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Atmospheric Chemistry and Physics Discussions

Interactive comment on "MIPAS observations of longitudinal oscillations in the mesosphere and the lower thermosphere: Part 1. Climatology of odd-parity daily frequency modes" *by* Maya García-Comas et al.

Anonymous Referee #3

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The manuscript aims to diagnose migrating and nonmigrating tides in 5-year monthly mean averages of MIPAS/ENVISAT temperature observations between 20-150 km and 80S-80N. The Sun-synchronous ENVISAT orbit prevents a standard Fourier analysis due to the lacking local solar time coverage. Instead, the manuscript uses the well-known ascending-descending orbit differencing technique to obtain amplitudes and phases of the zonal wavenumber 0-4 patterns in the satellite local solar time frame. The inherent limitation of the approach is that it does not allow one to separate between diurnal and terdiurnal signals, and westward and eastward propagating nonmigrating tidal components. The observed zonally symmetric pattern, that is, the superposition

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Discussion paper

of the migrating diurnal and terdiurnal tides, is also analyzed on a monthly basis (w/o the 5-year averaging) and compared to the stratospheric Singapore zonal winds, in order to derive a QBO modulation amplitude. Comparisons with the migrating diurnal tide from the GSWM tidal model and NRLMSISE-00 are also shown.

Any new information about tidal characteristics in the 110-150 km region is of value to the aeronomy community since global tidal observations in the transition region into the diffusive regime, where tidal amplitudes and phase are constant with height, are very sparse. As such, I believe the manuscript should ultimately be published. There are, however, a number of important shortcomings in the manuscript that impact its scientific impact.

1. The meat of the manuscript are the data above 110 km since temperature tides in the MLT and below have already been extensively analyzed on monthly mean tides using SABER and MLS data. SABER diagnostics can actually separate tidal components in the MLT and MIPAS does not contribute much here. The bottom line of the lengthy description of MIPAS MLT tidal characteristics in section 4 is that it agrees with SABER. It thus should be scaled back significantly and the paper should focus on the new contribution from MIPAS, that is, tides above 110 km. For example, an interesting finding is the occurrence of the secondary k=4 amplitude maximum above 130 km in Figure 9. This certainly warrants more discussion. I also believe the higher peak altitude of the k=4 pattern warrants more discussion. From a modeling point of view, it is very difficult to shift the maximum towards higher altitudes. This would require a substantial change in the dissipation scheme, resulting in much higher tidal amplitudes in the upper thermosphere. This would then lead to breaking the currently very good agreement with CHAMP and GRACE DE3 tidal diagnostics. In addition, Figure 12 of Lieberman et al. (2013, doi:10.1002/2013JA018975) indicates that the tidal dissipation schemes are actually quite good when comparing to WINDII, including the height of the amplitude maximum. A higher altitude of the DE3 tidal temperature maximum -which would also change the vertical wavelength- would also be difficult to reconcile with DE3

observations above 110 in infrared emissions observed by SABER, since the latter are driven by temperature. See Oberheide et al. (2013, doi:1002/2013JA019278). More discussion of possible reasons for the inconsistency between MIPAS, the current empirical tidal models (and thus also with observed tidal winds from WINDII and infrared emissions from SABER) is needed.

2. There is a considerable number of migrating tide - QBO studies in the MLT from SABER, and it is difficult to see what is new in MIPAS. Everything agrees with SABER. I am OK with leaving section 5 as it is but the earlier work by Huang et al. should be given credit.

3. Tides above 110 km react very strongly to solar conditions, mainly due to the temperature dependence of thermal conductivity. The key figures in the manuscript are 5-year monthly mean averages, from 2007 to 2012, and as such do not account for the important solar cycle dependence. The current results only show that tides are present, but this is something the community already knows. What's needed here is to do the diagnostics for individual years because this would actually help modelers to better constrain dissipative processes and help with our physical understanding of tidal characteristics in the thermosphere.

4. The manuscript does not demonstrate a broad knowledge of previous work in the field. Global tidal observations in the thermosphere are sparse, but the authors seem to be unaware of a number of studies based on WINDII and SABER. See for example See for example Talaat and Lieberman (2010, doi:1029/2009GL041845), Lieberman e tal. (2013, doi:10.1002/2013JA018975), Cho and Shepherd (2015, doi:10.1002/2015JA021903), Oberheide et al. (2013, doi:1002/2013JA019278), and other. I grant that these studies deal with tides in winds and infrared emissions but they have been conclusively connected to in-situ tidal temperature diagnostics from CHAMP and GRACE in the upper thermosphere (see the various papers by Jeff Forbes) using empirical tidal modeling, including the abovementioned solar cycle dependence. I also believe the presented results need to be put more carefully into the context of recent

progress in whole atmosphere modeling, e.g., using WACCM-X, WAM, and GAIA. The current discussion in the GCM context is essentially limited to a one year long run of the CMAM model that has been done a few years ago. CMAM development has been stopped a few years ago and more up-to-date models (or at the very least the more recent eCMAM30 run) are more appropriate for this discussion.

5. What is the purpose of the GSWM/MSIS comparisons? What model version has been used and how? The given GSWM reference points to an old TIME-GCM study (where GSWM was used as a lower boundary condition only). There are several versions of GSWM around, the most recent one is GSWM-09 (see papers by Xiaoli Zhang). I doubt that this one has been used since no reference is given. Older GSWM versions had issues with seasonal variations and partly did not include the in-situ tidal forcing in the thermosphere. Also, GSWM is for 110 sfu (if I remember correctly) and does not include any solar flux dependence. I am also puzzled to see that MSIS shows such a poor agreement with MIPAS. The MSIS amplitudes close to 150 km look way too small for migrating tides. Forbes et al. (2011, doi:10.1029/2011JA016855) compare the MSIS migrating diurnal tide at 400 km with CHAMP and GRACE. The agreement is actually quite good with amplitudes on the order of 120 K.

6. Several conclusions are not supported by the data and speculation. (1) How do you know the propagation direction from the latitude/height Figure 7 (section 4.3, section 6)? Longitude/height plots give some indication about propagation direction, assuming that all tidal signals are propagating upward w/o any possible downward propagation or in-situ forcing (which is an assumption that needs to be stated!). (2) The TW3 as the leading migrating component at 110 km (section 4.1, section 6) is mere speculation since MIPAS cannot separate DW1 and TW3. In-situ DW1 forcing is as likely (or more likely).

7. Methodology section 3. I doubt that a non-expert in tidal satellite diagnostics will understand this section. It gives an overly complicated description of a well-established method that has been applied over the past 20 years to every single remote sensing

infrared instrument when looking into tides. I strongly suggest to significantly shorten the section (or moving the shortened version into section 2 altogether). If the authors insist to keep this level of detail, the section should be moved into an appendix, but with the addition of a few intermediate steps that have been omitted, to help readers not familiar with the satellite orbit geometries and sampling.

Specific comments.

line 523. Oberheide et al. (2009) do not discuss the QBO in the westward propagating migrating tide, only in the eastward propagating DE3.

The lower altitude in the Figures should be moved up to 50 or 70 km. There's not much tidal activity going on in the stratosphere.

The language is mostly fine but another round of proof-reading by the native speaker on the co-author list would be good.

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