Response to the Third Referee.

First, we would like to thank the Referee for her/his comments, all of which we have attempted to address. We think that the paper has been improved by them. Now we detail our response to each comment.

1. **The similarity index of 28% could be acceptable for a review provided that all credits are given, even if the authors of the previous published papers are also on the authors list of the review. But 26% (including Figures 1 and 2) are simply copied from Dominguez et al. (2014).**

   We have made several modifications in various parts of the manuscript in order to deal with this issue, including dropping parts of the text that were not relevant for the line of the discussion intended in this paper.

2. **Obviously, as mentioned in the introduction fractal dimensions have already often been calculated for space and laboratory magnetized plasmas in nature, including the magnetosphere (e.g., J. Geophys. Res. 96, 16031, 1991) and the solar wind (e.g., J. Geophys. Res. 114, A03108, 2009; Astrophys. J. Lett., 793:L30, 2014). But the subject of the submitted review is rather limited to very selected examples of space plasmas, basically only to geomagnetic activity (besides preliminary results applied to magnetic clouds and additional discussion in the context of the turbulence shell model) and therefore the title of the review should possibly be much more specific.**

   We have changed the title to "Evolution of fractality in space plasmas of interest to geomagnetic activity", in order to be more specific and consistent with the content of the manuscript.

3. **By the way, the phenomenological MHD shell model describes the energy cascade in turbulence that sometimes exhibits fractal characteristics, but geomagnetic storms have quite different more intermittent characters, sometimes related to multifractality. It would be nice to provide convincing physical**

arguments justifying application of this model to dynamics of geomagnetic activity.

Maybe we should stress that we are not attempting to use the MHD shell model to account for *Dst* dynamics. Our interest in the connection between two model arises from the possibility of having similar intermittent behaviors, as the shell model can also yield simulations which do not exhibit intermittency levels which resemble the *Dst* time series.

We have added a text in the first paragraph of page 8, related to this issue.

The new text reads:

We first notice that, in general, setting parameters ν and η with arbitrary values yields $\epsilon_b(t)$ series which do not have the necessary intermittency level to resemble the Dst time series. Compare, for instance, the different panels in Fig. 16 in Domínguez et al. (2017), which shows that Pm $= 0.2$ *leads to a very noisy output, unlike simulations with Pm* $= 1.0$ *or 2.0, where individual, large peaks can be easily identified from the background. In fact, previous studies have shown that the statistics of bursts follows a power law for Pm* $= 1$ *(Boffetta et al., 1999; Lepreti et al., 2004; Carbone et al., 2002), and for this reason we start by taking* $Pm = \nu/\eta = 1$.

We have also been careful in the use of words, refering to dissipative events in the shell model as "active" states, whereas in the *Dst* time series they correspond to "storm" states, with definite physical meaning.

The possible connection between geomagnetic activity and the GOY shell model has been suggested in Lepreti et al. (2004), but testing this goes beyond the simple fractal analysis we propose in this manuscript.

4. **page 3, lines 16-18: Admittedly, there is no commonly accepted definition of a fractal (for example, according to B. B. Mandelbrot, 1977: "a fractal is by definition as set for which the Hausdorff Besicovitch dimension strictly exceeds the topo-**

logical dimension"). But certainly, "noninteger numbers measuring the complexity" is rather unclear (maybe roughness, irregularity) and certainly not general (e.g., for the trail fractal Brownian motion its fractal dimension is integer, equal to 2, but greater than 1, the topological dimension).

We agree with the Referee in that one has to be careful with definitions. However, we should notice that the cited sentence in our paper refers to the problem of defining fractal *dimensions*, rather than fractal objects. So, for a given fractal object, there are several ways to define its dimension, and this is what we intended to stress. We have modified the sentence to be more clear.

The new text reads:

In general it can be said that they are numbers, which can be noninteger, measuring the complexity of a data set.

5. **Section 2: The methods of nonlinear time series are well-known, see e.g. the textbook of H. Kantz and T. Schreiber published by Cambridge University Press in 1997. Besides the box-counting (zero-order, capacity) dimension one can also define the (higherorder) generalized dimensions (related to a multifractal spectrum), which are (e.g., the correlation dimension) much more suitable for nonlinear dynamical systems as is in the case of the magnetosphere. Therefore, I would like to ask why the authors use only the box-counting method, which is certainly not very reliable?**

   Our aim was specifically to investigate whether a single fractal dimension may yield useful information on the systems studied, and in what sense. Certainly, given the complexity of the system, there is no guarantee that this is possible at all, but we have found some positive results as described in the manuscript, which we think are interesting. Other choices for that single fractal dimension could have been made. However, rather than changing the type of dimension used, we think it is more interesting to perform a multifractal analysis, in accordance with the nature of the systems studied, and this is currently in process.

This is mentioned in the final paragraph of Sec. 7.

The new text reads:

Given the rich and complex dynamics governing the evolution of magnetized plasmas, we would not expect that a single index would be able to capture all their relevant information. In fact, multifractal analysis should be made in order to represent the dynamics of the systems studied more accurately, and such an analysis is currently being prepared for future publication.

6. **Further, for estimation of any fractal dimension one would require at least approximate stationarity. Hence, my main question is how do the authors cope with non-stationarity of the data under their study, especially during storms. I think that in the magnetospheric studies it would be more difficult task than in the case of the solar wind plasma. Maybe also some filtering is needed before estimating the actual dimension of the fractal structure (see, e.g.: Phys. Rev. E 47, 2401, 1993; Physica D 122, 254, 1998).**

   It is not clear that, for the kind of analysis we are interested, stationarity is a requisite to get meaningful results. For instance, the magnetic cloud analysis clearly involves a process where various degress of stationarity are found. Without looking at the fractal dimension, one could argue that the flux rope stage satisfies the stationarity criterion, the sheath does not, and the solar wind stages could also be approximately stationary. And yet, calculation of the fractal dimension on each state, regardless of its level of stationarity, yields useful results, being able to distinguish the various stages.

   This is because the fractal dimension that we calculate is related to the intermittency level of the time series, which is also why storms leave a signature in the dimension, a signature which could be lost with filtering, as suggested by Fig. 8 in our paper. The issue of the need for stationarity in the *Dst* or shell model time series should be studied more systematically in order to give a definitive answer.

7. **Results and Conclusions: Relation of the fractal dimensions to storms should be better justified. Namely, a decrease of the fractal dimension based on Dst index presented in Figures 8 and 9 during storms may simply artificially result from lack of stationarity. Anyway, a more comprehensive nonlinear time series analysis is needed before drawing any robust conclusion (e.g., page 13, line 8ff).**

   We have attemted to tone down the conclusion in this respect. Figure 9 of the previous manuscript has been dropped from the current version of the manuscript, since it was not relevant to the main discussion. Regarding Fig. 8 in the previous version (Fig. 5 in the current one), it is true that the decrease in the fractal dimension previous to the storm could be due to pre-storm intermittency unrelated to the upcoming storm. However, our aim in this manuscript is focused rather on the dissipative events themselves and the fractal dimension, not on the finding of precursors for geomagnetic activity, an issue which requires further, detailed analysis.

   Thus, we have changed the wording in the sentence mentioned (now at the bottom of page 5).

   The new text reads:

   As shown in Domínguez et al. (2014), the box-counting dimension of the Dst index decreases as the storm approaches for all cases studied. Moreover, this decrease occurs before the window includes the geomagnetic storm, as marked by the vertical lines in Fig. 5. Whether this is relevant for forecasting geomagnetic storm needs further study, as it may simply be due to an increase of the intermittency in the time series, unrelated to the upcoming dissipative event.

   Also, the "Some robust behaviors are identified" sentence in the conclusions has been dropped, in order to moderate the conclusions.