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Interactive Comment

Interactive comment on "The latitude dependence and probability distribution of polar mesospheric turbulence" *by* M. Rapp et al.

M. Rapp et al.

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We greatly appreciate the reviewers' comments on our manuscript which have helped improving the quality of our work. For the revised version of our manuscript we have taken the reviewers' suggestions into account where possible.

General remark

Before we start addressing the four reviews point by point we first want to make a general remark addressing one critical comment which is common to all four reviews: All four reviews have pointed out that the small number of sounding rocket flights available to us prevents us from drawing any definite conclusion on the latitudinal

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variation of turbulence from our measurements alone. In the light of this argument we acknowledge that the title of our manuscript and some of the conclusions were too ambitious and not sufficiently supported by our arguments.

However, our in situ data are actually the only available and precise data on mesospheric turbulence at 69^{o}N and 79^{o}N that is currently available and will be available for a long time given the costs of these sounding rocket experiments. Hence, we argue that these two existing data sets should be communicated to the scientific community and their differences should be discussed provided that the shortcomings regarding statistical significance with regard to the climatological mean are clearly identified. In addition, even with these shortcomings identified, we further consider it justified to take identified differences between our data sets at 69^{o}N and 79^{o}N as motivation to study the latitudinal variation of polar mesospheric turbulence with two very different versions of a mechanistic GCM. To the best of our knowledge, this GCM with either parameterized or resolved gravity waves is the only middle-atmosphere GCM that takes the turbulent dissipation (i.e. the frictional heating owing to parameterized Reynolds stresses) in an hydrodynamically consistent way into account and accordingly allows for a diagnostics of turbulence at all (Becker, 2001; Becker 2003; Becker 2004).

In order to reflect these points in our revised manuscript we have first of all changed the title to: *The latitude dependence of polar mesospheric turbulence: suggestions from available in situ soundings and global simulations*.

Furthermore, we have added the following paragraph to the end of section 2:

However, we also have to note that given the small number of rocket flights available to us we can certainly not exclude the possibility that the observed difference does not

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represent the true climatological difference in mesospheric energy dissipation between these two latitudes but might be a consequence of an under-sampling of atmospheric variability. Nevertheless, we consider our observational results as sufficient motivation to investigate the latitudinal variation of polar mesospheric turbulence in a global circulation model.

In addition, corresponding statements have been added to the conclusion section and the abstract of our manuscript.

In the following we address the four reviewers' comments point by point.

1 Reply to Referee 1

1. See our general comment above.

2. The reviewer criticizes that we took data from all altitudes between 72 and 95 km to derive histograms for the two latitudes where measurements are available. We agree with the reviewer that the full altitude range was problematic because of the very large gradient in the mean dissipation over this altitude range. As a consequence, we have now limited our analysis to the altitude range between 82 - 92 km which is the altitude range with maximum turbulence occurrence rate and strength (of course, the altitude range for the corresponding analysis of model results was limited accordingly). Importantly, this change of the altitude range does not alter our conclusion with regard to the significant difference between the data sets at the two latitudes. In addition, we now also show altitude resolved histograms for the case of our measurements at 69^{o}N which show that they are actually very similar in the considered altitude range between 82-92 km

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(Figure 4).

As for the physical meaning of these histograms, they represent estimates of the probability distribution of turbulence in the vicinity of the polar summer mesopause.

3. In order to explain in better detail how we derive energy dissipation rates from our data and on which basis we can classify some of the observed spectra as ‘non-turbulent’, we have added a new Figure (now Figure 1; see also the corresponding new discussion in the beginning of section 2) showing examples of ‘turbulent’ and ‘non-turbulent’ raw data and spectra. This clearly shows that our criteria to regard some spectra as non-turbulent is not just that it can not be fitted by a -5/3-power law in wavenumber. Consequently, we are also confident that our method does not result in an underestimation of the heating rates.

4. The reviewer criticizes that the model results are neither unique nor do they reproduce the observations one by one.

   Besides our general remark regarding the used GCM above, we note that it is actually one of our results that the two models - one with a Lindzen-type gravity wave parameterization (model 1) and and one with resolved gravity waves (model 2) - do not give the same results. We actually considered it important to discuss the results from model 1 since many state-of-the-art models of the middle atmosphere actually use similar gravity wave parameterizations. We discussed the mechanism resulting in the observed structure and then pointed out that the more sophisticated (though certainly not ideal) model 2 shows differences which point at the fact that the mechanism in model 1 is only of minor importance and that the latitude dependence of turbulent energy dissipation is mainly dominated by the latitudinal variation of the underlying gravity wave source (which in model 2 is a self-consistent consequence of the simulated tropospheric dynamics which cannot be tuned in any way). Since most of the available models do not account for the latter point (latitudinal variation of gravity wave source) we consider this an important result. Despite these differences, however, we also find it noteworthy that the overall feature, namely the decrease of dissipation rates towards the pole, is a robust result of both model versions.

We have tried to reflect the above arguments in our revised manuscript by adding references to independent model-results showing similar results as our model 1 and we have also added a statement that the poleward decay of simulated dissipation rates is actually a robust feature of both models.

As for the point that the models do not reproduce the data one by one we note that these models are intentionally not tuned to yield a one to one correspondence but are rather designed to study general dynamical effects. In the revised version of our manuscript we have added plots showing zonal mean climatologies of temperature, the zonal wind and turbulent heating so that the reader can clearly see that the models only give an overall reasonable picture of the atmosphere. Nevertheless, the simulated latitudinal and seasonal variation of turbulent heating is a robust feature of these models.

2 Reply to Referee 2

1. See our general comment above. In addition, the reviewer is actually wrong with regard to his suspicion that we only used a subset of our turbulence data. The presented data are all in situ turbulence measurements in the polar summer

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mesosphere (based on neutral air density fluctuation measurements) that have been conducted to date and there are no additional data 'at our fingertips' (please also see our previous publications by Lübken (1997), Lübken et al. (2002) where the referee can see that we actually discuss more summer data than in these previous papers). Should the reviewer be referring to in situ measurements of positive ion densities we note that these are not useful as passive tracers for turbulence in the summer mesosphere since these spectra are modified by the Schmidt-number effect due to the presence of charged ice particles (see Rapp and Lübken (2004) for a review paper with more details and references on this effect).

2. With regard to errors we have added a statement about typical errors of our dissipation rate measurements. In addition, for testing the statistical significance of the difference between the histograms of observations at 69^{o}N and 79^{o}N we had certainly already taken into account the variance of these histograms. For clarification, we have added a paragraph to section 2 where the procedure of the Student's t-test is now explained in detail and where we also state the corresponding values (means and confidence intervals of these means) explicitly. As for error bars on the histograms in order to judge whether the distributions are truly lognormal or not, we note that we never intended to make the statement that the distributions are exactly lognormal. What we wanted to convey was that the distributions were approximately symmetrical in log(Q), i.e., approximately lognormal. While it is clear that our distributions are clearly much broader than a normal distribution, the available number of measurements does not allow us to draw any further conclusions. We have tried to make this even clearer in the revised version of our manuscript.

3. See general comment above. As suggested we have also added histograms for different altitudes at Andenes. The reviewer is also completely right that a discussion of just two latitudes does not give a picture of the latitude dependence of

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a certain quantity. While we cannot present this from our measurements we can certainly do this with our models such that we now show complete zonal mean climatologies of the relevant fields (see new Figure 5 and 6). As for the request for standard deviations, we note that we actually provide MORE information than that by showing the actual distributions (which are - as described above - not Gaussian so that standard deviations are actually not very meaningful).

As to the suggestions a-d:
a) Done.
b) Unfortunately, there is no further data than what we used.
c) See arguments above.
d) In order to underline that the observed distribution function at Andenes is close to lognormal we have now fitted a lognormal distribution to our data and show this in Figure 3. Still, we note that this should be considered only as qualitative support for our notion that the distribution is lognormal. Please see our arguments above.

4. Specific comments:

   - Please see our arguments above.
   - Our procedure to derive dissipation rates from our data and to identify non-turbulent spectra has now been explcitly described in the beginning of section 2 (including the new Figure 1).
   - We have added this reference as suggested. Thanks for pointing this out!
   - See above: we have used all available summer soundings.
   - This is certainly true! Thanks for pointing this out! We have added a corresponding statement to our manuscript.

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- See arguments above.
- This is a valid point. For once we now show global fields of temperature, zonal wind and energy dissipation. In addition, Figures 5 and 6 (now Figures 7 and 8) now show data from the relevant latitudes.
- This Figure is only meant to illustrate the variability in the model. Quantitative analysis is provided in the corresponding histograms which we now show for the two relevant latitudes.
- Technical correction: Done.

3 Reply to Referee 3 (B. Williams)

Regarding the general statements about our manuscript, please see our general comment above.

1. (Point 5): (a) This is indeed a very valuable suggestion, however, the implementation is unfortunately far beyond the scope of the present paper. In fact, the Svalbard temperature data are only now in the process of being published - and this only with regard to the general observed climatology and not with regard to tides. A tidal analysis will not be feasible within the next months. In any case, the reviewer makes a very valid point under point 5 (d). Given the fact that we only have 3 single profiles we can certainly not check if we have under-sampled natural variability (including tides). This has now been explicitly stated in the manuscript (see also general comment above).

   (b) See general comment above.

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(c) This is certainly a valid point. However, to our knowledge gravity wave activity at Svalbard has not been documented to date. Part of this will be possible from the lidar data that the referee mentioned but the corresponding analysis is not yet available. As the minimum we have now documented the wind fields in our models better by showing global zonal mean climatologies from both models. This allows the reader at least to see that the overall structure of the modeled wind fields is indeed consistent with observations. A corresponding statement has been added to the manuscript.

(d) See above.

2. (Point 6): We have added a more detailed description of our method to derive turbulent energy dissipation rates (and heating rates) from our data.
3. (Point 13): The grey text is now in red.

4 Reply to Referee 4

1. The first paragraph of this review states the problem with the number of observations and criticizes the way we initially derived histograms for the two latitudes. The first point is covered in our general comment above. Regarding the histograms we narrowed the altitude range to 82 - 92 km where the turbulence occurrence rate and strength is maximum and relatively homogeneous. Likewise the altitude range for the corresponding analysis of model results has been limited accordingly. In addition, we added altitude-resolved histograms for the location of Andenes (please also see our answer to referee 1, point 2).

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2. As explained in our general comment above we have now explicitly stated that we may have under-sampled natural variability. Hence, we can only consider our obsevations as indications motivating us to further consider global simulations.

3. The model fields have now been better documented - see our answers above. As for the distribution type, we note that the model with resolved gravity waves actually produces the distributions now shown in Figure 8. This is not an assumption but a result of this model calculation.

4. As suggested we have toned the title of the manuscript down and have tried to state the restrictions of our data sets and model results explicitly.

5. Minor comments: The statement that this apparently was a 'normal summer period' was made on the basis of temperature observations with a potassium resonance lidar. Unfortunately, we can not answer why a completely 'turbulence-free' atmosphere does not immediately show up in the temperature data. One explanation might lie in the fact that the observations were not made at exactly the same place. Also, we certainly have no information on how long this 'turbulence-free' state lasted.
   Finally, we have added a more detailed discussion of the models and their degree of reality as requested.

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