Ann. Geophys. Discuss.,
https://doi.org/10.5194/angeo-2020-55-RC1, 2020

Annales Geophysicae Discussions

Interactive comment on "Seasonal evolution of winds, atmospheric tides and Reynolds stress components in the Southern hemisphere mesosphere/lower thermosphere in 2019" *by* Gunter Stober et al.

Anonymous Referee #1

Received and published: 14 September 2020

Reviewer comments

MS No.: angeo-2020-55 Seasonal evolution of winds, atmospheric tides and Reynolds stress components in the Southern hemisphere MLT in 2019 by Gunter Stober et al.

Summary evaluation:

This paper presents an analysis of data collected from measurements of six meteoric radars in the mid- to high-latitude southern hemisphere. The measurements are related to the height range of 75-105 km. The analysis aims to separate the mean winds, tides,

and residual fluctuations, the latter representing gravity waves (GW) and considered as Reynolds stress components. One-year-long time series of each parameter from each of the radar are constructed revealing characteristic seasonal patterns at each location. The momentum fluxes and wind variances are calculated using a recently developed algorithm (described elsewhere) which allows more accurate decomposition of the measured signal into a background flow and the GW fluctuations. Considerable longitudinal and latitudinal asymmetry (or variability) is found in the wintertime zonal and meridional wind patterns. Because a minor stratospheric warming occurred in the southern hemisphere in 2019, the observed asymmetry is believed to be related to the asymmetric structure of the polar vortex at mesospheric heights forming spatially variable filter for vertical GW propagation.

The study introduces new multi-radar observations, utilizes sophisticated data retrieval methods, and contributes to a better understanding of the mesosphere-low thermosphere circulation and vertical coupling in the atmosphere. The paper seems significant and appropriate for publication but substantial revision is needed.

The paper is interesting to read but this referee met some inconsistencies while reading. The following questions and comments should be addressed and clarified in the revised version.

Major comments.

First of all, in some way, the paper is not clearly and concisely written. In particular:

1) Introduction (ll. 16-76). This section seems imbalanced regarding the topic indicated in the title. It is focused on the gravity waves only, while the title (and the study as a whole) includes winds and tides. Briefly: the first part of the introduction discusses the role of GWs in the atmospheric dynamics (ll. 16-46), the second part (ll. 47-63) discusses the advantage of the meteor radar in the GW detection along with the techniques applied to retrieve the wind fluctuations, the rest of the section (ll. 64-76) presents the list of the observational sites and introduces the structure of the paper.

The mean winds and tides in the southern hemisphere should be introductorily discussed in more detail in order to put the present study into context in this regard.

It is worth mentioning (in the introduction or anywhere throughout the paper) what approximate fraction of the energy is carried by the GWs with periods less than those detected by the meteor radar (<1 h)?

2) Since comparisons are always made between stations, but not between components, it seems reasonable to rearrange Figs 2-6, 8, 9 so that zonal and meridional (and vertical) parameters from different stations are presented in separate plots. In this case, it is easier to visually identify spatial similarities and differences. Please consider rearranging.

3) Conclusion. This section (57 lines in total) is too broad to be a proper conclusion. A great part of the material included is more appropriate for the discussion section. There are even several references. The section should be shorter and more focused. In its present form, it is difficult to take the main new results of the study. Here (or in the introduction), the main goal of the paper should be explicitly formulated.

Lines 509-510. In the present paper a new retrieval algorithm is not presented but rather utilized, isn't it?

4) Fig. 5. It is curious why the spring semidiurnal tidal amplitude at KEP is much larger than at all other stations. Is there any explanation? Other parameters do not show any considerable difference for KEP.

5) Lines 453-458. The methodological shortcoming of this statement is that, based on one particular year during which a SSW occurred and without comparison with non-SSW years, we are not confident that the observed variability in the MLT winds is related to a wobbling and asymmetric polar vortex. The statement should be taken with caution. Maybe, the typical inter-station variability is of the same order.

Minor comments

1) Title. Since the broad geophysical community of AnnGeo readers may not be very familiar with the acronym MLT, it seems better to use the full name: mesosphere-low thermosphere. 2) For convenience, please indicate the radar codes in Fig. 1 (e.g., in the right panel) and include the full name of the sites to Table 1. 3) Line 186. Correct reference?

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