this from Figure 1) is not clear from the methods. —Thanks for your useful comments, we think it may be caused by the demodulation feature of EEMD, so we added some explanations in Line 151. L12: "this might be the reason" -not clear exactly what the authors refer to here. The low SNR? –YES, we have modified this, see Line 153. L14 this claim needs a reference or demonstration —Thanks, we have modified this, see Line 155. P654 L10 referene to Fig 5 should be Fig 3. It would be useful to have the equivalent to the green line from Fig 1 added to Figure 3 so we can see the background signal. —Thanks, we have added the sum of the others IMFs to the old Figures 3 and 4 just like Fig. 1. Please see the new Figure 4&5. L15-30: a lot of the differencing done in this text should be moved to a table and the text reduced to a few lines.—Thank you for your useful comments, combining with you other suggestions (namely using the vector difference), we have shorted this part, and moved some of those in a new Table 2. See Lines 203 and 218.

P655 L1: it would be useful to add that 18months is 547d, so slightly different to the period used here —Thanks, we have modified this, see Line 221. L15: "clearly" overstates it given N=3! My analysis attached also shows the amplitude of Chandler wobble reduces over the period the 531d increases and the phase drifts. –Thanks, ‘clearly’ have been replaced by ‘perhaps’, see Line 244.

##
P656 L7: delete "very well" L8: "series very likely originates from..." L15: do you mean you searched for a value of M that did produce sufficient modulation? L20: are *the* same L22: concerned -> considered? L29: delete "very well" P657 L13: considering->tuning? "of CW *to*" L16: are as -> are the L29: have *a* similar L658 L1: "nature *to* the results shown" L4: "an open question" L5: not clear what "mechanism" refers to L7: "figure out" -> "answer" L8: "find" -> confirm? "the gravity" -> "independent

gravity" (I think these data being independent is important to note here) —Thank you for your very careful review and useful comments, we have modified all of those, Please see the highlighted parts in the revised manuscript.

##
L13: define SG and GGP. It is not clear what mb and mc are. Are they products or site names or something else? —Thank you for your careful review, we have defined them, and explained what mb and mc are. See Lines 352, 355. L19: the peaks are not super well defined and suggest "Two peaks around 531 dW..." –Thanks, we have modified this, see Line 358. L24: the meaning of the phrase "global’ is not clear. Do you mean a global convolution? If so, justify that the error in using a non-global convolution is sufficient. —Here we followed J. P Boy (http://loading.u-strasbg.fr/GGP/index.html), and the ECMWF model is used. As we only show this effect in the Figure, but didn’t remove them from the residual SG records, so we decide to keep it for the readers who may be interesting for this.

##
P659 L2: delete first "the" L4: "the 531dW signals excited by ..." P660 L4: That is *the* reason —Thank you for your very careful review and useful comments, we have modified all of those. Please see the highlighted parts in the revised manuscript.

##
L11: "cannot explain" is misleading in that this paper does not attempt to explain. –it has been replaced by ‘this paper does not attempt to’. See Line 404.

There is no conclusions section and such a section is required. —we changed the title of section 4: Discussion and Conclusion, and reorganized this section. Some relevant expressions have been added. Figure 1: legends: IFM6 -> IMF5. green/red on same plot is not a good idea. Limit phase to +-pi . —We have limited phase to +-pi, see the new Figure 1; we also have tried to change the color, but find using the green color can make the sum of other IMF more clear, so we keep the same color. Figure 2:

RMS is a white noise estimator of noise. Coloured noise should be considered. —We are sorry for that we have made a MISTAKE about the RMS noise. For the x- and y-components of the 1962-1977 series, the RMS noise amplitudes are 9.5 mas and 9.2 mas, respectively; for those of the 1978-1994 series, they are 8.8 mas and 8.2 mas; for those of the 1995-2013 series, they are 5.4 mas and 5.5 mas. And according to your suggestions, we further considered the errors of x/y component in time domain, and the RMS mean errors in three different sub-series are simply combined with the RMS noise levels to re-calculate the error bars of our estimated values. See Table 1.

Figure 3&4: I think it would help the analysis to merge the top and bottom rows. —We have modified this. See the new Figures 4 and 5.

Caption: slots -> panels Figure 6 caption: "results when considering" —Thanks, we have modified this, see Line 304.

Please also note the supplement to this comment:
http://www.nonlin-processes-geophys-discuss.net/2/C222/2015/npgd-2-C222-2015-supplement.pdf

Interactive comment on Nonlin. Processes Geophys. Discuss., 2, 647, 2015.

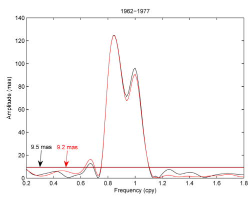

Figure A1. A re-plot for Figure 3d (in the revised manuscript) to show we have read some wrong values for the RMS noise level.

Fig. 1.

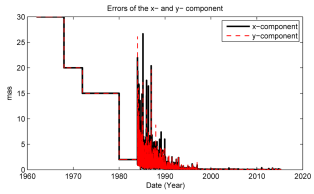

Figure A2. The formal errors of the EOPC04 time series (obtained from IERS).

Fig. 2.

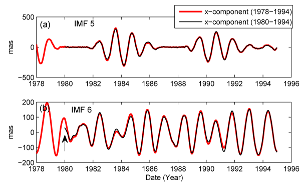

Figure A3. The IMF 5 (a) and IMF 6 (b) of the x-components of the 1978-1994 and 1980-1994 time series, respectively. Note that the difference at the starting time of IMF 6 of the 1980-1994 time series (as indicated by the arrow) is just due to the end-effects of the EEMD (although it has been improved, this effect cannot be completely removed).

Fig. 3.