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Interactive Comment

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.

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

Major points:

1) We have taken this point into account by answering your minor points 18) and 20).

2) The validity of the ray tracing techniques is not restricted to stationary environments (Andrews, Holton and Leovy, Middle atmosphere dynamics, Academic Press, San Diego, 489pp, 1987, Appendix 4A) but the underlying assumption is that the background flow must vary in space and time less than the wave packet i.e. its wave lengths and period (see Andrews et al, 1987). This WKB assumption is necessary for the derivation of the ray-tracing equations (Andrews et al., 1987). To verify the validity of this assumption Marks and Eckermann (1995) compute a measure of the local rate of

variation of the zonal (alpha), meridional (beta), vertical (gamma) wavenumbers and intrinsic frequency (delta). These parameters were also computed in our simulation, they are order 0.1 in the stratosphere and ˜1 in the vicinity of the jet. Therefore, we regarded this assumption as valid. Section 5.3 has been reorganized with inclusion of the above explanations. To address the question of instationarity we changed figure 6.

3) About the sentence in the previous version of our conclusions "The issue that remains ...": We agree, that this sentence was confusing and we changed the paragraph. In our simulation, the simulated wave (that we conclude to be emitted from the tropopause region) does account for the 8K mesoscale cooling. What remains an open question is whether in reality small-scale non-hydrostatic gravity waves (that can not be captured by our hydrostatic model simulation) further modify the temperature field and the internal cloud structure.

Minor points:

1) The restrictive adjective has been removed.

2) The range of the specific values of published observed/simulated jet stream-induced GW have been added. Short comparison with own values is provided as a response to this point and your point 16) in section 5.3.1.

3) The quotation has been corrected.

4) As stated in the text, the smoothing reduces (and not suppresses) the precarious waves with wavelength close to twice the mesh width. Our goal thereby is to avoid numerical problems. Furthermore as shown by the additional panel in Fig. 1, by filtering the orography, we removed the little mountain just beneath the observed ice cloud, which we first thought was responsible for the GW.

5) This is a fair point, our DeltaZ is at at the upper limit. Still, due to computational limits we cannot increase the vertical resolution without lowering the uppermost level, or reducing the integration domain or period. We do acknowledge that our vertical

resolution is at the lower limit and this is now stated in the text. Remark however that it appears to be sufficient for obtaining a realistic temperature signal.

6) We wanted to say disposition, and this has been changed in the text. Thanks.

7) The underlying orography with several smoothings have been added. Mention of it was included in the paper.

8) Our view is that there might be effects of interaction of resolved (in our model) inertia gravity waves and (unresolved) smaller-scale waves (see also our response to your major point 3). However, with the larger waves alone we get the cooling required for the formation of the observed ice cloud. In this sense, our study investigates the dominant contributions of the total wave spectrum.

9) This means that the wave is quasi-stationary during the short time period it takes an air-parcel to flow across the wave-induced cold region. Mathematically, it means that the wave frequency is smaller than 2*pi*U/L, where U is the velocity at the level of the PSC and L its diameter. For our wave this is indeed the case: omega=3.3*f and 2*pi*U/L is about 12*f. The first sentence was included in the manuscript.

10) "monthes" has been replaced by "hours". Thanks.

11) A legend has been introduced in the figures indicating the correspondence of the length of the arrows to the wind velocity in m/s.

12) A remark has been added in your sense in the text.

13) Figure 6 (and therefore Figure 4) have been changed in your sense. And the discussion in the section "Wind profile" has been rewritten according to the new figure and your point 18). A cross reference to the new figure has been added in the section "Synoptic situation".

14) The caption should be all right now.

15) The point has been clarified.

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16) A cross reference has been added to the quantitative discussion of the wave parameters. And these are compared with the previous literature in section 5.3.1.

17) Text changed. Thank you

18) We hope you have less doubts about the interpolated wind profiles (that are shown now instead of the averaged wind profiles shown previously). Our opinion is that we can rely on ECMWF data certainly above of the PBL, especially now that they are not smoothed by taking the average! The discussion in Section 5.1 has been rewritten including your remarks.

19) "quasi" has been removed.

20) We agree with the possibility you are mentioning. However, in the present case this does not seem to be the case. We infer this from an systematic analysis of a whole set of vertical cross-section (at different times and locations) to validate that the divergence field shown is not simply a remnant of a wave excited hours before.

21) We have substantially reformulated and reorganized section 5.3 to improve the discussion of the ray tracing technique and its limitations.

22) In our sense, the vertical wavelength compares well with previous studies (see e.g. Hitchman et al (2003)). This is now mentioned in the text, Section 5.3.1. Further, this high vertical wavelength might be due to propagation/refraction effects because (according to the results from the ray tracing) a few kilometers below the cloud the vertical wavelength was in the range 4-6 km.

23) Figure 9 displays a vertical sounding through the HRM model atmosphere at the time of observation of PSC II. The divergence signature (Figure 7) and the ray-trace (Figure 11) suggest that the wave propagation has an important horizontal component. It is therefore not astonishing that in the vertical sounding the phase relation does not extend far below the observation.

24) You are right, we have changed to Ric=1. Thanks.

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25) We do agree and omited the term ’climatology’.

On behalf of all coauthors

Sandro Buss

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