

Report on Ms. angeo-2018-7 (Johnson et al.)

“Transfer entropy and cumulant based cost as measures of nonlinear causal relationships in space plasmas: applications to Dst”

In this work the Authors shows an application of some statistical and information theory-based methods to the study of the Earth’s magnetosphere response to solar wind changes with the aim to demonstrate that these methods and tools can be useful to study the nonlinear dynamics of the Earth’s magnetosphere. In particular, the Authors applied two quite novel methods, the transfer entropy analysis and the cumulant based cost, to the investigation of the causal relationship between VBs, Vsw and Dst, showing how some of the information contained in the dynamical evolution of Dst are not directly to solar wind driving. The manuscript is quite well written. However, although I believe that the topic treated in this work is timely appropriate and the techniques presented could be of interest of a wide community as the one of *Annales Geophysicae*, I think that there are some aspect and points of this work that need to be revised before considering it ready for publication.

Thus, I recommend to send back the manuscript to the Authors for a major revision according to the points listed below.

Major Questions.

1) In the overall paper (Introduction, Linear vs Nonlinear Dependency, etc.) the Authors miss to cite several previous topical works dealing with the nonlinear and complex dynamics of the Earth’s magnetosphere (e.g. Tsurutani, B., et al., GRL, 1990; Vassiliadis, et al., GRL, 1990. Klimas, et al., JGR, 1996; etc.). The same is for what regards previous application of information theory methods to space plasma physics and the Earth’s magnetosphere. I strongly invite the Authors to revise their introduction and manuscript considering more extensively the previous literature.

2) I would like to understand why the Authors in making their analysis do not consider instead of Dst its high-resolution version, Sym-H. Indeed, in disentangling the internal magnetospheric dynamics with respect to the external driven one the use of Dst index could be not sufficient, because all the fast internal processes are not contained in this index. I would like to stress that the internal magnetospheric dynamics is generally related to processes taking place in the tail regions which are characterised by timescales shorter than 60-90 minutes. Thus, Dst cannot be able to provide a reasonable information on it. Please, comment your choice and justify it.

3) In section 3.1, Cumulant based analysis, the Authors state that each of the considered variables is Gaussianized. I do not understand this statement. The PDFs of Dst and also external drivers is generally not Gaussian. What do they mean with this statement ? I guess that probably they refer to the fact that time series are normalized to unit variance. Please explain better this statement.

4) If I have correctly understood the cumulant based method, the nonlinear cross-correlation quantity should provide an information of the overall (linear and nonlinear) correlation between VBs and Dst. Thus, how can the Authors state that peaks at 25, 50 and 90 hours are of an internal origin on the basis that they are not present in the auto-correlation of external drivers ? Furthermore, in doing their analysis the Authors have considered Dst records covering 27 years (1974-2001) without discriminating between single geomagnetic storms and multiple geomagnetic storms. So how they can assert that these secondary peaks (which is less prominent) do not come from such multiple geomagnetic storms but reflects internal processes ? This conclusion seems to me not convincing. To convince the reader that there are secondary peaks in the nonlinear cross-correlation that are of an internal origin, the Authors should make the analysis on a subset of geomagnetic storms which are characterised by only a single negative-peak in Dst.

5) Page 11. To my knowledge there should be also other processes/mechanisms than ion cyclotron waves -particle scattering that could be responsible for ring-current decay. For instance, I remember that also ENA loss mechanisms could contribute to the decay of the ring current. Perhaps, this could be considered in discussing this point.

6) In the Transfer Entropy analysis section few details are given about the way Transfer Entropy and Mutual Information are computed. To my knowledge binning procedure and PDF computing method are critical issues in evaluating these quantities. I believe that more information should be provided to make the reader able to reproduce the results.

7) The result on the time delay (8-11 hr) between the information transfer from Vsw and Dst looks very long. The Earth's magnetosphere is expected to respond to solar wind changes on shorter timescale and this is also the case of ring-current. This is also corroborated by the capability of several Artificial Neural Network models of the Earth's magnetospheric response that consider a time delay of 1-2 hours as input variables for predicting Dst (see e.g. Wu and Lundstedt, JGR,

1997; Lundstedt et al., GRL, 2002; Pallocchia et al., Ann. Geophys., 2006). The Authors should motivate this result with more physical considerations.

8) Figure 1 is hardly readable. I suggest to expand the X-axis or to include a inset where the first part of X-axis is expanded.

Minor points.

Some references are missing (there are some question marks at page 8.