Manuscript angeo-2020-42 "Lower thermosphere – ionosphere (LTI) quantities: Current status of measuring techniques and models" by Palmroth et al.

Response to Reviewer #1

We thank the Reviewer for their very positive and constructive comments on our manuscript. Below we present how we implemented them in the revision of the paper. The Reviewer's comments are reproduced in bold font, and our responses are given in normal font.

Overall quality of the manuscript (general comments):

The manuscript "Lower thermosphere - ionosphere (LTI) quantities: Current status of measuring techniques and models" by Palmroth et al. is a well written review of the lower thermosphere and ionosphere, with the focus on the open questions with a lot of them cannot be addressed without high-quality in-situ measurements and describing the state of the art of measuring this region. The manuscript is motivated by the Daedalus mission. In my opinion the manuscript is a very valuable contribution to the literature of the LTI region and will most likely be used by many researchers.

Thank you very much for this very positive opening statement.

Addressing individual scientific questions/issues ("specific comments"):

Line 5: should the wind dynamo be mentioned?

This is a very good point, we added a mention to the wind dynamo.

Line 8: does directly mean "in-situ" or without any elaborate assumptions?

Indeed, here the intended meaning is "in situ", hence we replaced accordingly.

Line 17: I think the mesosphere was originally termed "ignorosphere" since it is too low for satellites and too high for airplanes and weather balloons. So maybe change "this region" to the "LTI region".

The Reviewer is right: the concept of "ignorosphere" often refers to altitudes encompassing the mesosphere and part of the lower thermosphere. We have rephrased the statement originally on l. 17 and followed the suggestion. We have also slightly adapted the occurrences in the "Concluding remarks" section where "ignorosphere" was used.

Line 44: "the motion of the atmosphere is driven by both solar irradiance and waves." Do the authors mean thermal atmospheric tides caused by solar irradiance? Maybe reformulated so that it fits to the waves.

Indeed, thank you for this notion; both are of course driven by solar irradiance in the end. We have reformulated as: "(...) *the motion of the atmosphere is driven by the solar irradiance* ***and the waves it produces***."

Line 85: There are other efforts of whole atmosphere models: WAM (), GAIA ().

Thank you for pointing to these models, which have been briefly introduced in the revision.

Figure 2: Is the depicted neutral wind the total horizontal wind? Similar for the ion drift-is this the ExB drift and is the one perpendicular to the magnetic field?

We have updated Fig. 2 based on comments made by both Reviewers. In the new version, the depicted neutral winds correspond to the zonal component of the total wind at a high-latitude location (Nordkapp, Norway) near local magnetic midnight, during quiet and storm times. This has been made explicit in the figure caption.

Regarding ion drifts, they were obtained from the momentum equation via post-processing TIE-GCM outputs of the St Patrick storm event. The shown profiles correspond to their zonal component, which has also been made explicit in the caption. One can see that at altitudes above ~150 km the profiles do not exhibit strong variations, as ions are essentially following the ExB drift because collisions with neutrals are scarce (see also the discussion on l. 662 and l. 671).

Line 199: "total upward energy flux by resolved waves at 100 km" Does this refer to only 100km waves or also larger ones?

Here, 100 km refers to the altitude where the waves are considered, not their wavelengths. We have clarified it.

Line 201: "horizontal scales less than 200 km are poorly resolved" Shouldn't waves be resolved with wavelengths approximately 4x the resolution? How does this fit to the 100 km in line 199?

With the removal of the ambiguity in the previous sentence, this statement no longer seemingly conflicts with it. The fact that horizontal scales below 200 km are poorly resolved is in line with the cited Liu (2016) paper.

Line 518: "resolution ranges from one orbit to several days" It is not clear to me what is meant here? Orbit averaged to several day averaged? Is this temporal resolution-one measurement pre orbit or every few days?

Thank you for this comment, which calls for a clarification. This technique intrinsically allows inferring changes in the neutral density based on the orbit tracking of a single object with a temporal resolution of the order of three days or longer (Doornbos et al., 2008). However, by combining orbit data from multiple tracked objects, it is possible to obtain information on neutral density changes at a resolution of 3 h (Storz et al., 2005). We have added this clarification to the revised manuscript.

Line 661: Maybe the Weimer (2005) empirical ion convection model based on DE-2 data could be mentioned.

Thank you for suggesting this addition, which we have gladly included in the revised manuscript.

Line 715: section 3.6 Magnetic fields: I may have missed it but the summary does not mention that the Swarm was able to derive currents without any assumption of current flow due the constellation with nearby satellites. I find this an important point since at the end of the section, the E-region is

mentioned and this is the region where strong currents flow. So the interpretation of magnetic fields with respect to current flow without constellation is a challenge (e.g., see the modeling of Maute & Richmond 2017). Maute, A., Richmond, A.D. F-Region Dynamo Simulations at Low and Mid-Latitude. Space Sci. Rev 206, 471–493 (2017).

Thank you for the suggestion, we added a statement regarding using constellations vs single-spacecraft magnetic field observations, and referred to the Maute and Richmond (2017) paper.

Line 757: "which are essential also for FACs". It is not quite clear to me what this means? That FAC flows along magnetic field lines?

We have rephrased the sentence as follows: "*The phenomena are mediated by magnetic field lines, along which FACs flow (Sect. 4.2) and transfer momentum, and whose direction is also essential for the Poynting flux (Sect. 4.3) that transfers energy.*"

Line 773: "Above the E-layer, electrons and ions drift together and the ionospheric current vanishes." I do not think the authors mean that there is no ionospheric current above the E-region as the sentence suggests. Could this be further explained?

This is an excellent point; the above statement is misleading and might suggest that there are no currents at all at F-region altitudes. We have rephrased as follows: "*Above the E-layer, electrons and ions* ***essentially*** *drift together and the* ***horizontal*** *current vanishes.*"

Technical corrections at the very end ("technical corrections")

Line 125: Fig 2. Remind the usual -> reminds of the

Changed into "recalls".

Figure 1: I suggest to add the altitude or pressure range of the plots. Does it go from the surface to approximately 500 km?

Great suggestion, we added in the caption of Fig. 1 that the concentric circles indicate heights of 100, 200 and 500 km.

Figure 2: It would be easier to add approximate solar local times to the geographic locations of the profiles in the captions.

Thank you for this suggestion. In the updated version of Fig. 2, we have chosen to rather depict quiet-time vs storm-time profiles at a high-latitude location to illustrate the variability of the parameters. The approximate solar local time corresponding to the profiles was mentioned in the caption, following the suggestion.

Line 146: reference frames with the neutral gas velocity -> of the neutral gas?

Implemented.

Line 212: "In this topic, the" Should this read "in this study/review"?

We have removed “In this topic”, which was redundent with the rest of the sentence.

Line 300: suggested: to the lower atmosphere

Implemented.

Line 564: Should planetary waves be mentioned?

Added.

Line 600: Any reason to use speed instead of velocity?

Corrected.

Line 648: metre-> meter

Line 449: analyser -> analyzer (at least that is the spelling UTD is using)

Indeed, although we are using British English throughout the manuscript, we agree that for instrument names we should follow their preferred spelling. These have been corrected.

Line 835: engineering grade magnetometer

Implemented.

Line 1083: “propagating tides” upward propagating? Tides can also propagate east-ward and westward. Not all tides reach the F-region.

Modified as suggested.

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Response to Reviewer #2

We thank the Reviewer for their very positive and constructive comments on our manuscript. Below we present how we have addressed them in the revision of the paper. The Reviewer's comments are reproduced in bold font, and our responses are given in normal font.

The article "Lower thermosphere - ionosphere (LTI) quantities: Current status of measuring techniques and models" by M. Palmroth et al. constitutes a large review of the complex physical environment of Earth Lower thermosphere and ionosphere, covering theoretical considerations, experimental techniques and models. The term ignorosphere has been used to indicate this part of the Earth outer space, because of the difficulties of establishing measurement systems that could monitor its physical and chemical parameters on a continuous base globally. Therefore there are still many open scientific questions, that the proposed Daedalus mission could help answering from in-situ measurements. In the Lower thermosphere the forcings from above (principally solar radiation and particle precipitations) deposit large amounts of energy and the forcings from below (principally through tides and waves activity) can significantly affect the physico-chemical processes of both thermosphere and ionosphere. There are such a large varieties of phenomena taking place in this region, that it is not possible to resume all of them in a single review paper, and this paper covers most of them, also pointing out to some phenomena of interest also outside the scientific community, with recently discovered auroral activities. In the article various measuring techniques are described, covering not only the Lower Thermosphere region, but the thermosphere and ionosphere as a whole, with a specific focus to point out the current limitations of this lower altitudinal range and expressing the need for new satellite missions devoted to collect in-situ measurements of the various quantities involved: particles, densities, winds, magnetic fields... Despite the complexity of the topics covered, the article is well organised and refers to recent advances in all the disciplines involved, both from the theoretical and experimental point of view.

The article can be accepted for publication.

The following remarks are suggestions for minor corrections that could be implemented for improving the overall quality of the manuscript.

Thank you very much for those encouraging words and for the suggestions!

Reading the manuscript it clearly appears that it has been written by multiple authors: a few definitions are expressed a couple of times, but without significant overlap.

Thank you for pointing this out, we have tried to harmonise the style and reduce redundancy in the revision, especially in connection to the next comment below on acronyms and definitions.

Acronyms are not always defined at first use, sometimes they are defined multiple times e.g. (list non exhaustive): WACCM on page 4, 8, 16 and 19 for WACCM-X, which was already defined on page 4 GPS used on page 19, defined on page 20. GNSS defined on page 39, used on page 19, 20. IPIM defined on page 16 and 19 SuperDARN defined on page 31, used first time on page 23. TomoScand is not defined on page 40.

Thank you for noting these; we have gone systematically through acronym usage and definitions when preparing the revision. We have also added an appendix with the list of acronyms which appear several times in the manuscript, as suggested by the Editor. Please note that TomoScand is not an acronym, but simply the name of the ionospheric tomography network in Fennoscandia.

Some figures show ionospheric parameters computed using the International Reference Ionosphere model (IRI). The citation provided and the model shown are not the latest version, which was released in 2016, IRI-2016. The profiles shown might be identical in the IRI-2016, but the latest publications of this model should be cited (e.g., on page 4 and 17): Bilitza, D., D. Altadill, V. Truhlik, V. Shubin, I. Galkin, B. Reinisch, and X. Huang (2017), International Reference Ionosphere 2016: from ionospheric climate to real-time weather predictions, Space Weather, 15, 418–429, doi:10.1002/2016SW001593.

Thank you for raising this point. We have added the suggested reference to IRI-2016 in the text. However, since Fig. 4 was originally made with the 2012 version, we have kept the figure legend and caption with mentions to IRI-2012.

I think that captions of figures 1 and 2 should be improved. it is not clear if these two figures show the same altitude range: figure 1 is representing an isobaric surface, while figure 2 a fixed geometrical altitude range. In the text it is indicate that WACCM-X has been extended up to about 500 km, but an explicit indication in the figure caption would be helpful. In the representation of the quantities shown in figure 1, there are no axes to allow the reader to understand where the various panel's quantities are located in altitude.

Thank you for these suggestions. We have indeed added a mention in the caption of Fig. 1 that the concentric circles indicate altitudes of 100, 200 and 500 km in the panels. To avoid reducing the legibility of the figure, we have not tried to add proper altitude axes in the 3D panels, but hopefully the added information is sufficient to facilitate the interpretation.

Fig. 2 has been updated following suggestions by Reviewer #1, and more information on the profiles, the location they correspond to, as well as the approximate magnetic local time were added in the caption.

Fig. 2 shows profiles from the ground up to 500 km altitude, whereas the topside surface in Fig. 1 is indeed isobaric (corresponding to a pressure value of 4.055e-10 hPa). We do not think adding this value into the manuscript would be meaningful, as we believe the altitude information (concentric circles in each panel) is easier for the reader to interpret within the scope of this review paper.

On page 11 line 286, the need of a sufficient horizontal resolution and time resolution at low latitudes is expressed, but it is not quantified. I suggest to provide some values that could be used as guidelines for reaching specific scientific goals. This same remark is valid for other conclusions of this article, where no explicit values are indicated.

This is an excellent point; indeed it would be very valuable to be able to refer to this paper for requirements in the future. We have taken this suggestion into account in the revision and provided quantified (or at least more detailed) needs wherever possible, in each subsection of Sect. 2.

Figure 4 shows combined results from two different empirical climatological models: NRLMSISE-00 for the neutrals and IRI for the ions. It shows that for some species (N and O) the scale heights of the neutral and its corresponding ion are extremely different. It turns out that in the upper part of the plot the density differences can be of many orders of magnitude. This points out clearly the

limitations of these models and an indication that in-situ measurements of both neutrals and ions are necessary.

Thank you for this comment, we have discussed this point in the revision. Essentially, the reason for the different scale heights for ion and atomic species (e.g., O^+ and O) is that there are charge-exchange reactions and other aeronomic processes at play in the upper atmosphere, which affect the corresponding number density profiles differently.

Page 38, line 1069: following earthquakes, even tsunamis are source of gravity waves observed in the ionosphere (e.g. Makela, J. J., P. Lognonné, H. Hébert, T. Gehrels, L. Rolland, S. Allgeyer, A. Kherani, G. Occhipinti, E. Astafyeva, P. Coïsson, A. Loevenbruck, E. Clévédé, M. C. Kelley, and J. Lamouroux (2011), Imaging and modeling the ionospheric airglow response over Hawaii to the tsunami generated by the Tohoku earthquake of 11 March 2011, Geophys. Res. Lett., 38(13), L00G02).

Thank you for this suggestion, we have added tsunamis as additional sources of gravity waves (including the provided reference), as well as volcanos and human-made explosions.

On line 1065 the focus of this part has been put on gravity waves, but some of the phenomena highlighted produce mostly acoustic waves. It is stated correctly that TIDs are observed, which encompass both kind of waves. I think that the unaware reader might not perceive their differences.

Thank you for pointing this; we have revised this subsection to discuss both acoustic waves and gravity waves. Further, we have also emphasised that investigating the link between the TIDs and gravity waves is one of the objectives for Daedalus.