Interactive comment on the paper by Victor **Munoz** et al. (2017)
Evolution of fractality in magnetized plasmas
submitted to a special issue of NPG on *Nonlinear Waves and Chaos* 2017

In my view, the submitted manuscript is interesting and possibly worth publishing in *Nonlinear Processes in Geophysics,* but after some revisions, and with more specific title and somewhat weaker conclusions. 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).

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.

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.

Please find also my specific comments:

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 topological 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).

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 (higher-order) 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?

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).

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).