

Interactive comment on “Magnetospheric chaos and dynamical complexity response during storm time disturbance” by Irewola Aaron Oludehinwa et al.

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Reviewer comments 2 Comment 1: Manuscript line 7: in correct “to” Response: The above comment has been corrected in the revised manuscript in Line: 7 as: “to”

Comment 2: Manuscript Line 14: response correct to “responses” Response: In the revised manuscript the above comment has been corrected in Line: 14

Comment 3: Manuscript Line 15: “decline” Response: the above comment has been corrected to “decline” in the revised manuscript

Comment 4: Manuscript Line 16: (I would prefer "obtained from VBs") Response: In

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Manuscript Line 16-17 of the revised manuscript, the statement has been corrected as “obtained from VBs”

Comment 5: Manuscript Line 17: “associated (the correct use is "are associated with...")” Response: The above statement has been corrected to “are associated with” in Line 17 of the revised manuscript

Comment 6: Manuscript Line 31: “I would recommend to replace "that" with "for which” Response: In the revised manuscript the above statement has been corrected as “for which” in Line 17.

Comment 7: Manuscript Line 36: Might a I suggest a rephrasing? As it is now it reads as "the dynamics display signatures of fluctuations in its dynamics" which sounds a bit weird. I would recommend something like "The Dst fluctuations exhibit different signatures for different categories of geomagnetic storms" Response: Thank you for your contribution towards the improvement of the manuscript. The above comment has rephrase as suggested in Line 40 of the revised manuscript as “The Dst fluctuations exhibit different signatures for different categories of geomagnetic storm.”

Comment 8: Manuscript Line 42: It’s has been corrected to “It has” Response: The above comment has been corrected in the revised manuscript in Line 46 of the revised manuscript.

Comment 9: Manuscript Line 43: “driven” correct to “driver” Response: In the revised manuscript line 47, the above comment has been corrected.

Comment 10: Manuscript Line 52: the name is "Vassiliadis" not Vassilidia! Please, correct it here and in the other instances Response: In Line 56 of the revised manuscript. The name has been corrected to “Vassiliadis”

Comment 11: Manuscript Line 55: the name is "Vassiliadis" not Vassilidia! Please, correct it here and in the other instances Response: In Line 59 of the revised manuscript. The name has been corrected to “Vassiliadis”

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Comment 12: Manuscript Line 76: presented (I think its a more suitable term than "extends") Response: The above comment has been corrected in the revised manuscript Line 80 as: "presented"

Comment 13: Manuscript Line 78: asserted (you are using past tense in the beginning of the sentence, so keep using it here as well) Response: Thanks for your contribution towards the improvement of the manuscript. The comment has been corrected to "asserted" in Line 82 of the revised manuscript.

Comment 14: Manuscript Line 88: move the parenthesis after the definition of Maximal Lyapunov Exponent Response: In line 92 of the revised manuscript, the above comment has been corrected as "Maximal Lyapunov Exponent (MLE),"

Comment 15: Manuscript Line 88: "have" correct as "has" Response: In Line 92 of the revised manuscript, the statement has been corrected

Comment 16: Manuscript Line 88: depict (the correct sentence is "has the potential to depict") Response: In the revised manuscript line 92 the comment have been corrected as: "has the potential to depict"

Comment 17: Manuscript Line 97: phenomena (this is the correct plural form of the term "phenomenon") Response: The above comment has been corrected in (line 101) of the revised manuscript.

Comment 18: Manuscript Line 103: "a record of" correct as "derived by measurements from" Response: In line 107 of the revised manuscript, the above comment has been corrected.

Comment 19: Manuscript Line 104-105: I am not an authority on Dst, but I would write this sentence as "depicts mainly the variation of the Ring Current, as well as magnetopause and tail currents to a lesser extent". In my mind, 90% of Dst variation is due to Ring Current, but I might be wrong Response: The comment has been corrected in the revised manuscript in line 108-109 as: depicts mainly the variation of the ring

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current, as well as the Chapman-Ferraro Magnetopause currents, and tail currents to a lesser extent

Comment 20: Manuscript Line 111: Is this the absolute value? Dst can be positive. Are positive values ignored? Response: No, the positive are not ignored, each month is being classified based on its minimum Dst value. In the revised manuscript line 117-118 we have included the comment "and each month is being classified based on its minimum Dst value."

Comment 21: Manuscript Line 116: signals correct as "signal (singular)" Response: the above comment has been corrected in the revised manuscript line 121.

Comment 22: Manuscript Line 171: Consider rephrasing: "If the ratio of the difference of the components that correspond to the m+1 dimension divided by the distance of the vectors at the previous m dimensions" or something similar Response: In the revised manuscript the above comment has been corrected in more details in line 166-185 as: For instance, suppose we have a one-dimensional time series. We can construct a time series $y(t)$ of D-dimensional points from the original one-dimensional time series $x(t)$ as follows: $y(t)=(x(t),x(t+\tau),\dots,x(t+(D-1)\tau))$ (3) Where τ and D are time delay and embedding dimension. Using the formular from Kennel et al. (1992); Wallot and Monster, (2018). If we have a D-dimensional phase space and denote the rth nearest neighbour of a coordinate vector $y(t)$ by $\hat{y}((r)) (t)$, then the square of the Euclidean distance between $y(t)$ and the rth nearest neighbor is: $R_D^2(t,r)=\sum_{k=0}^{D-1}[x(t+k\tau)-\hat{x}((r)) (t+k\tau)]^2$ (4) Now applying the logic outlined above, we can go from a D-dimensional phase space to (D+1) dimensional phase space by time-delay embedding, adding a new coordinate to $y(t)$, and ask what is the squared distance between $y(t)$ and the same rth nearest neighbour: $R_{(D+1)}^2(t,r)=R_D^2(t,r)+[x(t+D\tau)-\hat{x}((r)) (t+D\tau)]^2$ (5) As explained above, if the one-dimensional time series is already properly embedded in D dimensions, then the distance R between $y(t)$ and the rth nearest neighbour should not appreciably change by some distance criterion R_{tol} (i.e $R < R_{tol}$). Moreover, the distance of the nearest neighbour when embedded into the next higher di-

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mention relative to the size of the attractor should be less than some criterion A_{tol} (i.e. $R_{(D+1)} < A_{tol}$). Doing this for the nearest neighbour of each coordinate will result on many false nearest neighbours when embedding is insufficient or in few (or no) false neighbours when embedding is sufficient.

Comment 23: Manuscript Line 174: define properly what is meant by that, e.g. "the percentage of false nearest neighbors" Response: The above comment has been corrected in Manuscript line: 187

Comment 24: Manuscript Line 186: Algorithm correct to "The algorithm" Response: The above comment has been corrected in the revised manuscript line 199-200.

Comment 25: Manuscript Line 197: "later time t_1 " please specify what this means in your application of the method Response: The time t_1 refers to another point. In the revise manuscript, the above comment have been corrected in line 210 as: "At a later point t_1 "

Comment 26: Manuscript Line 200: In your description of the method there is no mention of any replacement, so it is not clear what is meant by this sentence. I assume you mean the same method as in the Wolf 1986 paper, so please add the full paragraph "We look for a new data point that satisfies two criteria reasonably well: its separation, $L(t_1)$, from the evolved fiducial point is small, and the angular separation between the evolved and replacement elements is small. If an adequate replacement point cannot be found, we retain the points that were being used." Response: the above comment has been included in the revised manuscript line 213-216 as: M is the total number of replacement steps. We look for a new data point that satisfies two criteria reasonably well: its separation, $L(t_1)$, from the evolved fiducial point is small. If an adequate replacement point cannot be found, we retain the points that were being used. This procedure is repeated until the fiducial trajectory has traversed the entire data Comment 27: Manuscript Line 207: "have" correct as "has" Response: The above comment has been corrected in the revised manuscript in line 223.

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Comment 28: Manuscript Line 258: " $\tau=15\text{hrs}$ " correct as " at $\tau=15\text{hrs}$ " Response: In the revised manuscript, the above comment have been corrected as " at $\tau=15\text{hrs}$ " in line 273

Comment 29: Manuscript Line 259: define more properly what this means, e.g. "percentage of false nearest neighbors" Response: In the revise manuscript, the above comment has been corrected line 274-275 of the revised manuscript

Comment 30: Manuscript Line 269: "for most of the months categorized as minor geomagnetic storm" this is repeated exactly the same in the previous sentence. Consider removing it Response: the comment has been removed in the revised manuscript.

Comment 31: Manuscript Line 308: I would prefer to use the term "higher" or "larger" instead of "prevalent" Response: The above comment has been corrected as "higher" in the revised manuscript in line 324.

Comment 32: Manuscript Line 313-314: This is more or less the same as the previous sentence, but does not explain why the authors state this assumption. Response: The above comment has been corrected and explained further in the revised manuscript line 331-338 as: This increase in chaotic behaviour for Dst signals during minor geomagnetic storm may be as a result of asymmetry features in the longitudinal distribution of solar source region for the Corotating Interaction Regions (CIR) signatures responsible for the development of geomagnetic storms (Turner et al. 2006; Kozyra et al. 2006). CIR generated magnetic storms are generally weaker than ICME/MC generated storms (Richardson and Cane, 2011). Therefore, we suspect that the increase in chaotic behaviour during minor geomagnetic storm is strongly associated with the asymmetry features in the longitudinal distribution of solar source region for the Corotating Interaction Regions (CIR) signatures.

Comment 33: Manuscript Line 331: "impending" implies prediction. I am not sure I see any predictive potential from the current study. Perhaps it would be better to replace this with "...information about monitoring space weather..." Response: In the

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revised manuscript, the word “impending” has been corrected as “information about monitoring” in line 356.

Comment 34: Manuscript Line 347: extension to the single-year investigation Response: In the revised manuscript line 372-373, the above have been included

Comment 35: Manuscript Line 364: “cause” correct to “caused” Response: The comment has been corrected in the revised manuscript in line 389.

Comment 36: Section 2: Why 9 years? Why not include a couple more and cover the entirety of solar cycle 24? I will not insist on repeating the analysis for the full 11 year period, but I think it would be useful to address the reason since it will probably be one of the first questions that readers will have. Response: Thank you for contribution towards the manuscript. This study considers the period of nine year (2008-2016) because between this period we can successfully categorized/access the period of minor, moderate and major geomagnetic storm. In addition, the year (2008-2016) comprises of solar minimum of solar cycle 24 (2008-2009), Ascending phase (2010-2011), solar maximum (2012-2014), and declining phase of solar cycle 24. We are grateful for this useful suggestion which will be another insight in our future work. we are going to consider the chaotic and dynamical complexity in the Dst and Δ_{VB} time series during solar minimum, ascending, solar maximum and declining phase of solar cycle 24 in our next paper.

Comment 37: What is the sampling rate of the data series? I assume that for the Dst the sampling time used was 1-hour, but it should be mentioned in the paper as well, perhaps somewhere in this paragraph. The same also for the VBs series. Response: The sampling rate of the data time series is 1-hour. We have included it in the revised manuscript line 114 as “The sampling time of Dst and VBs time series data was 1-hour.”

Comment 38: You include the categorization by Dst, but from the Results section I realized that the analysis is being performed separately for each month in the data, and that each month is classified as Minor, Moderate or Major, but I don't think I saw anywhere

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the description of how this is done. I assume that each month is being classified based on its minimum Dst value, but it should be written explicitly somewhere in this section. Also, perhaps it would be better to change the definitions from “minor, moderate and major geomagnetic storm” to “month of minor, moderate and major geomagnetic storm activity” in the entirety of the paper.

Response: In the revised manuscript line 117-118 we have included the suggested comment as: “and each month is being classified based on its minimum Dst value.” In addition we have changed the definitions from “minor, moderate and major geomagnetic storm” to “month of minor, moderate and major geomagnetic storm activity”. For instance, the Figures have been corrected from “minor, moderate and major geomagnetic storm” to “month of minor, moderate and major geomagnetic storm activity”.

Comment 39: Section 2.2 Was there a binning used for the AMI calculation? How many bins? Response: Yes. The number bin used in the analysis is 3. Comment 40: Section 2.3 I think that in the original method by Kennel, the use only one neighbor for each point, namely the nearest neighbor. In this, I read: “the algorithm search for neighbour $P(j)$ such that, $|P(i) - P(j)| < \epsilon$ ”, where ϵ is a small constant”, which implies that there might be more than one nearest neighbors. Have I understood that correctly or is it just miswritten? Additionally, I think the sentence “If the distance of the iteration to the nearest neighbor ratio exceeds a defined threshold (ϵ),” should be re-written to be more clear what is meant. Also, the definition of the final quantity is not very clear. I assume that the final metric is the percentage of nearest neighbors, but I cannot be certain if you are using something else. Please clarify this in the text. Response: In the revised manuscript the above comments have been corrected in more details in line 166-185 as: For instance, suppose we have a one-dimensional time series. We can construct a time series $y(t)$ of D-dimensional points from the original one-dimensional time series $x(t)$ as follows: $y(t)=(x(t),x(t+\tau),\dots,x(t+(D-1)\tau))$ (3) Where τ and D are time delay and embedding dimension. Using the formula from Kennel et al. (1992); Wallot and Monster, (2018). If we have a D-dimensional phase

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space and denote the r th nearest neighbour of a coordinate vector $y(t)$ by $\hat{y}^{(r)}(t)$, then the square of the Euclidean distance between $y(t)$ and the r th nearest neighbour is: $R_D^2(t,r) = \sum_{k=0}^{D-1} [x(t+k\tau) - \hat{x}^{(r)}(t+k\tau)]^2$ (4) Now applying the logic outlined above, we can go from a D -dimensional phase space to $(D+1)$ dimensional phase space by time-delay embedding, adding a new coordinate to $y(t)$, and ask what is the squared distance between $y(t)$ and the same r th nearest neighbour: $R_{(D+1)}^2(t,r) = R_D^2(t,r) + [x(t+D\tau) - \hat{x}^{(r)}(t+D\tau)]^2$ (5) As explained above, if the one-dimensional time series is already properly embedded in D dimensions, then the distance R between $y(t)$ and the r th nearest neighbour should not appreciably change by some distance criterion R_{tol} (i.e. $R < R_{tol}$). Moreover, the distance of the nearest neighbour when embedded into the next higher dimension relative to the size of the attractor should be less than some criterion A_{tol} (i.e. $R_{(D+1)} < A_{tol}$). Doing this for the nearest neighbour of each coordinate will result on many false nearest neighbours when embedding is insufficient or in few (or no) false neighbours when embedding is sufficient.

Comment 41: Section 2.4 What time 't1' did you use when applying the method? In your description of the method there is no mention of any replacement, so it is not clear what is meant by the sentence "M is the total number of replacement steps". I assume you imply the same as the method described in the Wolf 1986 paper, so please add the full paragraph "We look for a new data point that satisfies two criteria reasonably well: its separation, $L(t_1)$, from the evolved fiducial point is small, and the angular separation between the evolved and replacement elements is small. If an adequate replacement point cannot be found, we retain the points that were being used." Response: The time t_1 refers to another point. In the revised manuscript, the above comment has been corrected in line 210 as: "At a later point t_1 ". Moreover, we have included correction in the revised manuscript line 213-216 as: M is the total number of replacement steps. We look for a new data point that satisfies two criteria reasonably well: its separation, $L(t_1)$, from the evolved fiducial point is small. If an adequate replacement point cannot be found, we retain the points that were being used. This procedure is repeated until

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the fiducial trajectory has traversed the entire data.

Comment 42: Section 3 Sometimes figures are referenced as "figure (5)" and other times as "Figure 5" and I believe I saw one instance of "figure 5" (with lower case 'f'). I would prefer if the authors chose one style and maintain it for the whole paper. Personal preference: the second. Response: Thank you for your contribution towards the improvement of the manuscript. We made the corrections to it as "Figure" in the revised manuscript.

Comment 43: Did you apply the DVV method on the vBs data series? It might be useful to show one example to showcase how a stochastic signal looks, or at the very least mention in the text that the results obtained look similar as the ones for the Dst for months of minor activity. (that is assuming that they did indeed prove the stochastic nature of solar wind, as the other measures did) Response: In the revised manuscript we have include the DVV analysis of the VBs during the month of minor, moderate and major geomagnetic storm activity and it is shown in Figures (10-12).

Comment 44: Section 4.1 I do not understand why the longitudinal asymmetry of CMEs would imply an increase in chaotic behaviour. Please elaborate more on this. Response: In the revised manuscript line 331-338 we have made the correction and elaborate more in details as "This increase in chaotic behaviour for D_{st} signals during minor geomagnetic storm may be as a result of asymmetry features in the longitudinal distribution of solar source region for the Corotating Interaction Regions (CIR) signatures responsible for the development of geomagnetic storms (Turner et al. 2006; Kozyra et al. 2006). CIR generated magnetic storms are generally weaker than ICME/MC generated storms (Richardson and Cane, 2011). Therefore, we suspect that the increase in chaotic behaviour during minor geomagnetic storm is strongly associated with the asymmetry features in the longitudinal distribution of solar source region for the Corotating Interaction Regions (CIR) signatures."

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