Reviewer Evaluations:

The major comments made during the previous rounds of reviews remain valid. Namely, the method chosen for data analysis is not supported by the physics of radiation belt radial diffusion. Thus, the data products presented are not convincing estimates of radial diffusion magnitude.

1. As previously stated: That the database presents significant variations with MLT is enough to demonstrate that the data products are not radial diffusion coefficients. It also shows that the objective of providing "event-specific" or "time-series" of radial diffusion coefficients using solely in-situ spacecraft measurements is unrealistic. Yet, this is what the manuscript proposes. The suggestions to remove the time series of DLLs and/or to rename the time series using a word other than "DLL" have been dismissed. I can't think of any other suggestion at this point.

Discussing DLL as a function of MLT is inconsistent with the definition of a radial diffusion coefficient. In this context, I maintain that Figure 4 should be removed, together with any discussion of DLL variations with MLT. Yet, this has not been done.

Our response: We believe that there has been a misunderstanding here and we would like to further clarify this. The values shown in figure 4 (now figure 2 in the revised manuscript) do not correspond to the **final** hourly database values. The values used in this figure are the 1-min resolution proxy of the DLL at each point of the spacecraft orbit, for each spacecraft separately. Since this DLL proxy, at each L* value, has been calculated as the product of the weighted averaged power with a simple multiplication factor it is expected to reflect directly the azimuthal distribution of wave power for both the magnetic and the electric component. We are sorry if this was not fully clear previously and in order to explicitly state that in the manuscript we have included a separate section (section 3) where we use these DLL proxies (explicitly stated as proxies and not DLL) in order to discuss the possible uncertainties introduced by the limited MLT coverage of THEMIS and any other attempt to calculate DLL time-series using in-situ measurements.

However, as stated in the manuscript, our hourly DLL calculations are derived using the simultaneous measurements from all three THEMIS spacecraft. Depending on the evolution of the azimuthal positions of the spacecraft, within the hourly time-bin we use, this results in an MLT coverage of up to ~6 hours, for each L* value. As such, there can be no MLT value associated with our hourly DLL, which are the final database products, as THEMIS spacecraft cover a wide range of azimuthal positions over one hour. **Therefore, even though our DLL calculations have uncertainties generated by the use of in-situ data, they do not violate – in any way – the physical definition of radial diffusion.** On the contrary, we have taken measures in order to minimize other significant uncertainties such as the use of the weighted averaged power, which minimizes the significant errors introduced by neglecting higher m (azimuthal wave mode) values.

We again emphasize that our DLL calculations include averaging from the three spacecraft which cover many different azimuthal positions over one hour, both individually and as a constellation. Therefore, the assumption that this partial azimuthal coverage can account for the entire drift of the electrons is exactly that, an assumption, and in no way a violation of the physical definition of the DLL. As we have thoroughly discussed in the previous responses these limitations are present in several recent works that use DLL time-series, e.g. Jaynes et al. (2018); Olifer et al. (2019); Sandhu et al. (2021). Another important example regarding such inherent limitations and necessary assumptions is the well-established Ozeke et al. (2014) semi-empirical model. It is notable that in the Ozeke model the electric component of the DLL is inferred based on measurements from ground-based magnetometers and there the authors have used electric field estimations **only from dayside measurements**. One could argue that this is not only an obviously partial azimuthal coverage, but also that it introduces a very consistent and potentially important bias in the estimation of the DLL. However, despite such limitations the Ozeke model is well-established and widely used because it is one of the more well-performing semi-empirical models.

The aforementioned discussion, combined with our several arguments in the previous responses, oblige us to retain both the term radial diffusion coefficient and its symbol.

2. Going back to comments relative to the data processing: The manuscript still does not quantify the error accompanying the radial diffusion estimate. Yet, in addition to (*) the use of Fei's formula and (*) the lack of knowledge for the repartition of the field variations along the drift shell at any given time, (*) other assumptions are also adding error, in particular, when it comes to extracting "fast" field variations from "slow" and "spatial" field variations. Specifically:

- The reply considers that oscillations with frequencies higher than 0.8 mHz "are the ones responsible for the breaking of the third adiabatic invariant of relativistic electrons". Yet, this omits the fact that the frequency threshold should depend on the population energy. Indeed, the time variations of interest are the ones "faster than the trapped particles' drift period", and the drift period is energy dependent. In this context, I would also recommend detailing why the PSD of the waves considered is in the "2-25 mHz" in the manuscript (l.99).

Our response: We have included in lines 98-100 of the revised manuscript that the 2-25 mHz range correspond to near-equatorial mirroring electrons roughly in the 0.4 – 13 MeV energy range.

- In addition, it is claimed that spatial field variations are usually slow variations, and thus, are filtered out by the 20-min moving average. Yet, this is not proven. The illustration provided discusses ~50 min (?) of LEO B field data (from Swarm) to advocate for the lack of importance of spatial averaging. Yet, this does not prove that a 20-min temporal averaging is similar to spatial averaging for the fields measured along THEMIS orbits (which appears to be the assumption made in the manuscript). If the proof can be made for THEMIS, I would recommend adding the proof to the manuscript to justify this aspect of the approach.

Our response: The CHAOS model is developed for the Swarm data and, therefore, any attempt to apply it to other instruments would require inter-calibration of the measurements, something that is well beyond the scope of this work (even though we intend to enrich our database in the future with other missions as well). As Swarm spacecraft are in LEO, we showcase here that as an approach, qualitatively the application of a high pass-filter is adequate to remove the variations due to the satellite flying through an inhomogeneous magnetic field, even in LEO, where **the magnetic field gradients are extremely steep.** Therefore, we safely argue that if it is adequate for LEO, then it is more than adequate for data at much higher altitudes (such as those from THEMIS used here), where the magnetic gradients are significantly less steep.

At very low L values, steep gradients are removed "manually" (l.108). Yet, it is not explained how.

Our response: After the application of the filtering, we perform a visual inspection of the time series and the steep gradients that have not been removed by 20-min averaging are removed by hand.

The work also lacks a discussion for the conditions of validity for the electric and magnetic field measurements for THEMIS (e.g., for the E field: Califf et al., 2015, doi:10.1002/2014JA020360).

Our response: The uncertainties by the use of in-situ instruments have been included in line 150 of the revised manuscript.

Response to Editor:

The thinking behind this recommendation is that in this way you can retain your results (diffusion coefficient dependencies, database etc.), while in the same time provide some caution to the readers/users of your database on what kind of (systematic) errors may be associated with your coefficient estimation assumptions. The reviewer has some additional recommendations for discussing error/uncertainty estimates which may be of help. In addition, that may prevent from having a work, which reviewers (also ref. #2 of the original report) find interesting and original, rejected partly on the basis of terminology and difficult to avoid assumptions.

Dear Editor,

First of all we want to thank you for all the effort you have put in this manuscript. We further want to clarify a certain issue that may have been the source of a serious misunderstanding.

We have never treated the DLL as "local". We have explicitly stated in section 2.1 that the DLL is calculated as hourly average of the three THEMIS spacecraft, which provides us with an MLT coverage up to ~6 hours per hour and per L* (line 150 of the revised manuscript). Therefore, there is no MLT dependence of the DLL included in the database and which is shown in our results. On the other hand, in figure 4 only (now figure 2 in the revised manuscript), we do not show the same DLL, but rather a "local DLL" which reflects exactly the azimuthal distribution of power. We understand that this was not explicitly clarified in the previous responses and, to that end, we have included a new section (section 3 in the revised manuscript) where we use these local proxies in order to discuss the uncertainties introduced by the limited MLT coverage of in-situ data. We emphasize that this "local DLL" is explicitly referred as "DLL proxy" throughout the entire section.

Therefore, we feel appropriate and correct to retain both the symbol and the term DLL in our manuscript.