Formal Review for Authors

The manuscript is on a case study of multi-period electron flux modulation observed with energy range of ~60 to ~200 keV using in-situ BD-IES instrument of a Chinese navigation spacecraft. Following the standard understanding of ULF pulsation interactions with electrons, the manuscript tries to address this interaction as the primary cause for the flux modulation considering the observed delay time between the oscillating signatures of the electron fluxes among different energy ranges. Thereafter, the manuscript tries to offer a more generalized formalism by inclusion of the typically-neglected term in the theory which is Betatron acceleration. The manuscript was not very easy to review since it has a considerable amount of mathematical equations. The reviewer followed and rederived all the math and mathematical relations presented in the manuscript. The manuscript managed to show analytically the difference between the conventional and the more generalized calculation results. The figures are clear and inclusive. There are a few serious ambiguities and typos in the manuscript as well as one miss-calculation that needs to be addressed/resolved since of which might lead to different results and conclusions. The authors are required to be more specific about which parts of their reasonings, assumptions and calculations are originally developed for the first time here by themselves and which parts they are borrowing the ideas from by including proper references when needed. The reviewer also tries to give suggestions and references to make some of the reasonings completer and more related to the main context.

Overall, the ideas and methods presented in this paper could be important enough for publication; however, there are certain issues in both scientific content and presentation that ought to be dealt with first before the reviewer can decide and suggest the manuscript for publication. The reviewer categorized his review and comments into three classes: comments on science, comments on figures, and minor comments. The reviewer hopes the authors find them helpful and informative:

Comments on science

Line 7: "We adopt the calculation scheme therein to derive the electron energy change in a multi-period ULF wave field"

> Are there other kinds of schemes to explain this energy change in ULF wave field? If yes, the authors are suggested to briefly talk about at least one of them and explain why they choose the specific scheme.

Line 23: "…, called the resonance energy, at which the particles would experience a stable electric field during their drift motion, …"

> What about magnetic field experienced by particles?
> Also, "stable" might not be an accurate or even a necessary word here. The unstable force could also affect particles as long as the resonance condition is met.

Line 39: "In addition, ULF waves in the magnetosphere have been found to be asymmetrically distributed (e.g. Takahashi et al., 1985; Liu et al., 2009), whereas a symmetric ULF wave field is assumed in the conventional drift-resonance theory."

> Mentioning recent works that address the distribution of the ULF waves is suggested. For example, Barani, M., Tu, W., Sarris, T., Pham, K., & Redmon, R. J. (2019). Estimating the azimuthal mode structure of ULF waves based on multiple GOES satellite observations.

Journal of Geophysical Research: Space Physics, 124, 5009–5026. https://doi.org/10.1029/2019JA026927. Figure 5 (a & b) as well as figures 6 of the mentioned paper and the explanations therein clearly show that the wave can be azimuthally localized.

It is also suggested that for the sake of clarity authors mention which "symmetry" they are talking about; the symmetry mentioned in the Figure 2 of Southwood and Kivelson (1981) or they are generally talking about the homogeneity of ULF amplitude (power) pulsations in the azimuthal direction? If the latter is meant, bringing the word "symmetry" could be miss-leading.

Line 63: "The electron flux data in this study are obtained by the BeiDa Imaging Electron Spectrometer (BD-IES) onboard a 55◦ inclined geosynchronous orbit (IGSO) spacecraft of China"

Since the study is conducted for equatorially mirroring electrons (~0 degree pitch angle), the following questions must be addressed in section 2 of the manuscript:

Did the authors project (map) the data to the equatorial plane? if yes, which scheme/methods they applied. If not, how this 55-degree inclination would affect/alter the results.

Which level of the data is used? What is the spin period and the spin axis direction of the spacecraft? Is the de-spun data used for the analysis in this manuscript?

Line 86: The residual flux, defined as $(J - J_{Avg})/J_{Avg}$, represents the flux variation normalized to the background flux so that the relative change of the particle flux caused by the waves can be quantitatively compared across different energy channels.

As the authors correctly stated later, the J_{Avg} is not necessarily $J_{background}$. So, this might be misleading to name J_{Avg} as background flux.

Line 93: "… to display the wavelet power spectrum."

Some basic information about the way the authors conduct the Wavelet analysis is highly suggested and should be addressed such as: Name of the mother wavelet, the cadence of the data used as the input to the wavelet computation functions, the scale (frequency) range selected for the wavelet analysis.

The reviewer (for his reference) is interested in learning about the scale (frequency) spacing in the output of the wavelet function. (In other words, what is the y axis spacing/resolution between the data points in Figure 2 and which shading method was used in the color-coded visualization in that figure?)

Line 94: "As the wavelet power is proportional to the square of the oscillation amplitude … ."

Please pay attention that this is the case for *any* power hopefully regardless of the spectral analysis methods such as FFT, WFFT, Wavelet, … .

Line 100: "Besides, the electron flux modulation exhibits a *dispersive characteristic*."

What kind of dispersive? In other words, dispersive with respect which quantity?

Line 112: "For *a symmetric background magnetic field*, the unperturbed drift orbit of an *equatorial mirroring particle* can be given by … "

Here in the model the authors look at ~90-degree pitch angle (or equatorially mirroring) electrons while in the measured flux data omni-directional particles' differential flux was analyzed. How this discrepancy can be addressed? Further information/reasoning or references seems to be needed to make sense of this omni-directional choice specially for those whose primary research focus is not looking at particle flux/ phase space density data.

Line 123: "Therefore, particle flux modulation caused by drift-resonance would present a characteristic 180◦ phase shift across the resonant energy."

It is strongly suggested that the authors bring some key previous works observing this phenomenon.

Line 129: "Then, when the wave starts damping, the phase shift would keep growing as the drift velocities of the particles depend on their energies"

Clarifying the connection between *dependency of particles' drift frequency on their energy* and *growth of the phase shift in δW* after the onset of wave damping is suggested here even by adding one/few sentences.

Line 135: "While the characteristic particle signatures of drift-resonance predicted by these prevailing theories have been proved by *recent spacecraft observations*, the particle energy change therein is derived in an incomplete way"

References on recent spacecraft observations that proved the mentioned drift-resonance driven behavior of particles flux should be added.

Line 140: "The Betatron acceleration caused by the curl of the wave electric field, denoted by $\frac{\mu}{\gamma}\frac{\partial B}{\partial t}$, is omitted in *those drift-resonance theories*.

Again, referring to some of the main papers (in addition to Southwood and Kivelson (1981)) on those theories is needed.

Line 141: "One might neglect this energy change, because the *magnetic field of fundamental mode waves has a node at the equator*. Especially in the case of a purely poloidal wave, the perpendicular component of the wave magnetic field B_r *can be identically zero in the equatorial plane*"

The reviewer's immediate understanding is that this reasoning might not be necessarily valid for all ULF wave cases. For example, for broadband ULF pulsations which is different from the field line resonance oscillations we do not necessarily have a node in the equatorial plane.

Looking at equation $\frac{dW}{dt} = q\boldsymbol{E}.\boldsymbol{u} + \frac{\mu}{\gamma}\frac{\partial B}{\partial t}$, we can see that vanishing/small B_r has no effect on making the $\frac{\partial B}{\partial t}$ term zero. Since it is the change of B in time that determines the second term, not the B itself. If the authors meant $\frac{\partial B_r}{\partial t}$ (not B_r itself) in the text, the explanation/reasoning should be provided since in the fundamental mode (Figure 2 panel (a) in Southwood and Kivelson (1981)) the temporal change of B in the equatorial plane is actually maximum.

Line 143: "However, even then, there would still be a non-negligible change of magnetic field magnitude, because there should be a parallel wave magnetic field B_z according to [the] Faraday's law."

Correct, although we do not have to assume that there is always an E_φ. Since $\frac{\partial E_r}{\partial \varphi}$ can also give non-zero B_z.

Line 146: "Note that, for poloidal waves, $\nabla \times E$ is controlled by $\frac{\partial E}{\partial r}$ since E is in the azimuthal direction. Consequently, the particle energy change would be greatly influenced by the radial gradient of wave electric field amplitude, although the particle drifts at a constant L shell in the unperturbed orbit approximation."

If the reviewer understood correctly, poloidal component for dipole field at equatorial plane means B_r, and following Faraday's law, $\frac{\partial B_r}{\partial t} = -(\nabla \times E)_r = -\frac{1}{r}\frac{\partial E_z}{\partial \varphi} + r\frac{\partial E_\varphi}{\partial z}$. Therefore, the reviewer does not see the effect of $\frac{\partial E}{\partial r}$ for poloidal component of pulsations, and more explanation by the authors is necessary here.

Also, the readers should notice that only in a pure dipole field, the poloidal means B_r at equatorial plane. However, in the more general case of non-dipole or extension of the study to non-zero magnetic latitudes, it does not have to only be B_r.

Line 153: "The background field is given by $B_0 = \cdots = \frac{B_E}{r^3} e_z$ where …"

In the denominator, there should be L instead of r. It is suggested the authors review all of the derivations and make sure that this typo/mistake was not present in their calculations.

Line 157: "The poloidal ULF wave fields can be given in a *general* form by $\boldsymbol{E}_1 = -\frac{\partial A}{\partial t} e_\varphi \triangleq \ldots$"

It is worth noticing that in a general case there is an electric potential term. So, the *general* form for an electric field can be $\boldsymbol{E}_1 = -\boldsymbol{\nabla}\phi - \frac{\partial \boldsymbol{A}}{\partial t}$. Therefore, it would be very useful if the authors could shortly talk (or bring references) about why $-\nabla\phi$ is neglected here and how likely the electrons face a free electric potential field in the magnetosphere during their drift around Earth.

Line 158: "For fundamental mode waves, it is reasonable to further assume that the amplitude of the wave does not vary in the vicinity of equator (i.e. $\frac{\partial \boldsymbol{A}}{\partial z} = 0$)"

Please look at the reviewer's comment under Line 141.

Line 162:

Relation (4) in the manuscript is neat. It is very good that the authors quantitatively explain the correction while considering a more general case although yet limited to the odd (and probably not even) modes of field line resonances.

Line 184 Equation (6):

In the exponential, there we see $t_{0,n}$ while it cannot be a constant. Having a constant time in the exponential means F_n would not give a wave behavior. The reviewer's understanding is that it should be t instead. Otherwise, authors are required to explain how the wave behavior is represented in the mentioned equation.

Line 190 Right hand side of Equation (8):

If we take time derivative of $\boldsymbol{A}$, we will get two terms: one is exactly what we see in Equation (8), another is proportional to $i\omega_n$ multiplied by the error function. Explanation of why we should not get the second term (or if we get, why that term must be neglected) is necessary here.

The reviewer stops here and would let the authors provide their reasonings, explanations, and corrections since the final results and conclusion might depend on these corrections.

Comments on Figures

Figure 1:

What are the narrow white-color vertical lines in the spectrogram (a) panel?

The reviewer suggests bringing the MLT and MLAT information to the readers' attention as captions under panel (d) or by briefly addressing them in the text. There, no information of the solar wind (such as solar wind P_{dyn}, IMF B_z, as well as geomagnetic Dst, and AE) is given, and the readers would not be able to track any relation between the plots and the geomagnetic and solar wind indices. Following or not following the mentioned suggestions here is not critical and would not affect the reviewer's final decision.

Figure 2 and line 82:

Is the dimension of the plots amplitude squared, or amplitude squared over frequency? If it is just the power (former case), there should be an explanation on how the power is deduced (i.e. is it an averaged value or summed over scale (frequency) range from … to …) since typically the spectral analysis codes give the analysis in terms of power density not power.

Minor comments

Line 11: "wave *electron* field"

It should be wave *electric* field.

Line 52: "First, we revisit the *origin* drift-resonance theory…"

Do the authors mean *the origin of the* or they mean *original*?

Line 54: "We show that the Betatron acceleration caused by the curl of the wave electric field, which is omitted in these theories, is comparable with the energy change caused by the poloidal electric field along the drift trajectory of the particle"

Before *Betatron acceleration* there should be *energy change due to*.

Line 72: "The IGSO spacecraft with BDIES onboard passes through the *radiation belt* twice per orbit. Figures 1a and 1b show the electron flux in a full pass of the spacecraft through the *radiation belt* in the format of *spectrogram* and series *plot* respectively."

It is suggested that before the words radiation belt there should be *outer*.

Plot should be *plots*

In the spectrogram, did you use any kind of interpolation or smoothing?

Line 95: "…, the upper limit of the colorbar *for each energy channel* is chosen to be the square of the mean value of the electron flux in the selected interval from 10:15 UT to 11:15 UT and the widths of the colorbars are consistently set to be *2*."

Which Figure the manuscript is pointing to?

Dimension of the value should be mentioned: for example, is it power or power over frequency?

Did you detrend the signals? If yes, what was the detrending method and width of the moving average scheme?

Line 112:

"For a symmetric background magnetic field, the unperturbed drift orbit of an *equatorial* mirroring particle can be given by … "

It should be *equatorially* mirroring particles.

Line 143: "However, even then, there would still be a non-negligible change of magnetic field magnitude, because there should be a parallel wave magnetic field B_z according to *the* Faraday's law."

the should be deleted.

Line 165: redundant *the*

Line 165: "For the empirical electric field model denoted by $E_\varphi \propto \exp[\sigma r]$ (e.g. Perry et al., 2005; Ozeke et al., 2014), …"

Which paragraph or equation number in the aforementioned references the authors are specifically pointing to?

As σ is set to be a positive value, the authors should bring a proper reference or explanation on the limitation of the exponential behavior with *positive* power in E_φ.

Line 171: "According to Li et al. (2017b), this dispersive characteristic implies that the ULF waves were azimuthally confined…"

Again, referring to a direct measure/evidence of this azimuthally confined ULF waves power (such as Barani et al. (2019)) would strengthen the authors' argument on the validity of defining an envelope in the azimuthal direction.

Line 178: "The constant factor $A_{0,n}$denotes the *amplitude* of the wave. The second term $G_n(r)$ *describe of* wave *amplitude* in the radial direction"

If defining two different amplitudes for a wave is not common, the reviewer suggests using a different language for the sake of clarity avoiding two-amplitude language.

describe of should be *describes*

Line 178: "The third term $H_n(\varphi)$ =… is a von Mises function, …"

As H_n here is not just function of n and φ, it would be more inclusive/accurate to use the notation $H_n(\varphi|\varphi_0, \xi_n)$ instead of $H_n(\varphi)$.

Line 182: "…is the zeroth-order *modified Bessel function.*"
It should be *modified Bessel function of the first kind.*

Line 182: “The von Mises distribution is an analogue of the normal distribution.”

The reviewer suggests adding words like *in the rotational/periodic scheme* at the end of this sentence.