Dear authors,

I have been through your response and the revised manuscript. I find that if you answer to several of the concerns raised by the referees, you sometimes pass too quickly over some of their comments (see point 3 below). Furthermore, it seems to me that some fundamental literature is ignored (point 1), and that synthetic tests should be carried out for the reader to accept your results (point 2). Consequently, I ask you to bring important modifications to your manuscript before it can be published to Solid Earth. I understand that it requires some significant work, but it is needed for the reader to accept your findings.

Best Regards

1) Part of the literature that cannot be ignored. For instance in the abstract you assume that decadal changes are « supposed to be related to external variations and called '11-year constituent' ». This is part of your background hypothesis, but you must recognize that this point of view cannot entirely be correct. Of course there exist an external signal at such periods; however, internal signals are of primary importance at these very periods, in link with the red spectrum of the internal field (Lesur et al, PEPI 2017).

Furthermore, the ambiguity in the internal/external sources separation being much reduced with satellite data, the community is pretty confident in a large signal from the core at periods around 11 yrs (see e.g. the global models such as CHAOS-6, Finlay et al, EPS 2016).

2) You should explain how your results (the existence of "cyclic constituents") is compatible or at odds with the approximately -4 slope found in the PSD of ground observatory series (de Santis et al, PEPI 2003; Lesur et al 2017). Indeed, series characterized by such a PSD slope in the considered frequency range do not contain any specific spectral line. Can you prove that the cyclic behavior you see (in particular towards long periods) do not come from the restricted time-span of the series ?

Said differently, you should apply your signal analysis tool to short segments of stochastic series presenting a PSD with a -4 slope over the considered frequency range ; if there you find cyclic constituents, it means they are possibly due to the limited duration of geophysical series. My concern is motivated by the fact that apparent periodicities are often wrongly put forward when too short series are considered.

3) Below is a list of comments on your responses to the referees (in blue the referees points, in red your response or modified text, in black my comments) :

<u>on comments by Susan Macmillan</u>

3.1- More detail is needed on how the 30-year and 73-year spectral lines are identified in Figure 2 because these values are then used (at least initially) in some of the subsequent filter design...

Most observatories indicate these figures for the spectral line corresponding period… the filtered time series do not lose the information related to actual periods involved, no matter what figure is used in filtering.

You do not answer the question : how do you estimate the periods ? Is it by applying an average over the several spectra ? Could you provide errorbars on the obtained period? Behind the question of the referee, I understand that the periods are not obvious from fig. 2.

3.2- What is the reason for the split in the 3rd panel of Figure 3? NGK runs continuously through this time. (a point also raised by J. Mound)

… We treated separately the two parts of the signal (1890-1960 and 1961-2014) because the plot suggests a change in frequency that contribute to the signal…

This affirmation is not obvious at all to me, as in both parts of the series it only concerns a few periods. Furthermore, the accuracy you give for the periods seems illusory to me. I do not think any conclusion such as "beatings between the sunspot (so-called 11-year) and magnetic (~22-year) solar cycles" can be drawn. Here analyses of synthetic series (see point 2) would be useful. Indeed, stochastic series often show natural modulations that look like changes in apparent periods (although no period line actually exists).

3.3- How can you be sure that the big jerks are not influencing the results of the filtering in Figure 6? … an important conclusion potentially.

The results of filtering are not influenced by the position of the big jerks.

This is not an answer. Could you detail a bit ? Again you could illustrate this with synthetic tests.

On comments by Jon Mound:

3.4- in your modifications following the comment "… could be noted that there is a corresponding six year signal în length-of-day that cannot be explained by known external sources and thus has been linked to processes in the core…" you refer to Cox et al (2016) saying "… shows, however, that a 6-year wave in the core cannot give the estimated effects at Earth's surface, placing the problem of internal high-frequency signals under debate."

This is not correct. Indeed, what Cox et al show is that the 6 yr signal from synthetic geostrophic waves (with amplitude that found by Gillet et al 2010) is tiny, comparable with the uncertainty level in observatory series. However, around 6 yr periods, core flows inverted from magnetic data are dominated by more intense non-geostrophic motions that are able to explain the resolved signal at such periods (i.e. there is no problem for interannual magnetic signals to be explained by core motions). Furthermore, because there are about 100 independent observatories to constrain the secular variation, the uncertainty level on core motions is much reduced. This explains why such small geostrophic motions can be retrieved even if they are only responsible for a tiny signal.

3.5- You do not modify the text in response to "… I don't see any easy resolution to this problem through pure time series analysis, comparison to external field models or proxies (e.g. indices of solar activity) seem necessary to unravel the origin of the high frequency content within the geomagnetic observatory time series.". You write "Once the external contributions to the first differences of the observatory annual means – of comparable amplitude with the inter-decadal and sub-centennial constituents – are minimized, the observed secular variation no longer exhibits a clear V-shape at time of geomagnetic jerks." Following point 1 above, how can you be sure you have only removed external signals ? In your conclusion you should acknowledge that you have most probably also removed some important internal signal. This may be part of the reason why the correlation with of the 11 yr constituent with the solar cycle is not so clear. You should also mention somewhere the attempts at extracting external contributions through global models on long periods (McLeod et al, JGR 1996; Langel et al, PEPI 1996; Gillet et al, G3 2013), who give an idea of what wan be achieved on the basis of spherical harmonics decomposition, and of the expected respective amplitudes of internal and external signals.

3.6- … Therefore I might be cautious about claiming that the sub-centennial signal is really traced all the way back to 1600.

… As the sub-centennial variation in *gufm1* closely follow the observed time series in the last ~200 years of the time series depicted in Fig. 10, there is ground to give credit to the entire time series.

I don't see the logic of your response. The point of the referee remains valid, and should be explicitly acknowledged.