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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Impact of upper-level jet-generated inertia-gravity waves on surface wind and precipitation" *by* C. Zülicke and D. H. W. Peters

Anonymous Referee #3

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The paper describes a case study of convection ahead of a cold front over the Baltic Sea and generation of inertia-gravity waves in a jet exit region aloft. The authors carry out mesoscale numerical simulations and show, using comparisons with several datasets of observations or analyses, that these simulations describe satisfactorily mesoscale features ahead of the cold fronts. The authors attempt to show that there is an interaction between the inertia-gravity waves generated by the jet in the upper-troposphere and the bands of convection seen ahead of the cold front at the surface. The problem is of interest but there is no conclusive evidence shown for a clear interaction between the upper-level jet-generated waves and the convection developing ahead of the surface cold front. Furthermore, some essential issues are not properly discussed. Unless the major objections below are convincingly addressed, I do not believe the paper can be accepted for publication.

Major Comments:

1- A first main objection is that a discussion on how jet-generated inertia-gravity wave may influence convection near the surface is absent. Yet it is a priori difficult for such waves to play a significant role, for several reasons: - jet-generated inertia-gravity waves generally have low frequencies, and hence their signature on vertical motions is relatively weak; - among the studies describing generation of inertia-gravity waves from upper-level jets, many have shown a clear signature of the waves above the jet in the lower stratosphere, but only a few have shown a clear signature of the waves propagating down from the jet. Evidently, two factors at least will make the signature of the IGWs less clear in the troposphere: the background flow is more complex, and the density is increasing as we propagate down. Now, IGWs will contribute to the initiation of convection if they induce sufficient vertical displacements of moist air near the surface. For instance, the impacts of high-frequency waves generated by the convection itself has been described in Lane and Reeder, (JAS 2001), using calculations of CAPE and CIN to discuss quantitatively the role of the waves.

To show that the jet-generated IGWs can indeed influence the initiation of convection ahead of the cold front, the authors need to show convincingly that they can induce significant vertical motions near the surface. This is a priori not evident.

2- The second main objection is that the reasonning of the authors seems flawed: the authors show that IGWs are generated aloft by the jet using dry simulations (Fig 3a, c, d). They also show that both their 'full' simulations and the observations contain oscillations in wind at the surface (Fig 4). The suggestion is then that the oscillations of wind near the surface are due to the IGWs, that those are causing the convection, and hence that the jet-generated IGWs have played a role in convection initiation. However, the wind oscillations could also be attributed to the convection events themselves (wind

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gusts below the convection), in which case the presence of these oscillations is in no way indicative of an interaction with the jet-generated IGWs. What is lacking is clear evidence that the jet-generated IGWs can have such a signature.

Now, the authors have run DRY simulations to try to isolate the jet-generated IGWs and their effects. In the cross-sections showing the divergence (Fig 3a) it is difficult to identify any tropospheric signal that would be a clear signature of downward propagating IGWs (see point 3 below). In the cross-sections supposedly showing the effect of these IGWs on stratification and vertical motions (3c and d), there is no clear evidence of fluctuations on the scale of the IGWs. These figures on the contrary suggest that there is evidence that the jet-generated waves do not have any meaningful impact near the surface. Better evidence of the interaction of the jet-generated IGWs and the convection should be provided, or the authors should completely revise their conclusions.

3- The figures are often covered with thick lines supposed to indicate phase lines of the IGWs or other features. These thick lines are sometimes justified (e.g. fig 2e) but they are then superfluous because the phase lines are already quite evident. In other cases, they are rather suspicious (Fig 2f in the troposphere) or worse (Fig 3a and b): they obscure the figure and suggests pattern where apparently there are none. It would be preferable that the authors do not use such lines.

4- Parts of the manuscript are poorly organized, in particular section 2: the authors first have a section (2.1) on one set of analyses to be used (ECMWF analyses), then (2.2) a general description of the meteorological background, then (2.3) a (insufficient) description of the mesoscale numerical simulations, then another section (2.4) on another set of analyses, and several sections on data, two of which should be combined (2.5 and 2.6).

5- More precision needs to be given about the numerical simulations carried out (e.g. what upper boundary condition is used, at what height? This is only an example...)

6- The style can be significantly improved (e.g. p2 line 16: 'Data assimilation to circulation models is a tool to generate meteoroological fields over the ocean...')

Minor comments

p5 line 18: PVU = ??

p16, line 3: the waves have frequencies below f (subinertial)?

p17, then -> than

p17 line 26-27: 'No other station showed...' contradicts 'A similar time series...'

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 15873, 2007.

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