Anonymous Referee #3

- **Reviewer's comment:**

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 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.

Author's response:

We thank the referee for his/her very useful comments that we think have improved the manuscript. We have taken into account his/her suggestions.

Main changes of the manuscript are: update of the version of retrieved thermospheric temperatures (results barely change); inclusion of new figures with lower altitude of 40 km; old Sect. 3 has been moved to an Appendix; introduction and discussion on thermospheric tides has been extended.

- **Reviewer's comment:**

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.

Author's response:

MIPAS, MLS and SABER are different instruments on different platforms. We think measurements of all three of them (and other instruments) are equally interesting and, thus, it is worth reporting them all.

MIPAS and SABER results qualitatively agree, a result itself, but they do not coincide. As explained several times in the manuscript, temporal resolution of SABER standard analyses is worse than that of MIPAS (2 months vs. 1 month).

Opposite to SABER, that yaws every two months to observe the two poles alternatively, MIPAS provides a pole-to-pole view of the MLT. In this context, the effect of the mesospheric migrating tide measured at high latitudes simultaneously in both hemispheres can be reported here. Also, high latitude tide activity can be tracked along the year by MIPAS (k=1,4).

Compared to MLS, MIPAS vertical resolution, that affects wave structures (see Sect. 2), is better.
Finally, MIPAS offers the rare opportunity to observe the atmosphere from the stratosphere up to 150 km globally. Since tides generally propagate from low altitudes, a continuous vertical coverage from a single instrument is an advantage, as we mention in the abstract and in Sect. 1.
Nevertheless, following the referee's suggestion, we tried to shorten the text deleting several full paragraphs on the discussion below 110km in Sect. 3, particularly, in its introduction and Sections 3.1 and 3.2 but also in Sect. 3.5.

- **Reviewer's comment:**
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.
Author's response:
We appreciate this comment. We do not actually see such a disagreement with models, as the referee mentions. Amplitudes over the equator increase with altitude from the upper mesosphere to 120-125 km, where they reach its maximum in the altitude range examined in this work. This qualitatively agrees with the results for temperature from models, that place de DE3 peak around 110-115 km (see Sect. 3.5 for references). The 10 km shift might be partially explained by the large vertical resolution of MIPAS temperatures in the thermosphere. We note that, as pointed by the referee, the peak in the u and v fields are placed around 105 km. We have included a broader discussion on the k=4 peak altitude in Sect. 3.5.

- **Reviewer's comment:**
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.
Author's response:
Done.

- **Reviewer's comment:**
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.

Author's response:

We agree that analysis of individual years providing information on influence of solar conditions would be of great interest. There are several reasons why we think it is not adequate to perform such analysis from MIPAS data. Firstly, continuous observations should extend for a big portion of the solar cycle or, at least, for a portion for which changes in solar flux are important. Unfortunately, that is not the case for MIPAS data. The data cover solar flux changes of the order of 50 sfu. Maximum variations from 2007 to 2012 of monthly 10AM DW1 temperature contributions at 150km in NRLMSISE-00 are 4-7K (depending on season). Secondly, not only DW1 amplitudes vary with the solar cycle but also phases and those would be tracked in MIPAS locked LT measurements together with the solar cycle impact. Thirdly, regarding other significant modes in the thermosphere, variations along the solar cycle in the altitude range studied here are expected to be hardly detectable (see Fig.4 for DE3 in Oberheide et al., 2009)

We note that, even if the community already knows that the tides are present, a quantitative analysis, even from averages, is valuable. We focus our comparisons with SABER because it has provided reliable measurements of tides and they have been used as input for several models. We additionally recall that temperature tide measurements in the E-region are scarce and those covering from the stratosphere up to that region from a single instrument inexistent.

- **Reviewer's comment:**

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.

Author's response:

We have included a number of new references along the text and tried to put our results into their context (particularly in the introduction but also along Section 3).

- **Reviewer's comment:**

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.

Author's response:

The aim of this paper is to report and describe MIPAS measurements of tides in the context of previous measurements but not to perform a thorough comparison with models. That will be the focus of future work, for which taking into account MIPAS sampling and vertical resolution is needed. Nevertheless, in this new version of the manuscript, we tried to put our results in the context of several models, particularly in Sect. 3.1 and also in Sect. 3.5, when discussing the peak altitude disagreement.
We note that we only mention CMAM in order to identify DE1 as the main contributor of k=2 but do not perform a direct comparison with CMAM.

- **Reviewer's comment:**

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.

Author's response:

The purpose is twofold: to see how well MIPAS ascending-descending zonal means (k=0 mode; DW1+TW3) compare with 10AM-10PM migrating tide fields in models and to evaluate how the migrating tides in the a priori are transferred to MIPAS temperatures (the a priori acts as a vertically smoothing agent through the Tikhonov constraint in our retrievals). After this referee's comment, we understand that it is confusing to use two models for comparison. For consistency, we now only compare with one model at all altitudes. We chose MSIS, the a priori. The averaging kernels already pointed out that MIPAS thermospheric temperature measurement have a good quality (see Bermejo-Pantaleón et al., 2011), but the comparison presented here further supports that the retrieved temperatures do not contain significant information on model vertical structures. The comparison however shows a poor agreement between the measurements and the model. We have re-written the discussion accordingly.

We will postpone a thorough comparison with other models (not only GSWM) for a future work. Nevertheless, following another comment of this referee suggesting discussion in the context of models, we kept in the text the comparison with GSWM and also other models. We note that we now updated the GSWM results to GSWM-09 (Zhang et al., 2010a; Zhang et al., 2010b).

- **Reviewer's comment:**

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.

Author's response:

We note that in the comparisons we have taken into account MIPAS sampling. MIPAS measurements provide the contribution of the DW1+TW3 only at 10AM. This coincides with the total migrating tide amplitude (which is the one shown in Forbes et al.) only at

the altitudes where the phase is 10AM. That is not the case of altitudes above 130km, where the in-situ diurnal tide, with a phase at 2-4PM in MSISE data, dominates. According to MSISE and as shown in the plot, DW1 contribution at 10AM is 15K at 150km (compared to 30K total MSISE DW1 amplitude).
We already mentioned this caveat in the text. We even wrote the factor of underestimation of the total DW1 amplitude. Nevertheless, we have re-written that paragraph for clarification.

- **Reviewer's comment:**
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!).
Author's response:
We agree. We only know the tilt of the phase with altitude at certain latitude in the latitude/height maps of the phase (right hand side plots). The assumption of a vertical direction of propagation for proposing certain horizontal direction of propagation is now stated in the text in the introduction of Sect. 3 and also when used (several times along the manuscript). Following a suggestion of Referee#1, we now also include two figures where we plot the phase vs. altitude at selected latitudes (new Figs. 7 and 10). We hope this point is clearer in the manuscript now.

- **Reviewer's comment:**
(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).
Author's response:
We now make stress in section 4.1 and section 6 that DW1 is as likely.

- **Reviewer's comment:**
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.
Author's response:
We moved old Section 3 to an Appendix. We have also re-written the section with the hope that it is more easily readable now.

<u>Specific comments.</u>

- **Reviewer's comment:**
line 523. Oberheide et al. (2009) do not discuss the QBO in the westward propagating migrating tide, only in the eastward propagating DE3.
Author's response:
We re-wrote the sentences and say now that Oberheide et al.'s referred to DE3.

- **Reviewer's comment:**
  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.
  Author's response:
  We moved lowest altitude of the Figures up to 40 km. We note that Zeng et al. (2008) detected DW1 activity (although with very small amplitudes 1K) already in the lower stratosphere.

- **Reviewer's comment:**
  The language is mostly fine but another round of proof-reading by the native speaker on the co-author list would be good.
  Author's response:
  A native English speaker has proof-read the text.

References

Oberheide, J., M. G. Mlynczak, C. N. Mosso, B. M. Schroeder, B. Funke, and A. Maute (2013), Impact of tropospheric tides on the nitric oxide 5.3 um infrared cooling of the low-latitude thermosphere during solar minimum conditions, J. Geophys. Res. Space Physics, 118, 7283–7293, doi:10.1002/2013JA019278.

Oberheide, J., J. M. Forbes, X. Zhang, and S. L. Bruinsma Climatology of upward propagating diurnal and semidiurnal tides in the thermosphere *J. Geophys. Res., 116, A11306, doi:10.1029/2011JA016784, 2011.*

Zeng, Z., W. Randel, S. Sokolovskiy, C. Deser, Y.-H. Kuo, M. Hagan, J. Du, and W. Ward (2008), Detection of migrating diurnal tide in the tropical upper troposphere and lower stratosphere using the Challenging Minisatellite Payload radio occultation data, J. Geophys. Res., 113, D03102, doi:10.1029/2007JD008725.