

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

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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 (~18 month) 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.

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

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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 data 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.

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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.

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.

Specific remarks:

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

P648L21: "researchers *have* suggested" L23: some *have* considered. Again L24.

P649L4: 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.

P650L6: 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

P651L8: Step 4 is unclear in that is it the r_1 that is used in the iteration? If not, then could you recheck the description since it is not really clear. Change "let" to "letting" L18: Step 3 here cross refer to earlier step 3 which I think this step is meant to modify

P653L1: on *the* original L2: used *on* these L9: the phases are opposite - do you mean IMF5 is opposite IMF6? I suggest remove "but" and start a new sentence. L10: what does "according to the characteristics of EEMD" mean? 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. L12: "this might be the reason" -

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not clear exactly what the authors refer to here. The low SNR? L14 this claim needs a reference or demonstration

P654L10 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. 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.

P655L1: it would be useful to add that 18months is 547d, so slightly different to the period used here 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.

P656L7: 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"

P657L13: considering->tuning? "of CW *to*" L16: are as -> are the L29: have *a* similar

L658L1: "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) L13: define SG and GGP. It is not clear what mb and mc are. Are they products or site names or something else? L19: the peaks are not super well defined and suggest "Two peaks around 531 dW..." 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.

P659L2: delete first "the" L4: "the 531dW signals excited by ..." P660L4: That is *the* reason L11: "cannot explain" is misleading in that this paper does not attempt to explain.

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There is no conclusions section and such a section is required.

Figure 1: legends: IFM6 -> IMF5. green/red on same plot is not a good idea. Limit phase to $\pm\pi$ Figure 2: RMS is a white noise estimator of noise. Coloured noise should be considered Figure 3&4: I think it would help the analysis to merge the top and bottom rows. Caption: slots -> panels Figure 6 caption: "results when considering"

Matt King, May 26, 2015

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

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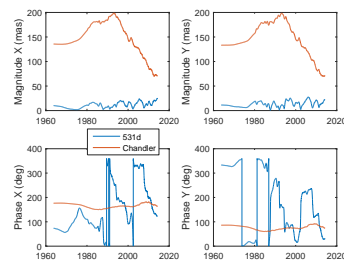


Fig. 1.

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