Dear Authors,

Given the differences in content of the reports provided by the reviewers, I read carefully your manuscript. I have to agree with the second reviewer in that you are using well-established methods as tools to investigate jerks through real and synthetic data. The tools you are using are all available through Matlab software and toolboxes. This may be of interest and could be published in SE but it requires that:

1- The applied methods are carefully tested such that their outputs are well understood.
2- The methods used provide further information on the phenomenon studied.

In the present manuscript only the DWT technique fulfill the second point; these are nearly the only results reported in the conclusion. However, as noted by both reviewers, the manuscript lack evidence that the "d_1" coefficient is really efficient and reliable in identifying jerks, particularly when applied to synthetic data. Further, I have strong reservation regarding some of the other techniques presented.

It is therefore clear to me that the manuscript has to be significantly revised and rewritten. Further, a revised manuscript will have to be fully reviewed. Therefore, I would like to discourage submission of a revised manuscript and suggest that you take the time necessary to address the main points raised by the reviewers and myself.

Are added below additional remarks:

1- In the equation (3), you define a function to test the SFTF. Why this function based on exponentials? This should be justified. Further, the notation [-0.5,0) is confusing. I suggest you use "<" and ">" signs.
2- Section 3.1.3 Fig(3) presents results for the SV. It should present also results for the components.
3- Section 3.1.4: (Fig.2) should be (Fig.1). Further, equation (4) is certainly not a "moving average applied on SV" but the difference between average values of monthly means. The main problem with your approach is the unit. For n=12 the SV estimate is in nT/y, but for n=6,9,… the SV estimate is in nT per 6 months, 9 months …. Fig-4 is therefore comparing different quantities, the unit of the y-axis is wrong. I tend to agree with reviewer-2 regarding this figure. Finally, your discussion on Huber weight is not really relevant.
4- Section 3.2.2. You cannot present a study involving synthetic data + noise without fully characterize the noise. A reference to the Matlab "warma" function is not enough. Is that a normal distribution? What are the variance and mean values? Also your de-noising technique is not sufficiently described. Furthermore, I am surprised that a Wavelet analysis requires a de-noising step. Finally (page 13, line 31) it is absolutely unclear to me (may be because of the color choice) how you can identify reliably jerk dates in NGK spectrogram. What is the interest

of your approach if the dates are not clearly identifiable? The same remarks hold for figure 5, 6 and 7.

5- Section 3.2.4 (numbered 3.2.2 in your manuscript). Fig-10 is the only place where you show how efficient the "d_1" value is in detecting jerks from magnetic model time series. Demonstrating that using NGK observatory only is not enough. A rapid comparison of fig-9 and fig-10 seems to indicate that the "d_1" estimate does not provide the same information for real data and magnetic models. Fig-10 must be revised as stated by reviewer-2 and further evidence that the technique is successful in detecting jerks given.

6- Section 3.3, Associating jerks with maximum of acceleration power is not acceptable without strong argumentation. It is only the third temporal derivative is maximum for a jerk. You may want to follow (Chulliat et al., 2010, GRL) relating Jerks and main field acceleration, but is that valid at the time scale you are looking at? How accurate is the acceleration in CM4 model? Can you trust anything above degree 3 or 4? On recent core field models based on satellite data the acceleration patterns are already strongly distorted by the regularization applied at SH degree 5.

Vincent Lesur