Responses to the reviewers' comments on Manuscript angeo-2018-25

An empirical model (CH-Therm-2018) of the thermospheric mass density derived from CHAMP

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#Interactive comment by Dr. Förster, mfo@gfz-potsdam.de

It is a nice study, which complements previous global mass density studies based on the same CHAMP accelerometer data. This one appears to be built on a simpler set of functions, e.g., in contrast to empirical orthogonal functions in the papers of Lei et al., 2012 and 2013.

Different to Lei et al. (2012; 2013), we use the multivariable least-square fitting method for constructing the empirical model. It is difficult to judge which method is better. Models based on empirical orthogonal function (EOF) analysis do not consider the physical information, and are mainly based on the data. Furthermore, the basic functions of an EOF-derived model can change significantly by changing the data, e.g. by considering a longer or shorter data set. From our point of view as the dependence of the thermospheric mass density on different parameters has been defined before the fitting, the physical motivation for the choice of these functions is much easier to be understood, compared to the empirical orthogonal functions appraoch. Therefore, we use the least-square fitting method in this study. Similar analysis has been performed by earlier studies, e.g., Müller et al. (2009) and Liu et al. (2013).

Müller, S., H. Lühr, and S. Rentz (2009), Solar and magnetospheric forcing of the low latitude thermospheric mass density as observed by CHAMP, Ann. Geophys., 27, 2087–2099, doi:10.5194/angeo-27-2087-2009.

Liu, H., T. Hirano, and S. Watanabe (2013) Empirical model of the thermospheric mass density based on CHAMP satellite observation. J Geophys Res Space Physics, 118, 843–848, doi:10.1002/jgra.50144.

However, I'd like to make some comments and address some critical items of the method used.

Thanks to your valuable comments on our manuscript angeo-2018-25, that will definitely help to improve our results. Below you find our point-by-point reply.

First of all, the assumption of a constant scale height in global scale and for all seasons and local times (page 3, bottom paragraph) seems to be unjustified and might lead to apparent abnormal distortions in some of the deduced model parameters. It implies that the neutral temperature is assumed to be constant throughout, while it actually varies at least within a range of factor 2 to 3. The connection (normalization) to another empirical model or an iterative approach are a practicable alternative used elsewhere already many times.

We actually started our analysis with a solar flux-dependent scale height, but the resulting fits were disappointing. Satisfying fits between observations and model could only be achieved by introducing a constant scale height (over the height range 310-460 km) as defined in Equation (4) combined with modifications of the reference density by scaling factors. We agree that the scale height actually changes with temperature and composition, which vary, e.g. with solar and magnetic activity, latitude, local time, etc. Therefore, we have selected other six key parameters (defined in Equations 5-10) for describing the variations of neutral density at the reference altitude, 310 km. By using the multivariable least-square fitting method, the variation of scale height depending on different parameters is absorbed by the coefficients in Equations 5-10. As a consequence the coefficient, H_d, as defined in Equation (4) can be considered as a mathematical expression for an isothermal atmosphere, but does not reflect the actual scale height. In the revised manuscript we will make these circumstances clearer and discuss the implications.

Different from Yamazaki et al. (2015) and other studies, we did not normalize the measurements to a constant altitude by using estimates from models, as models like MSISE-00 have problems during the extreme solar minimum of 2008-2009 (e.g. Thayer et al, 2012; Liu et al., 2014). It will introduce extra errors into the fitting results. Therefore we decided to allow for height dependence, which worked quite satisfyingly in the end.

Thayer, J., X. Liu, J. Lei, M. Pilinski, and A. Burns (2012), The impact of helium on thermosphere mass density response to geomagnetic activity during the recent solar minimum, J. Geophys. Res., 117, A07315, doi:10.1029/2012JA017832.

Liu, X., J. P. Thayer, A. Burns, W. Wang, and E. Sutton (2014), Altitude variations in the thermosphere mass density response to geomagnetic activity during the recent solar minimum, J. Geophys. Res. Space Physics, 119, 2160–2177, doi:10.1002/2013JA019453.

It is not explicitely stated in the manuscript - do you use the data set based on the work of Doornbos et al., 2010, or some different approach (page 3, section 2.1)?

Yes, we used the same dataset as Doornbos et al. (2010), and we will clarify that in Section 2.1.

The reference height is said to be at 310 km with a fixed mass density "rho_0" of 10ˆ-12 kg/mˆ3 (page 6, line 15). I suppose, it’s a guiding or reference mass density. Equations (3) and (4) use the same "rho_0" parameter obviously in a different meaning; the values for the latter are given in Table 1 as ~0.102 and ~0.077 for the higher and lower solar activity level, respectively. This should be clarified.

The chosen reference height at 310 km is the lowest height of CHAMP considered in the 9-year period. As answered above, our present description is not too clear. The term “rho_0” does not give the density at the reference height, but it is scaled by all the functions as shown in Equations 5-10. By using the multivariable least-square fitting method this one factor for the mass density at 310 km results to 0.102 and 0.077×10^{12} $kg \cdot m^{-3}$ for high and low solar activity level. In the revised discussion we will better explain the physical meaning of the derived parameters.

You describe extensively the equinox asymmetry between ~March and ~September, but does not mention the annual asymmetry at all, although this is clearly seen as a striking difference between the solstice periods, e.g., in Fig. 4, middle panels, but less obvious as an interhemispheric difference between the December and the June solstice. The missing of the latter might be due to the assumption of the globally constant scale height, mentioned before.

We will add the description about the annual variation.

Minors:

page 8, line 9: "depends"

Corrected.

Fig. 7, the insert says "JB2008-HWM14" and gives different numbers of the medium value and the stddev as in the text (page 13, line 5). Is this done here by inclusion of the neutral wind model HWM14? Has the neutral wind been used to correct the mass density (accelerometer) measurements?

Similarly, there is a difference between text and insert with regard to the model CH-Therm-2018 or -2017?

Thanks. The insert has been corrected in Figure 7.