

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

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My apologies for letting the deadline creep up on me (along with others). I have not found time to investigate this manuscript with proper thoroughness, and have very little to add to Dr Doernbrack’s extensive comments, apart from a few nit-picks. More importantly, I agree with his implicit verdict that the paper should be published after some careful revisions and – I hope – at least one model run at higher resolution, even if only to check the stability of the larger cloud structures under resolution increase. I share the concerns about whether model resolution is adequate. (But I applaud the other sensitivity experiments; and at least a resolution reduction was tried.)

The topic of the paper and this particular case study is undoubtedly interesting, both from a chemical and even more from a dynamical viewpoint. It cannot be too strongly

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emphasized that the spontaneous generation of GW either by instability, or by adjustment – or perhaps, in some cases, by some combination of those two types of process – is an area where understanding is sorely lacking, or completely absent. I think the case made that the "DC8" wave is non-orographic seems on balance to be persuasive.

Of course I agree with Dr Doernbrack that the writing should be more explicit, e.g. mentioning wavelength, frequency and amplitude values, arrow lengthscales in figures, longitude numbers (e.g. Fig 3) etc – just a standard matter of applying lucidity principles! (One of those principles is, of course, that the author or speaker needs to be about twice as explicit as he/she feels to be necessary. I wish this elementary point were more widely understood.) (To paraphrase the great mathematician J.E. Littlewood, "one trivium omitted is an annoyance, two trivia an impasse!")

For instance it isn't clear from my reading whether, in the ray tracing, the rates of change of background quantities were taken into account ("time-dependent Hamiltonian"). If not, I would share Dr Doernbrack's concern about using ray-tracing that assumes a steady background. The ray-tracing equations are not explicitly shown; so the reader has a hard time guessing precisely which version of those equations was used.

(To compound the problem, the typing is a bit careless, as with "let" for "led", "stays for stands", "hydrostatic" for "non-hydrostatic" (p.5891 line 6), etc.)

In the abstract: "geostrophic adjustment of a jet instability": does this make sense? So-called "geostrophic" (actually ageostrophic) adjustment is one thing; instability is another. There seems to be a tendency throughout the paper to conflate instability with adjustment, perhaps because both happen to be favoured by low Richardson number. For an up-to-date discussion of adjustment, see the article on "balanced flow" in the new Encyclopedia of Atmospheric Science (Vol 2, p 680), or [www.atm.damtp.cam.ac.uk/people/mem/papers/ENCYC/](http://www.atm.damtp.cam.ac.uk/people/mem/papers/ENCYC/) – there is no necessary connection with instability, but rather with vortical-flow unsteadiness, in a subtle and poorly-understood sense.

Further points that deserve attention, in order of occurrence:

p 5876 line 24: (Introduction). "several hundred... molecules" makes no sense. Do the authors intend to say that a single Cl atom can catalytically destroy "several hundred ozone molecules"?

p 5878 line 6: Non-sequitur that "GW can be ruled out" (just because it's high latitude winter). Phenomena like the polar lows of the Norwegian Sea disprove this: as with wintertime explosive marine cyclogenesis, deep convection significantly changes PV distributions. Perhaps the authors can assure us that their case doesn't involve such cyclogenesis, even though it's possible under some high-latitude wintertime circumstances.

p 5878 line 20: "in the power law range". Please be explicit: power of what? Rough numerical values? Is this "range" comparable to that in the Uccellini-Koch and Fritts-Nastrom cases? If not, what is the point of comparing and contrasting those cases with those of Nastrom et al?

p 5878 line 22: The "life cycle" referred to by O'Sullivan and Dunkerton isn't that of the GW. It's the life cycle of the initially balanced baroclinic wave, which they claim generates GW by spontaneous (ageostrophic) adjustment.

p 5878 line 25: "such an instability". The Sutherland and Peltier study was about GW generated by Kelvin-Helmholtz (KH) instability (though not a very realistic case), as distinct from adjustment. The Scinocca and Ford work cited describes a KH scenario that tries to get somewhat closer to realistic parameter conditions (implying GW wavelengths several times greater than KH wavelengths).

p 5882 line 2: high static stability (AND vertical shear).

p 5883 line 1: I'm no cloud physicist, but if "homogeneous" means what it seems to (homogeneous nucleation), are the authors forgetting the possibility of Junge-layer condensation nuclei? Have they checked whether the cloud is above the top of the

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Junge layer?

p 5885 line 12: "decreasing dynamic stability"? Inertial stability? KH stability? Other? (EXplicitness again....)

Fig 4 caption: Green stands for  $T_{ice}$  (= ? in degrees K?), blue... $T_{nat}$  (NAT?) (= ? in degrees K?) The implicit assumption that printers and computer screens can distinguish the blue/green boundary is quite wrong, even for readers with normal colour vision! (EXplicitness principle again...)

p 5888 lines 16-17: "the" vertical eddy momentum fluxes (not shown). EXplicitness principle again: this time there's a serious ambiguity. According to wave-mean theory this should mean something like the Eliassen-Palm flux, including the rotational term, but the phrase "momentum flux" is often used loosely to mean the non-rotating approximation, the Reynolds stress, neglecting the rotational term. The rotational term should of course be included if the rotational dispersion relation (2) is relevant.

p 5888 lines 23-24: "The intrinsic frequency... goes to zero." This makes sense only if rotation is neglected (yet on 5891 line 2 we are told that the intrinsic frequency is only about 3 times  $f$ ). In any case, critical level or levels, if any, for a non-orographic wave will be different from those for an orographic wave since the apparent frequency or frequencies will be different.

p 5888 line 25: "the momentum transported... vanishes". Is this a slip for "the momentum flux [in some sense] of the wave vanishes..."? (The second statement would be closer to being correct than the first. At least it could be correct in a large-Richardson-number background, sufficiently close to the critical layer.)

If, on the other hand, the first statement means the pseudomomentum of a wave packet – this could make sense, since pseudomomentum is a wave property, though momentum isn't – then the first statement would be untrue until wave breaking or other wave dissipation takes hold. Within linear, nondissipative, large-Richardson-number wave

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theory, the pseudomomentum of a wave packet is constant as the packet approaches its critical level.

p 5888 line 28: Coherent-ordering issue (premature statement)? The basis for suggesting a source around 300hPa for "wave DC8" cannot yet be evident to the reader. We haven't been told either the sign and sense of the momentum flux, nor the orientation of the horizontal wavenumber. And frustratingly, the 500-300hPa layer in Fig 7 seems to show no visible signal.)

p 5889 line 9: "origin is situated around 300 hPa". Again, if only I could see oppositely tilted wavecrests between 300 and 500 hPa then the whole thing would become clear, including the point about the momentum-flux sign change mentioned earlier.

Fig 7 caption: I'd guess that the lower solid curve is the (highly smoothed, hence extended) Greenland topography; but can't I be told explicitly?

p 5889 lines 20ff, Section 5.3: Are the authors aware that the ray-tracing theory requires a large-Richardson-number background? This comment connects with some remarks above.

Also, I find it disturbing that "initial" always means "final" or "terminal" in this section. I can understand how things look from a computational viewpoint, but as the reader trying to get hold of the physical picture I have to keep telling myself that "initial" really means "terminal". So (even if the whole text isnt rewritten to give the physical picture more plainly) couldn't there at least be a few more warnings, e.g. in the section heading, "Backward GW ray tracing", and, on p 5889 line 24 and also much later on p 5892 line 2, "the initial (= terminal) wave specifications". And a lucid repetition of the phrase "backward ray trajectories" would help on page 5892 line 3, as is already done on page 5892 line 19 (apart from the gratuitous variation "wave" for "ray"). There is also "until 13 January" on p 5892 line 20 — deeply confusing. I don't think there an English word for "backward-until". Then on line 22 we suddenly have "comes from" instead of "goes to": now the authors are confused (dare I suggest it) as well as the reader! (Then it's

back to "descends" rather than "ascends" on p 5892 line 25.)

I wonder whether it wouldn't truly be better to keep the "comes from", and to rewrite the remainder of the section with time running forward as in real life!

p 5890 line 16: "it". Coherent ordering again: firstly, it would help the reader if "it" were replaced by capital Theta, as well as inserting Theta after "propagation angle". Secondly, the 180 degree ambiguity needs explaining here, especially since the reader will understand from  $\Theta = 7$  degrees (line 22) that this is an eastward propagating wave, only to find out on the next page (p 5891 lines 20-22) that it's westward after all.

p 5891 line 6: NON-hydrostatic (as already noted). Is this mere carelessness? Or should the reader begin to wonder whether or not there's a close relation between what's printed and the actual ray-tracing code? (Recall the other relevant remark: do the ray-tracing equations actually solved allow for background time-variation or not?)

p 5892 line 2: "In ray-tracing calculations, the major source of error...?" Should I believe such a sweeping statement? For any length of ray path? Even in the present case, are background Richardson numbers really large enough for other kinds of error to be confidently dismissed? Is the background really sufficiently slowly-varying?

p 5892 line 27: The authors mean "APPARENT [and of course spurious] change of sign in the action density"; it's the ray Jacobian that changes sign, not the action density itself.

p 5893 line 8: Spurious comparison: it's not temperature that's comparable, but temperature minus frost-point temperature. (There is a considerable pressure change between 23 and 27 km.) Also, wave-induced temperature fluctuations  $T'$  are unlikely to be comparable, with such a large change in wave parameters.

I'd respectfully suggest global replacement of "GL" by "Greenland" and of "BSR" by "backscatter ratio". A few seconds' work, saving many minutes of reader's time! (Think about lucidity principles – making superficial patterns consistent with deeper patterns,

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avoiding what psychologists call `Stroop interference"', etc. Think about the sheer horror of phrases like "the HML GL GW BSR simulation".)

While at this level (please forgive me), consider also the still-fashionable construction:

I like (dislike) clarity (confusion) in the ways of using parentheses,

or

Turn left (right) at the first (second) intersection.

Now try reading page 5891 lines 18-19 from cold. What's wrong with "Turn left at the first intersection, and right at the second", or "For  $k < 0$  the calculated apparent period is....., and for  $k > 0$  it is..."? See also, e.g., p 5892 lines 9-10. What's so terrible about writing the more lucid version, "the zonal wavelength varies between 245km and 685km, and the meridional between 1280km and 12875km"?

At the risk of boring everyone, here's a little spoof that really ought to go on the record. Again please forgive me:

I must confess to disliking (liking) the convention of using parentheses (not using parentheses) to indicate alternatives. The space saved (taken) does not (does) seem to be worth the way in which the busy reader is slowed down (helped on his or her way). The problem tends to be compounded (simplified) when the sentences get too long (are kept short). Quite often one can be almost as succinct, and certainly more lucid, by stating one of the alternatives and then using a phrase like `vice versa'.

Even in a simple case like `In the northern hemisphere, anticyclones (cyclones) rotate anticlockwise (clockwise)' I would dare to suggest that it's better to write `In the northern hemisphere, anticyclones rotate clockwise and cyclones anticlockwise'.

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