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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Simultaneous lidar observations of temperatures and waves in the polar middle atmosphere on both sides of the Scandinavian mountains: a case study on 19/20 January 2003" *by* U. Blum et al.

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The observational study of simultaneous lidar observations of the temperature structure in the middle atmosphere is a unique contribution. In the past only airborne lidar observations of polar stratospheric clouds (PSCs) revealed the nature of stratospheric mountain waves upstream, above and downstream the Scandinavian mountain ridge. Thus, the general goal of the experiment to explore the stratospheric wave pattern well above the PSC level on the upstream (western) and downstream (eastern) side of the mountains for the prevailing westerlies is an interesting scientific topic and the current study certainly the first one addressing this issue. Generally, the paper is well written. However, the whole text needs improvement in wording and correct scientific formulation.

I have a major concern: Is this case really suited for a comparative study of wave signatures on the up- and downstream side of the Scandinavian mountain ridge? I don't think so! Here, my main two arguments:

(1) The whole analysis of the wave event assumes implicitly that the waves are excited by orography. But in the whole paper there is no clear indication that this is actually the case. I have serious doubts about the orography as wave generator as explained below. I rather suggest instabilities occuring during the warming event at the edge of the Arctic polar vortex as a possible source of the wavy structure in the stratospheric T'-profiles of Fig.3.

(2) Analysing the weather situation during the observation days, I found that the whole atmosphere was rather transient both in the troposphere as well as in the stratosphere. In the troposphere a weakening low over Finland propagated eastward leading to northerly and later in the night to easterly lower tropospheric winds in northern Scandinavia. At the same time, the Arctic polar vortex was splitting during a warming event. As a result the vertical structure of the vortex edge was strongly deformed and the vortex edge vertically tilted. These transient phenomena don't give sufficiently stationary conditions desirable for a comparison of the stratospheric wave structure above the two separated lidar instruments.

Altogether, I suggest major revision of the present paper. I suggest to reanalyse the present case more carefully, especially in terms of the nature of the wave structure in the T'-profiles. These observations are interesting enough to be explained! As a kind of moderate help I put additional plots of PV, Wind and Geopotential etc. on the anonymous ftp-server ftp.pa.op.dlr.de. The files can be downloaded from the directory pub/doernbrack/blum. In the further discussion I will refer to them.

DETAILED DISCUSSION

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Titel:

- I would skip "and waves": You measure temperature! - replace "both sides" by east-west

Abstract:

- "extensive effects on ...": specify clearly what you mean - "either side": again, a more correct definition of the location is desirable - "propagation conditions": you mention these conditions but no excitation conditions; as this points follows more often, here my comment:

The ECMWF analyses (20030119.ps and 20030120.ps in pub/doernbrack/blum) show that the wind direction during the observational campaign at 850hPa was predominantely north at 19.1 18UT and turned to easterlies at 20.1 00UT and 06UT. The northerlies were not very strong (~5..10m/s). The 850hPa wind became stronger toward east (see the Sodankylä radiosonde sounding of 19. Jan 2003 12UT with 20m/s at ~850hPa!!). Under these conditions, wave excitation by flow past the Scandinavian topography is rather unlikely. Especially, there is no flow nearly perpendicular to the mountain ridge for which a comparison between the lidar sites might useful. The satellite imagary (0301...scand.jpg) also shows no indication of wave clouds.

Furthermore, during the whole period in the upper troposphere (p~<500hPa) to the lower stratosphere (p~5hPa) we have westerlies and above this level northerlies again which are associated with the polar vortex east of Scandinavia. Thus, the wind turning alone filters possible (but unlikely) mountain waves! Therefore, the horizontal divergence don't indicate wave propagation for all pressure levels p>10hPa. Just above that level, weak indications of a wave pattern appear in the divergence field.

The polar vortex: I put horizontal sections of the hemispheric Lait PV (ErtelPV*(Theta/420)**-4.5) in the directory pub/doernbrack/blum/laitpv. These plots at different isentropic levels clearly reveal the already completed vortex breakup at higher levels (1100K) and the ongoing splitting of the vortex at lower stratospheric levels (600K) at 20.1 00UTC. The processes occuring during the warming event certainly result in significant shear of the horizontal wind in magnitude as well as in direction. For example consider the edge of the polar vortex (yellow color in the plots) at 20.1 00UTC: at 600K the edge of the eastern lobe of the splitting vortex lies zonally over both lidar sites whereas at 1110K the edge is displaced eastward: this must result in a significant vertical tilt of the edge and substantial shear of the horizontal wind vector. The associated instabilities might be a possible source of waves observed above Esrange or Andoya. I know, it is only a hypothesis. However, radar studies at Urbana-Champaign, IL also show a close correlation between stratospheric warming events and an increase of the gravity wave activity (paper given by R. L. Herman - rlherman@atmos.uiuc.edu - at the AGU Chapman Conference on Gravity Wave Processes and Parameterization, Hawaii, 2004).

- "wave pattern show random distribution ..": isn't a wave something regular and random something turbulent? Are the T-perturbations really the result of coherent wave motions?

- last sentence: see comments before, I doubt the orographic nature of the waves

Introduction:

The Introduction should be revised. The authors should give a current state of the knowledge of stratospheric mountain waves above Scandinavia including the most relevant recent publications. In the recent years, many international field campaigns were devoted to explore mountain wave induced PSCs above Scandinavia. My own moderate contribution is summarized in Dörnbrack etal 1999 and 2002. There, we explore the observed and simulated wave structures above the Scandinavian mountain ridge. It might be of interest to the authors to relate their own observations to ours.

Another point I really miss in the Introduction is the discussion of other measurements during the January 2003 period (SOLVE2, EUPLEX, radar observations at Es-

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Interactive Comment

range/Andoya, radiosonde soundings). At least partly these observations could help to clarify the excitation and propagation conditions based on data (as ECMWF T106 analyses are inappropriate as seen later).

- does "gravity waves" really "drive the global circulation"?? - p971line4: Störmer is written with ö not with the Norwegian o/ - p971line5: no word about excitation - Scorer parameter: $N^2/U^2 > k^2$ for propagating internal gravity waves assuming constant N and U, k is the horizontal wave number - p971line8. more specific: critical level where U=c inhibit vertical wave propagation - p971line13-15: this is interesting: how do you identify atmospheric wave based just on T'-profiles? And: are the profiles in the paper just one example and T-fluctuations like this are always present? - outline of the paper should be addded

Data Set:

- p972line5-8: see general remark (1); here, the situation is different and you should discuss this! furthermore: why do you expect wave signatures upstream of the mountains? - p972line16: is the seed temperature consistent with the ECMWF data used in the study?

Method:

- p974line15: the dispersion relationship is valid for non-rotating hydrostatic gravity waves (see Gill, 1978); I would replace "simplified" by this wording. The horizontal group velocity of these hydrostatic gravity waves is zero, I don't understand the discussion about the meridional and zonal propagation. If you consider rotating hydrostatic gravity waves (see our JGR 2002 paper), then you can refer to horizontal propagation!

Meteorological Background:

- a more careful analyses would help to distinguish between tropospheric (assumed excitation region) and stratospheric conditions. I would suggest to rewrite this section. - add in Fig. 1 that the T-field in the nightly mean temperature. - p975line25: critical level filtering doesn't depend on T-structure as U-c=0 is the definition - p975line26: lambda_max doesn't refer to conditions where you can have significant wind turnig and critical level filtering associated with this - again: no consideration of excitation mechanism: in 2001 Dörnbrack and Leutbecher (JGR) proposed a simple parameterization of meteorological condition suitable for the propagation of gravity waves in the stratosphere; probably something simple as this might relate the present case to known knowledge

Observations

- p976line18: temperature perturbations - p976lines21-22: repitition (inhibition of ...) - p976line24. it would be interesting to get a comparison with the magnitude of T-perturbations in non-wavy periods; do you have "typical" profiles to compare with Fig.3? - why T'(z) doesn't increase exponentially with increasing altitude as expected for vertically upward propagating waves due to the decreasing density? - p978line2,3: no wonder if you consider T106 analyses! To make the difference, additional observations at both sites would be helpful! - p978line5: What is a "quasi critical level"? I cannot follow the discussion about critical levels and propagation: some clarification is needed!

References

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Interactive comment on Atmos. Chem. Phys. Discuss., 4, 969, 2004.

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