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Interactive comment on "Solar-cycle, seasonal, and asymmetric dependencies of thermospheric mass density disturbances due to magnetospheric forcing" *by* Andres Calabia and Shuanggen Jin

Anonymous Referee #1

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General comments:

The authors investigate the relations between thermospheric neutral mass densities and geomagnetic indices. The study is based on the evaluation of so-called residual disturbances of the neutral densities obtained from the GRACE mission. Studying the coupling processes in the Earth's atmosphere is a current topic, especially since in-situ measurements of thermospheric neutral densities are available from satellite accelerometer data. The results of this study may have potential to improve the understanding of the thermosphere-magnetosphere coupling. However, the analysis section is not well-described and difficult to read. In large parts, the methodology refers to

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the authors' previous publications. Without reading the author's previous papers from 2016, it is hard to get an idea of the applied methods.

Specific Comments:

My main concern about the manuscript is the analysis section. In section 2.1, the authors cite three publications of their own on the processing of the GRACE densities. I recommend to briefly summarize the procedure with a focus on differences compared to other papers. One important point is that the densities have been normalized to an altitude of 475km using densities from the NRLMSISE-00 model. The authors mention in their 2016 publication that this leads to errors of up to 5%. Please discuss this issue.

In section 2.2, the principle modes of neutral densities obtained from 13 years from GRACE accelerometry and POD (from the authors' previous 2016 publication) were used. Why do the PCA modes of 92 %, 3.5 %, 3 %, and 1.3 % listed here differ from the values of 90.3%, 3.5%, 2.9%, 1.2%, listed in Calabia and Jin (2016)? Since the GRACE densities are given along the orbit, it would be interesting to mention the data set on which the PCA has been applied. Did you do a sparse PCA or did you expand the GRACE densities to global grids? Moreover, it is not self-explanatory what the authors mean by "the correlation coefficients between the parameterized time series of PCA modes and the originals". How did you parameterize the time series or is it the principle component itself? What are the originals? Why to the authors add the constant value of Am=6 to which kind of parameterization? How did they choose the value? Additionally, a concise explanation of the "radiation model" would be helpful. Which frequencies did you use, what about semi-annual variations and variations with the solar rotation? Then, the readers should be informed about the reasons for the computation of residual disturbances. What is the benefit from using residual disturbances for the analysis of this paper instead of directly using the GRACE-derived densities? What are the expectations from removing the "radiation model" from the GRACE-derive densities? What about errors in the processing of the GRACE-derived densities? Is there an impact of normalizing the GRACE-derived densities to an altitude of 475km? Are the

density disturbance a temporal or a spatial quantity?

In section 2.3, the residual disturbances are computed for different regions (north, equator, south). What is the difference between density residuals and residual disturbance (both are abbreviated with \rho_r in section 2.2)? What is the benefit of choosing these three regions? What is the dimension of the density profiles in time and in space (gridded or along the orbit)? Do equations 4 and 5 fit the residual disturbances or their standard deviation? Why does the variable \rho_r' not occur again in this paper? Since equations 4 and 5 are essential for this paper, I suggest to better motivate and describe this procedure.

Figure 5: The authors' GRACE-derived densities have a resolution of 3 minutes, however, it is possible to compute densities with a resolution of 10seconds. Why do you choose this comparably coarse resolution of 3 minutes? How do you conduct the correlation analysis, i.e., are the indices interpolated to 3 minute intervals? Please discuss the temporal resolution of the indices. Is it reasonable to discuss sub-daily frequencies?

Since many questions arise when reading the analysis section, a lot of effort is required to improve the manuscript. Although the manuscript is well structured, it is difficult to read. I highly recommend to check the language (especially grammar) by a native speaker. Additionally, concise formulations would improve the readability. Typos in citations should not occur (e.g., Lüh → Lühr, Muller → Müller) and abbreviations should be mentioned before using them (e.g., IMF, NPR).

Technical Corrections:

Abstract Line 10 Local-Solar-Time → Local Solar Time Line 10 solar-cycle fluctuations → solar cycle variations. Please write "solar cycle" instead of "solar-cycle" also elsewhere. Line 11 ... and investigated in terms of magnetospheric drivers. Please write this into a full sentence. Line 13 weaker variations: This formulation is not clear to me. Maybe you mean low-frequency variations? Please rewrite. Line 14 Russell-

McPherron (R-M): Abbreviation R-M is not common. Please use RM. Line 19 Dst shows good correlation, while Am and E m are best predictors. It is not clear to me, why Dst shows better correlations than other indices and at the same time it is a worse predictor? Line 18. The reader might not now what Dst, Am and Em stand for. I suggest to rewrite include the meaning of the indices as follows (also for Am and Em): "... in terms of the disturbance storm time index Dst shows ..." Line 18 "Good correlation" is really vague. Adding a number here would be appropriate.

1. Introduction line 26 IMF. Please write out the abbreviation when first mentioning it. Line 27 phenomena. Phenomena is plural. Please change it to phenomenon. Line 37 CME's → CMEs Line 43 equator-ward → towards the equator Line 30-44 Please cite your references! I do not see the point in explaining CIRs and CEMs in detail here, since these phenomena are not relevant for the analysis and conclusions in this study. Line 45 Existing thermospheric modeling → existing thermosphere models Line 46 Low Earth Orbit (LEO) Precise Orbit Determination (POD) → precise orbit determination (POD) of low Earth orbit (LEO) satellites. Lines 46-51. Potential readers might wonder why thermospheric density variations are of great importance for POD. Please add an explanation. Line 52 NRLMSISE00 → NRLMSISE-00 Line 51-54. The authors mention three different models that can provide the thermospheric mass density. Why do you think that these models are the most representative ones? For the sake of completeness, please also mention the DTM-2013 by Sean Bruinsma, which is also a representative model. Bruinsma, S. (2015). The DTM-2013 thermosphere model. Journal of Space Weather and Space Climate, 5, A1. Line 54-56. As far as I understand, the mentioned functions are used to model the vertical profile. Please mention this explicitly. Line 60 Liu et al. [2005] is not the first paper on mass densities from CHAMP accelerations. To my knowledge, Villain (1980) was an early paper on densities derived from the CACTUS accelerometer. Then, with the CHAMP mission, Bruinsma and Biancale (2003) published -as far as I know- the first CHAMP-derived mass densities Please clarify this in your literature review: (a) Villain, J.P., 1980. Traitement des donnYees brutes de l'accelerometre CACTUS. Etude des perturbations de

moyenne e Y chelle de la densite thermospherique. Ann. Geophys. 36, 41–49. (b) Bruinsma, S., Biancale, R., 2003. Total density retrieval with STAR 2003. On board evaluation of the STAR accelerometer. In: Reigber, Ch., Lühr, H., Schwintzer, P. (Eds.), First CHAMP Mission Results for Gravity, Magnetic and Atmospheric Studies, Springer, Berlin, Heidelberg, New York, pp. 193–200. Line 61 Please reformulate using proper English. Line 66 Lüh → Lühr Line 70 Muller → Müller. I highly recommend to look at typos in all citations. Line 71 The authors cite Müller et al. (2009) using the Am instead of the Ap index. I agree that this is an important finding, but the difference between the indices remains unclear. Line 97 & 100 The expression "for a better understanding" occurs in two subsequent sentences. I suggest replacing one of them. 104 I suggest to start a new paragraph here to simplify the readability. Line 105 POD -based → POD-based

2. Data and analysis methods Line 117 near-circular orbit → nearly circular orbit Line 117-118 "The highly sensitive accelerometers on-board the GRACE satellites were originally designed to measure the Earth's gravity field..." This sentence is misleading because the accelerometers were not designed to directly measure the Earth's gravity field. The instruments measure the non-gravitational accelerations! Line 172 furrier fitting? Most likely the authors mean a Fourier transform here. line 210, Figure 1. Please put the legend next to the plot to make it fully visible to the reader. Line 260, Figure 4. In comparison to the previous figures, this figure uses bold font in legend and labels. Please do not change the layout of the figures in one paper and stick to normal font.

3. Results and analysis Table 2: Why is there a column \sigmaˆ"_N that does not include any information? Figure 8 (and its discussion starting at line 344): The over-estimation of different density models with respect to different in-situ measurements (from GRACE, CHAMP and Swarm) has also been discussed in other studies. Please address previous findings. See for example: (a) Emmert, J. T. (2015). Thermospheric mass density: A review. Advances in Space Research, 56(5), 773-824. (b) Bruinsma,

S. L., Doornbos, E., & Bowman, B. R. (2014). Validation of GOCE densities and evaluation of thermosphere models. Advances in Space Research, 54(4), 576-585. (c) Mehta, P. M., Walker, A. C., Sutton, E. K., & Godinez, H. C. (2017). New density estimates derived using accelerometers on board the CHAMP and GRACE satellites. Space Weather, 15(4), 558-576.

5. Summary and Conclusions Line 430 is minimum → is minimal Line 431 may relatively be higher. This is no proper English, please rewrite.

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