Response to reviewers

Reviewer #1 Evaluations:
I thank the authors for the reply. However, my most major comments remain valid:

1. The database still does not provide radial diffusion coefficients. A radial diffusion coefficient is independent of magnetic local time, yet the database provides quantities that clearly depend on MLT (Fig.4).
The answer to this comment seems to be that “in order to achieve a full MLT coverage, one would need a large multi-satellite dataset which would span several years” (l.150-151), and since this study relies “solely” (l.152) on THEMIS data, the MLT coverage is limited.
=> This explanation is confusing. This study relies on 9 years of multi-point magnetic and electric field measurements by THEMIS A, D and E (l.3-4). Thus, it provides full MLT coverage. Drift-averaged radial diffusion coefficients can be obtained from this data by averaging PSDs (phase space density) over all MLT (magnetic local time) bins. Yet, this has not been done.

In Sandhu et al.’s study (2021), how the PSD varies with MLT is discussed, but the radial diffusion is then calculated by averaging over all MLT bins and performing superposed epoch analysis. In other words, Sandhu et al., who computed DLL using Van Allen Probes data only, do not claim to provide “event specific” DLL.

It seems that the objective of this study is to provide “event-specific” radial diffusion coefficients, using solely THEMIS data, assuming “uniform distribution of wave power in azimuth” (l.143). This is problematic. The data analysis clearly shows that this assumption is not valid (wave power depends on MLT): This demonstrates that the data products are, at best, poor estimates of the magnitude of radial diffusion. Under these circumstances, the objective of providing “event-specific” radial diffusion coefficients using solely in-situ spacecraft measurements appears unrealistic.
To address this comment, I would suggest to either:
- Remove the time series of data products equated with radial diffusion coefficients in the database, and compute statistical DLLs, following an approach similar to what Sandhu et al.’s did

- Or Rename the data products using a totally different terminology, so that they are not mistaken with radial diffusion coefficients by a hasty reader.

In this case, it would still be interesting to see the DLLs that would result from averaging over all MLT bins.
In all cases, Figure 4 should be removed, together with any discussion of DLL with MLT. In addition, the manuscript should not claim to provide “event specific” DLLs.

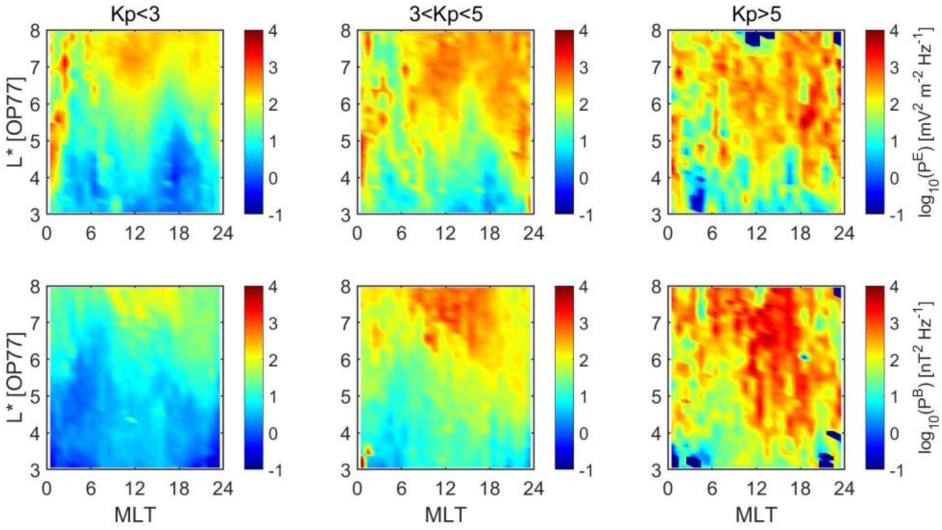
Our response: As we have already thoroughly discussed in the revised manuscript: “Even though we have followed a well-established methodology in order to calculate - as accurately as possible - the ULF PSD and the corresponding DLL there are still worth-mentioning assumptions, which are based on the theoretical approach we have used as well as on the inherent limitations of the in-situ data”. Of course, this is true not only for the DLL calculations but for every (without exceptions) work attempting to combine theoretical approaches with actual observations in the field of space physics. In this case, an important assumption is that we infer DLL using a small fraction of MLT coverage provided by the three THEMIS spacecraft.

The reviewer refers to Sandhu et al, 2021 indicating “the radial diffusion is then calculated by averaging over all MLT bins and performing superposed epoch analysis”. We believe that there is a misunderstanding here. It is quite obvious that the methodology followed by Sandhu et al. is similar to ours. Sandhu et al. also discuss time-series of DLL (see figure 1 in Sandhu et al. 2021). Of course, it is impossible for these DLL to be drift averaged, simply because of the limited MLT coverage of RBSP. Then, indeed, a superposed epoch analysis is performed but only in order to highlight statistical features. There, the authors **assume** that, since several events are considered, there is a full MLT coverage by RBSP satellites. We have adopted exactly the same assumption in figure 5 of our manuscript, where we compare ICME and SIR driven events using superposed epoch to reveal statistical features. Note that even figure 5 in Sandhu et al., which shows the median of the superposed epoch analysis, also shows DLL time-series in the background. Regarding DLL time-series, calculations similar to our own have also been published in several recent papers. Examples include Olifer et al. 2019 (see figure 6 in the corresponding paper) and Jaynes et al. 2018 (see figure 4 in the corresponding paper). The aforementioned examples, and our own work, highlight the fact that, even though the MLT coverage of in-situ data cannot provide the ideal full azimuthal distribution, this is the only way to calculate in-situ time-series of DLL.

We would like to stress that, unlike several other works, we have already included: a) a dedicated subsection stressing out all the assumptions of our approach and b) extensive discussion on the limitations of this approach. Therefore, we find the suggestion of the reviewer to rename the DLL product “so that they are not mistaken with radial diffusion coefficients by a hasty reader”, unjustified. However, in the interest of accuracy, we have replaced the sentence in line 113 “These drift-averaged radial diffusion coefficients” with “These grouped radial diffusion coefficients”. For the same reason, we have replaced the term “event-specific DLL” with “DLL time-series”, throughout the entire manuscript.

The reviewer alternatively suggests to: “Remove the time series of data products equated with radial diffusion coefficients in the database, and compute statistical DLLs, following an approach similar to what Sandhu et al did”. Sandhu et al. derive statistical DLLs which are calculated as median values over 45 storms and these DLLs span approximately 2 orders of magnitude, while the actual DLL time-series can span 6 orders of magnitude (see also figure 5 panel d in Sandhu et al). This plainly demonstrates the fact that a statistically inferred DLL using the superposed epoch analysis will overestimate/underestimate the actual DLL time-series by several orders of magnitude, simply due to the long-period averaging, and thus, will be a less favorable option for use in modeling/simulations. Such a feature is even more pronounced in statistical studies that use median values of entire datasets over a long time period (e.g. Ali et al 2016, etc) and an important disadvantage of most semi-empirical models. Indeed our work, and others (e.g. Olifer et al. 2019; Sandhu et al. 2021; Drozdov et al. 2020), have shown that these empirical models can underestimate the DLL by orders of magnitude especially during intense geomagnetic activity. All the above indicate that statistically inferred DLLs with the use of superposed epoch analysis would be – as the reviewer states – unrealistic for use in simulation/modelling efforts.

Furthermore, we have explicitly stated in the revised manuscript that: “…the MLT dependence reflects directly the azimuthal distribution of wave power for both the magnetic and the electric component of the DLL. This means that even though the radial diffusion coefficient is calculated with the drift-averaging assumption, in practice, the limited MLT coverage from single mission in-situ data introduce an azimuthal structure, which accounts for the coupling of external and internal ULF generation mechanisms and may be quite important for future modelling efforts”. We have also included in the response to the reviewers, the corresponding distribution using ULF wave power instead of DLL only to show that they are quite similar.



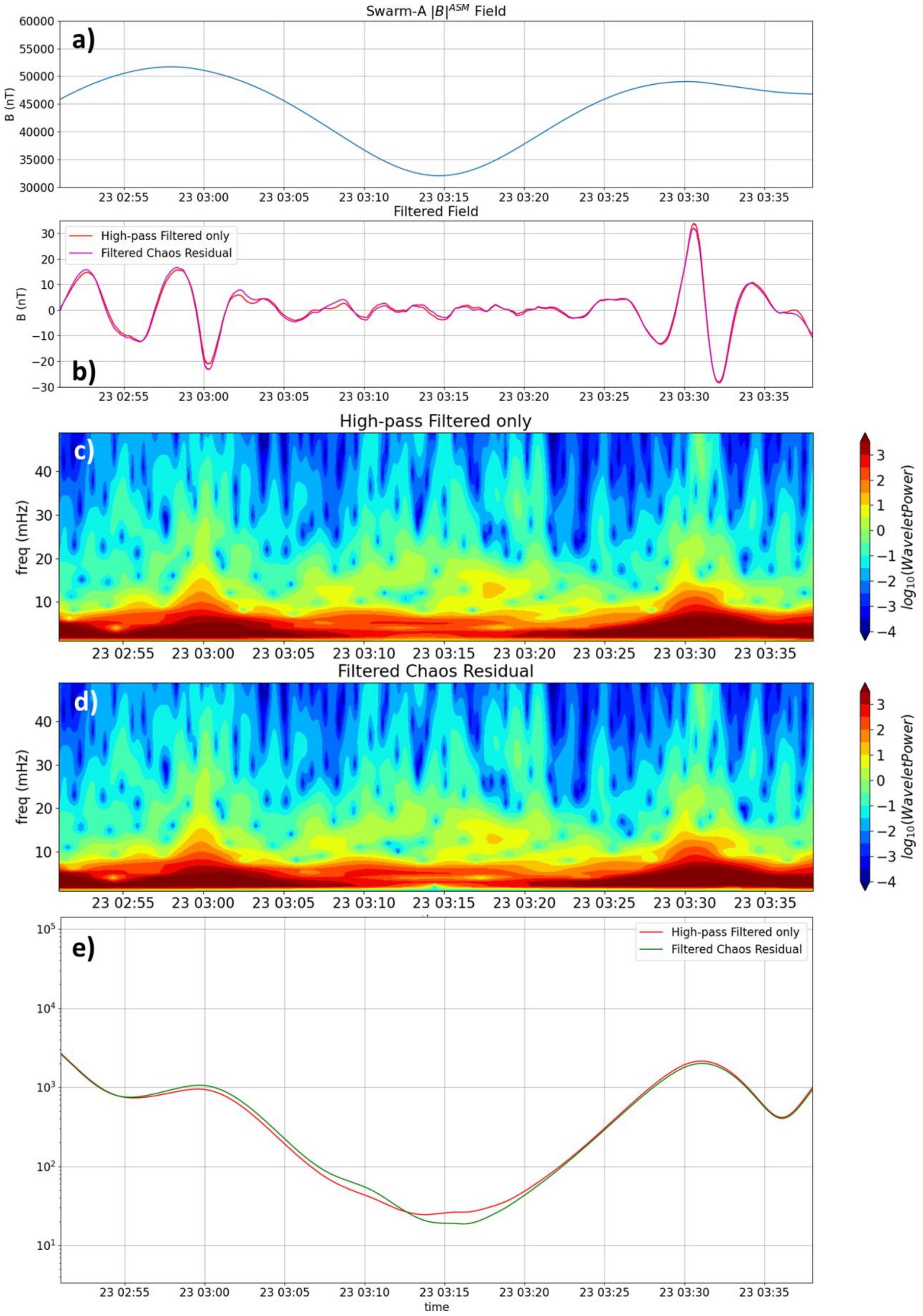
Finally, the reviewer states: “This demonstrates that the data products are, at best, poor estimates of the magnitude of radial diffusion”. We believe that this is an overstatement and rather degrading of our work. Every attempt to calculate diffusion coefficients is nothing more than an estimation and all the approaches that have been developed and used have their advantages and disadvantages, without any exception (see for example discussions in Brautigam and Albert, 2000; Ozeke et al. 2014 and Lejosne, 2020). We emphasize that we are showing, using actual simulations with the established Salammbo code, that our DLL time-series, not only are comparable with widely used empirical models (i.e. the Boscher model also used in Salammbo simulations) but can also produce much better and meaningful results (see also figure 7 in the revised manuscript). Therefore, we consider this work a worthy contribution to the space physics community, compared with statistically inferred DLL and semi-empirical models.

2. The data processing remains unclear:
Quantifying radial diffusion requires differentiating between: (a) the time variations of the fields that are faster than trapped particles’ drift period (responsible for non-adiabatic effect), (b) the slow field variations, occurring on a time scale slower than the drift period (responsible for adiabatic effect) and (c) the spatial variation of the fields (responsible for the trapping). Field variations measured by a spacecraft along its trajectory include all 3 sources of variations. The first component, (a), is the only component of interest for radial diffusion (i.e. we only need to use the time variations of the fields that are below energetic particles drift frequency when computing the PSD), and this component depends on the population energy (e.g. Falthammar, 1968). The manuscript does not clearly explain how this component is isolated, and it does not quantify the error accompanying the estimate.

Our response: Indeed the time variations of the fields that are faster than the trapped particles’ drift period are the important component for the calculation of the DLL. As thoroughly discussed in the revised manuscript, those are isolated from the slow field variations by de-trending the time-series using a 20-min moving average which is similar to a low-pass filtering with a cut-off frequency at ~0.8 mHz. This means that we keep oscillations with frequencies higher than 0.8 mHz, which are the ones responsible for the breaking of the third adiabatic invariant of relativistic electrons. We note that since the spatial variations, especially at the radial distances corresponding to the outer belt, are usually slow variations, they are also filtered out by this procedure. In the following figure we provide an example using the Swarm-A magnetic field data and the Swarm-based CHAOS-7 Magnetic Field Model (Finlay et al. 2020), which can model the internal magnetic field (core and lithosphere contribution) with very high accuracy, thus allowing us to remove the contribution of spatial gradients. The top panel (a) shows the total magnetic field by Swarm-A, and panel b the two processed signals, one after high-pass filtering only and the other after first subtracting the CHAOS model and then filtering. There are some minor differences, but they are almost inconsequential. Panel c shows the PSD of for the high-passed filtered time-series, while panel d, for the high-passed filtered residual after subtraction of the CHAOS model. Finally, bottom panel shows the weighted average wavelet power at all frequencies for both the high-passed only and for the high-passed filtered residual after subtraction of the CHAOS model. As illustrated, even for a low altitude satellite, such as Swarm-A, which is orbiting at approximately 400 km, the frequency content of the signal is almost identical, regardless of the subtraction of the CHAOS model, which is used to remove the spatial variations of the field. For satellites at higher altitudes (such as THEMIS used in our study), where the magnetic field gradients are not nearly as steep, we can safely argue that applying a high-pass filter is enough to remove the variations that are due to the satellite flying through an inhomogeneous magnetic field.

Nevertheless, the spatial variations of the field at very low L-shells can occasionally introduce errors in the estimation the background trend. Thus, wherever needed, we have completely removed the part of the spectrum, where steep spatial gradients exist, manually.

Finally, since we have adopted the Fei et al. 2006 approach we note that the energy dependence is included in the mathematical form of the DLLB component (see also equation 2 in the revised manuscript). We emphasize that, as discussed by Lejosne and Kollman [2020]: “Although Fei et al.’s formalism is inadequate from a theoretical standpoint, it is very convenient from a practical standpoint. It is indeed difficult to differentiate the induced and electrostatic components of an electric field measurement. This poses a serious problem when it comes to applying Fälthammar’s formalism to quantify radial diffusion. The same problem is circumvented when applying Fei et al.’s erroneous formalism.” To that end we have included both a brief discussion and the corresponding reference.



l.91, regarding the MFA: “The unperturbed field [is] obtained by a 30-min running average of the fields magnitude” + l. 95: “ the toroidal and compressional component of the electric and magnetic field, respectively, were detrended using a 20min moving average”: is the data averaged twice?

Our response: The compressional component of the field is obtained by the following mathematical form:

$$B_{com} = \Delta B \cdot \frac{B}{|B|} = (B - \bar{B}) \cdot \frac{B}{|B|}$$

where $\bar{B}$ is the unperturbed field obtained by the 30-min running average. The 20-min running average is then used in Bcom and Etor, as clearly described in the revised manuscript, in order to isolate the fast from the slow field variations. Note that the corresponding description has been revised to clarify the transformation (lines 90-96 of the revised manuscript).

l.98: "PSD of the waves in the 2-25mHz" => how is the PSD adapted to the energy of the population (or first adiabatic invariant)?

Our response: According to Fei et al. the energy of the population is included in the magnetic component of the DLL and the mathematical form is shown in equation 2 of the revised manuscript.

l.101: "4s PSDs were averaged in 1 min windows" => what is the motivation behind this step?

Our response: This step accounts for the economy of the dataset. A 4-sec resolution dataset for 9 years would result to ~71,000,000 data points per satellite, while a 1-min resolution dataset has ~4,700,000 data points per satellite. As one can imagine, the latter is less CPU intensive and makes it easier to perform statistical studies. Moreover, the 1-min is chosen because the THEMIS satellites exhibit negligible ΔL and ΔMLT in this timespan.

l.105-109: regarding the removal of spatial field gradients: this seems to be the motivation underlying the fact that PSDs estimates are not provided at low L shells (below L =3). Yet, errors due to the spatial variations of the fields (component (c)) occur also at high L values: The error only increases with decreasing L. What's the level of error created by the spatial variations of the field? And how does it vary with L? This needs to be included in the manuscript so as to demonstrate that L = 3 is the correct cut-off value, and so as to quantify the error in the PSD.

Our response: We would like to highlight, as it has already clarified in the lines 105-108 of the revised manuscript, that we have not chosen L=3 as a cut-off value. Actually, the PSD calculations in the database cover L values down to L~2. In, detail, the part of the spectrum (in radial extend) in each satellite's orbit which, is dominated by the spatial gradients, depends on the level of geomagnetic activity. During intense activity levels (e.g. St. Patricks 2015 storm) the spatial gradients are important at very low L-shells (below the slot region), while during moderate or quiet conditions may span several L-shells up to L=4.5. Since there only a handful of events during Solar cycle 24 which are intense enough, one can understand that the amount of data-points at L<3 are significantly less and would introduce statistical errors in the binning process and, thus, the final statistics.

Concerning the error due to the spatial gradients at high L-shells we have already shown that it is negligible.

We have also revised the corresponding paragraph in the “Data and methods” section as follows:

“Note that, as the satellites move inbound and outbound with high velocities at low L-shells, the magnetic field measurements exhibit, not only orders of magnitude increase but, very large gradients as well. Therefore, the spatial variations of the field at low L-shells can no longer be removed due to the filtering process, thus, we remove them manually.”

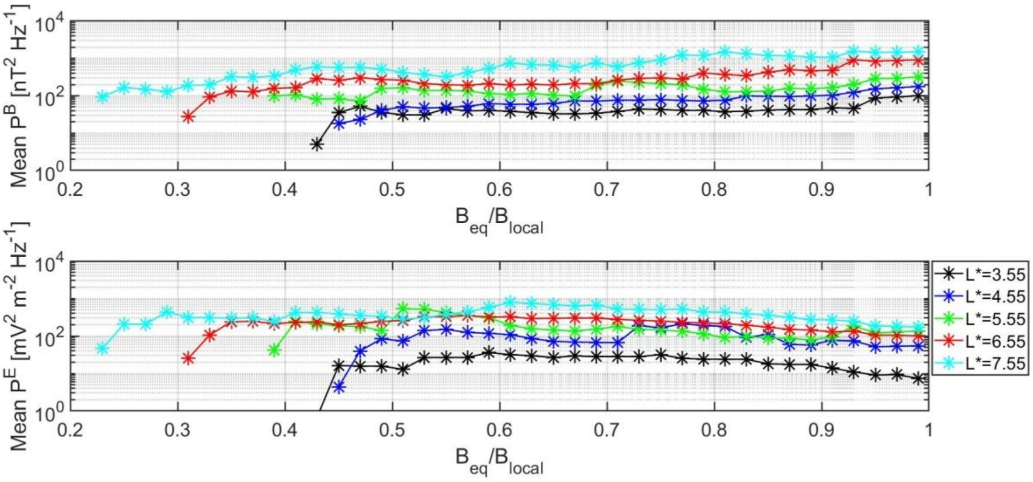
Other comments:

l.116 “ drift-averaged coefficients”. This is a misleading statement that needs to be removed. Drift averaged means drift phase averaged. Thus, it implies both spatial (MLT) and temporal averages. The data products in the database are only averaged over 1 hour in UT, and they are not MLT averaged.

Our response: As already mentioned, we have replaced the sentence in line 113 “These drift-averaged radial diffusion coefficients” with “These grouped radial diffusion coefficients”.

l.157: “the uncertainty in DLL will be rather small”: please provide quantification to support this claim, or reformulate.

Our response: We believe that we have already quantified this in figure 2 of the previous response to the reviewers. Nevertheless, we include figure 2 here as well.



The figure illustrates the distribution of the mean PSD versus the Beq/Blocal ratio for different values of L* from all three THEMIS spacecraft. As shown, both in the electric and magnetic field power, the variation of power versus Bratio is up to a factor of 2, at least for Bratio values larger than 0.8. We highlight that this 0.8 threshold in Bratio is used in our study (line 155-156 of the revised manuscript) so all results correspond to DLL values at points with Bratio>0.8.

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Reviewer #2 Evaluations:
General comments: The authors have implemented all the revisions requested, and significantly improved the structure and clarity of the discussions. The results are interesting and important, and I recommend this paper for prompt publication. If possible, I recommend the following grammatical / typographical revisions, but I think these can be implemented in the copyediting stage and should not hold up publication.

Recommended grammatical / typographical revisions:

Line 95: “quite similar with” Change to “quite similar to”

Line 103: “field measurements exhibit, not only…” Delete comma, change to: “field measurements exhibit not only…”

Line 107: “Especially for the magnetic component…” Delete “Especially”, change to” “For the magnetic component”…

Line 348: “the relativistic electron population and but generally…” Delete “and”, change to: “the relativistic electron population, but generally…”

Line 361: “dependence of these calculated DLL to several solar wind…” Replace “to” with “on”, change to: “dependence of these calculated DLL on several solar wind…”

Our response: We thank the reviewer for noticing these errors, which we have now corrected.