

Reply to Report #1

Comment:

The authors have revised their manuscript. I am still concerned on the suitability of the paper for the journal. The journal aims and scope state that it "solicits disruptive and innovative concepts and methodologies, as well as original applications of these to address the ubiquitous complexity in geoscience systems...". At present, I don't see neither a clear application of such methodologies nor the inclusion of innovative concepts. Therefore, please consider to incorporate or at least discuss some advanced analysis or concepts that emphasize nonlinearity, making it more aligned with NPGs standards. This addition wouldn't necessarily require a lot of work.

Dear Dr. Norbert Marwan,

Your last question in the interactive discussion was posted just after we had uploaded our revised manuscript. We are happy to have the opportunity to include our comments in this version. We used this opportunity to correct also some typos and check cleanliness of some sentences. Bellow is our answer to your previous comments:

Although our paper makes it clear that the standard satellite drag computer codes are not able to predict the fast loss of 32 of the Starlink satellites, perhaps including the possible nonlinearity of the atmospheric changes during disturbed geomagnetic periods, and the satellite drag with spacecraft orientation would help. Present models assume satellites with standard cross sections. However, the Starlink satellites are formed by two flat panels (one for the satellite body and another for the solar panels). Just after the launch, the solar panels are folded on top of the satellite panel. During the period for the orbital elevation, the solar panels are unfolded but kept aligned with the satellite body, becoming a long flat panel. In the final position, the satellite assumes an "L" shape. This unusual shape of Starlink satellites, make their cross sections completely different than traditional satellites, and highly dependent of its orientation.

Future computer codes should take these different shapes into account, their orientation, and then calculate a maximum and minimum time for reentry. The codes should also consider satellite tumbling. Would the loss times be within the max and min entry times or would they be even greater?

We include this discussion in the revised version of the manuscript, around lines 318 and 328.

Reply to Report #2

Review for "NORAD Tracking of the 2022 February Starlink Satellites and the Immediate Loss of 32 Satellites" by Guarnieri et al, submitted to NPG

The authors have made some changes, although not all of my original points have been addressed. I still feel that the paper is overall light on citations and detailed explanation. I detail this below. Throughout this report, I use the point numbers from my original report. Thank you for inviting me to review this paper.

Dear Dr. Alexandra Fogg,

We thank you for your constructive comments. We have made additional changes to clarify the points you considered still missing. We also took this opportunity to correct some typos and improve sentence clarity. Below are our responses to your comments.

Major Comments

2. I still feel that this paper is not within the remit of NPG, and note that Anonymous Referee #2 also commented on this. This is up to the editor to decide.

As we mentioned in the answer to Referee #2, the last round in the interactive discussion occurred after we had uploaded the revised version of the manuscript. Now we had the opportunity to include this discussion of future steps around lines 318 to 328.

Minor Comments

1. Although my suggested citation was included in the Discussion section, no more citations were added to the introduction on recent storm work. My view is that the introduction should include both original/seminal citations, as well as more recent, novel work.

We included new citations:

line 110: Fiori et al., 2014; Lühr et al., 2009; Oliveira & Samsonov, 2018; Takeuchi et al., 2002;

lines 113, 114 e 115: Burlaga et al. 1998; Gopalswamy et. al 1998; Lepri and Zurbuchen, 2010; Sharma and Srivastava, 2012; Gopalswamy, 2015, 2022

line 119: Yashiro et al., 2013; Marubashi et al., 2015;

line 123: Wang et al., 2018; Kozyra et al., 2013.

These references were included and highlighted in the references list.

3. The authors have added two citations (Clark+ 2022a and 2022b) to this section. My feeling is there are still many asserted facts without citations. For example lines 65-69 have no citations. If all this information is from Clark+, that's probably ok, but please make this clear.

We adjusted the citations in line 65 (Manley, 2021) and in line 68 (Clark, 2022b and McDowell, 2023);

7. I am happy with the flux rope explanation, but my view is that not all the phenomena have been described clearly enough. For example line 114-115 "Thus, the magnetic storm is caused...". This needs expanding – a negative peak in SYM-H isn't all that's needed for a geomagnetic storm, it needs to be a characteristic shape as well.

We included the following explanation in between the lines 77 and 82:

"For the geomagnetic storm identification, we followed the classical definition by Gonzalez et al., (1994), which uses the Dst index (equivalent to the SYM-H index used here) to identify a storm. When the index falls in the range from -30 nT to -50 nT for more than 1 hour, it characterizes a small (weak) storm. Events with decreases between -50 nT and -100 nT for more than 2 hours are considered a moderate storm. An intense storm present falls to Dst values of less than -100 nT for more than 3 hours. The recovery phase is the period when the index returns to the values observed prior the storm. It may last from hours to several days."

17. I like your explanation in the response to reviewers document, however I can't find the similar explanation in the new text. It's worth just taking your sentences from response to reviewer and putting them into the draft.

We checked again our previous answers to include in the manuscript.

Line 100 to 105;

Line 310 to 312;

Lines 318 to 328.

18. You say in the text on line 273 "We think this is not probable". Please justify this in the text.

In the original publications of PPEF, it was shown these effects occurring uplifting plasma from about 300 km to altitudes as high as 600 km. These altitudes were demonstrated by satellite data (CHAMP, SAC-C and TOPEX). Since the SpaceX satellites never reached such high altitudes, we believe they were not directly affected by PPEF. Anyway, we recommended future simulations to investigate whether a similar effect could be happening in lower altitudes.

The text was changed in between lines 275 and 280.

21. I don't feel the authors have addressed my point here. I would like to see a clear conclusion point, saying what steps forward their analyses have caused. I think it is necessary and within the scope of the paper to say what results they have found in terms of which mechanism their work contributes to.

A new discussion was included since the last revision related to the other referee discussion. It is between the lines 318 e 328.

22. I understand you cannot get the data from the company – this is of course a shame in terms of our community's drive towards space weather preparedness. Perhaps you could add a statement in section 8 to the effect of emphasizing the need for this telemetry data in our community.

Sure. The statement was included between the lines 341 to 349.