Dear dr.Kepko, thank you for your comments. We will take all of your suggestions into account. Point-by-point answers are given below and marked with colour.

Interactive comment on "A case study of the spectral parameters of ULF fluctuations before substorms with no evident trigger in the interplanetary space" *by* Nataliya Sergeevna Nosikova et al.

Emil Kepko (Referee)

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This paper presents a case study of ULF oscillations observed in the magnetotail by Cluster, on the ground with ground magnetometers, and in ionospheric density. The paper demonstrates coherence between the different magnetospheric regions. As with the other reviewer, I find the connection (or not) with substorms tenuous, at best. Conclusions 1 & 2 are conjecture but are not supported by the data in the paper. I suggest simply removing that aspect, so I will not repeat those concerns here – but I fully concur with the other reviewer.

We understand the problem, that a possible relation between ULF in the Polar cap and a forthcoming substorm cannot be proved with the analysis of a single event. In any case, it was not the purpose of the present study. The existence of a statistical relation between ULF in the polar cap and auroral activation was found statistically by (Heacoc and Chao, 1980, https://doi.org/10.1029/JA085iA03p01203 and Yagova et al, 2000, http://adsabs.harvard.edu/full/2000ESASP.443..603Y). However, these papers did not discriminate between the externally and internally triggered substorms.

This step was done in our previous paper (Yagova et al, 2017), where the analysis of ULF in the polar cap was undertaken intentionally for non-triggered substorms. This analysis led to the conclusion of two possible reasons of pre-substorm activations in the polar caps, the first related to increased level of ULF fluctuations in the IMF and SW plasma parameters, and the other links to inter-magnetospheric ULF waves.

The present study gives a detailed analysis of ULF waves in the magnetosphere and on the ground developing under undisturbed IMF/SW. This, on the one hand, shows a possibility of the second scenario and, on the other hand, allows us to formulate a hypothesis about properties of these waves for future studies.

In the revised version of the paper, we plan to extend the Introduction and Discussion sections to clarify the relevance of the present research in the problem of substorm triggering and, in particular, of ULF activity in the polar caps and the magnetotail.

I have some questions regarding the spectral analysis and data presentation. With respect to external vs. internal driving of the oscillations, I would encourage the authors to examine the solar wind number density, in addition to the IMF, as the solar wind dynamic pressure – whose variations are driven primarily by number density variations – is the largest driver of ULF pulsations in this frequency range. The fact that these pulsations were observed in the polar cap certainly limits the internal mechanisms.

In the revised version we will include an analysis of the solar wind dynamic pressure and number density fluctuations. The physical mechanisms for the MHD wave excitations on the inhomogeneities on open filed lines will be also discussed in the revised version of the MS (see, e.g. Pilipenko et al., 2005, https://doi.org/10.1029/2004JA010755)

I also would like to see a bit more information on how the spectra were computed. The paper mentioned a low-pass filter – what was the filter type? It is important to present those details, as certain techniques (e.g., running average) can introduce spurious spectral signals.

The Data Analysis section will be extended, and all the details of filters and spectral estimates will be given. We understand the problem of the artificial peaks originated from the non-adequate filtering. To exclude any uncertainties, we've plotted the non-filtered signals with removed trends.

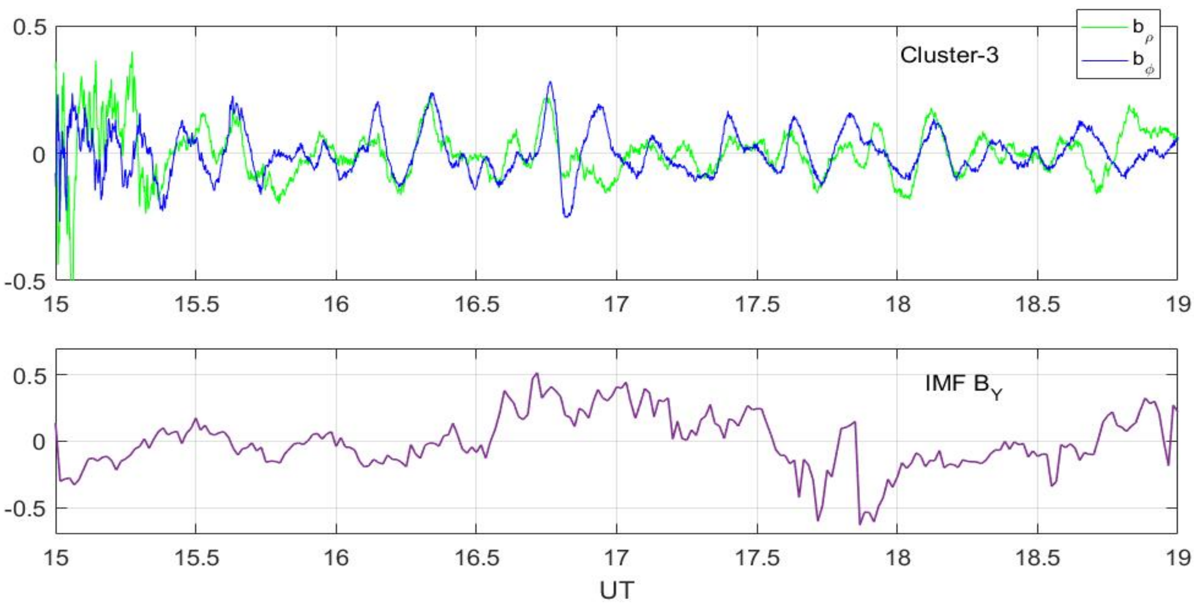

Fig.1. Signals with removed trends seen in (from top to bottom): the magnetotail in Cluster-3 transversal components; IMF By component.

The spectra look very smooth, so I'm curious as well how those were computed. The paper states Blackman-Tukey but, specifically, what is the effective Rayleigh frequency of the analysis? The spectra look very smooth. Finally, it is difficult to judge the purported peak near 1.5 mHz without seeing the lower frequencies. I Suggest plotting to the zero frequency so a reader can see the context for this peak. Relative to the background, the peak near 1.5 looks little different than other peaks (e.g., near 3 mHz). Plotting to lower frequencies would help the reader be able to determine significance, at least visually.

The Blackman-Tukey method is well known and widely used in geophysics. In contrast to methods such as maximal Entropy (MEM), it has a worth frequency resolution, but it estimates PSD with a dispersion which decreases with spectral smoothing. In fact, the parameters were chosen as a compromise between the frequency resolution and dispersion of spectral estimates. The spectra of non-filtered signals are given in Figure 2. It can be seen from the Figure that the main spectral maximum at 1.5 mHz is not a result of filtering. Besides, the nearly 11-minute pulsations are clearly seen in time series, as well (fig.1).

In the revised version of the manuscript, we will give a description of the technique in more detail.

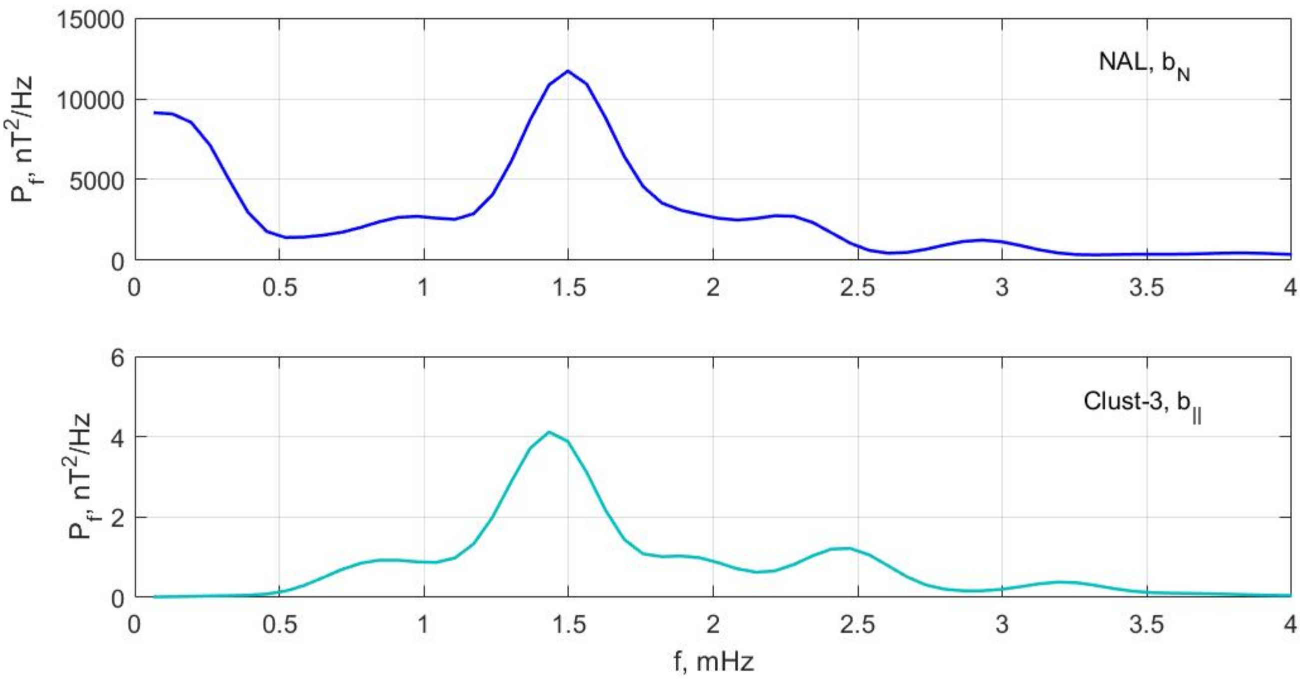

Fig.2. PSD spectra for (from top to bottom): NAL ground-based magnetic data; Cluster 3 field aligned component.

There are statistical tests one could apply as well (see our recent Kepko, Viall & Wolfinger paper for one, but there are many others), and I would encourage looking at those, although the cross-coherence is an indicator that these are geophysical phenomena.

Thank you for the method suggested. We will consider the possibility of applying this technique to our future studies.