Atmos. Chem. Phys. Discuss., 3, S2543–S2546, 2003
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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Analysis of a jet stream induced gravity wave associated with an observed ice cloud over Greenland" *by* S. Buss et al.

S. Buss et al.

Received and published: 9 February 2004

We first thank Michael McIntyre for his constructive and insightful comments on our paper that helped to improve the discussion of our results.

General comments:

-Model run with higher resolution: This is a justified suggestion. However, there are at least two reasons why we will/can not check the stability of the larger cloud structures under resolution increase: 1) HRM is a hydrostatic model, it makes therefore little sense to further increase the resolution 2) A non-hydrostatic simulation is beyond the scope of our study because a) we do not dispose on cpu time any more as my PhD project is finished and b) a successful non-hydrostatic simulation would require months of work. A sentence clarifying this point has been added in the Appendix (resolution).

- We removed as well as we could the confusion between instability, adjustment or com-

bination of both throughout the manuscript. A reference to the encyclopedia chapter has been added.

- The same holds for "let" vs "led" (p5878 line 12)

- Lat/lon values have been added in Figure 3.

- Yes, we take account of the time evolution of the background winds. This can be seen in the evolution of the apparent frequency along the ray (Fig 10f). These variations are small in the stratosphere, which demonstrates that the simulation is valid, at least above the tropospheric jet. The employed ray-tracing equations have been added to section 5.3 along with a clarification about the unsteadiness of the background and other sources of errors. In fact, Section 5.3 has been completely reorganized, rewritten, and a new figure (Fig 12) has been added.

Nit-picks:

p5876 line 24: Of course. Text has been clarified.

p5878 line 6: Thanks for this pertinent hint. The formulation has been adapted and a side remark on convection has been added in Section "synoptic situation".

p5878 line 20: Thanks for pointing this out. The comparison does not make sense and has been removed.

p5878 line 22 & line 25: Yes. Text has been clarified.

p5882 line 2: Wording has been changed.

p 5883 line 1: We neither. Still, you might be mixing two different processes. In the (Christian) Junge-layer (part of the stratosphere in which enough H2O and H2SO4(g) is available, and temperature is low enough) homogeneous nucleation of the background aerosol might occur. This is the liquefaction of these two trace gases to form small condensation nuclei (radius about 0.1 microns) in concentrations of about 10 cmˆ{-3}. Heterogeneous formation (at higher temperatures) of these aerosol can occur in

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several ways, e.g. when volcanic ashes reach the stratosphere. Suppose now the temperature sinks further, then the liquid aerosol will uptake HNO3 from the gas phase, forming the supercooled ternary solution droplets. If the temperature drops further, the liquid water within will freeze. This process is called homogeneous ice nucleation, in the case there is no solid substrat within the droplet on which the ice could form preferentially. Yes, we make the silent restriction on this widely accepted and precisely described homogeneous nucleation mechanism (Th. Peter, personal communication), and do not consider alternatives. We have changed "formation" for "nucleation".

p 5885 line 12: we have removed this reference.

Fig 4 caption: you unwittingly pinpointed to a further oversight on our side in estimating the mean pressure on the Z-level. Threshold temperatures are now indicated and our inattention resolved.

p 5888 lines 16-17, 23-24, 25, 28: For consistency, we have removed the entire paragraph and (pseudo-) momentum flux is not a concern in the paper anymore. Still, we think that the inclusion of rotation in the vertical component of the EP flux will not affect the sign of it. The vertical component of the EP flux, 'momentum flux', reads EPz=-<u'w'>+f<v'theta'>/<theta>_z and can be rewritten in terms of wave parameters EPz=-<u'w'>(1-fˆ2/om_0ˆ2) (Fritts et Vincent, Mesospheric momentum flux studies at Adelaide, Australia - Observations and a gravity wave - tidal interaction - model, JAS, 44(3), 605-619, 1987). The rotational term fˆ2/om_0ˆ2 is always smaller than 1 for inertio gravity waves and therefore, our conclusions about the change of sign of the momentum flux would have been valid regardless of including or not the rotational effects.

p 5889 line 9: You will see the opposite tilt in the new Fig. 12. According to the ray simulation, we do not expect to see the oppositely tilted wave crests in the vertical plane defined by the DC8 path. However, in the vertical section defined by the ER2 path, which is much more to the south, opposite tilted divergence patterns might be

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guessed (see especially Fig. 8, lower panel, -45 longitude, 500 hPa; not that far in time and space from where the ray simulations evaluated the wave emission). Whether this wave was emitted simultaneously than 'wave DC8' remains an open question. We have changed our formulation.

Fig 7 caption: with pleasure.

from p 5889 line 20ff to p 5892 line 27 (all remarks concerning ray-tracing): The entire section on GW ray tracing has been reorganized, and rewritten, we think with benefit. As mentioned before, a figure has also been added. In particular, your Richardson number input has been integrated, and all your (pertinent) remarks have been included. We kept the title GW ray tracing as this section is general, and for the discussion of our simulation we intended to describe the physical rather than the computational picture and carefully checked our wording.

p 5893 line 8(a): Yes of course, we neglected this point in a not justifiable manner. This point has been clarified in the text.

p 5893 line 8(b): We don't really get your argument. Did you understand us? Our point was to say, if the wave wouldn't change, then (at least NAT) clouds could be expected. We have tried to clarify the text.

GL has been replaced with Greenland and BSR with backscatter ratio: a truly pleasure. And the double formulations have all been rewritten explicitly.

On behalf of all coauthors

Sandro Buss

Interactive comment on Atmos. Chem. Phys. Discuss., 3, 5875, 2003.

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Interactive Comment