Report on Zuzana Prochazkova’s Master thesis

entitled

On the internal gravity wave-atmospheric circulation interaction

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General comment:

Isolating the gravity wave (GW) component of the flow is an important issue for GW studies. Recent works have emphasized the associated uncertainties for the analysis of both observational and model data. The present thesis addresses this complex problem in high-resolution numerical simulations of the flow over topography (the Andes, Antarctic peninsula, and South-Atlantic islands).

The manuscript is divided into 3 sections which provide a general presentation of atmospheric gravity waves, a more focused description of the methods used in the manuscript and some applications to the aforementioned high-resolution simulations. Overall, the manuscript is well-written and the introduction and presentation of the results are appropriate. However, the presentation could certainly have been more synthetic: for example, there is some redundancy between the general presentation and the method section. Some statements are somewhat approximate and some analyses could have been more thorough (energy spectra), but this is to be expected for a master thesis. The results section, although showing still on-going work and reflection, is already interesting. The ensemble makes a honorable master thesis.

Despite this positive general impression, I have some specific critical comments/questions regarding the structure of the manuscript, in particular the introduction and method sections (detailed below). In general, I appreciate the efforts of the candidate to rederive fundamental relations, but again this could have been more concise and there are also slight inconsistencies between sections. On the other hand, the description of the analyzed simulations could have been more detailed. It is currently lacking important information regarding the damping layer (sponge layer) and the vertical resolution of the simulation. In the case studies as well, more information on the background flow would have been welcome.

Finally, the title could have been more specific, since the manuscript mainly deals with extracting gravity waves from the background flow. It would in my opinion be more appropriately entitled : “On methods to isolate gravity waves from the background flow”.

Detailed comments/questions:

1) The equations for the background flow proposed in system (1.9) on page 8 include inertial oscillations (and time derivatives) but not the stationary geostrophic flow (since there is no pressure/temperature gradient). This is not standard and in my opinion partly inconsistent with what is done later in Sect. 2.2 . Am I missing something here ? Could the candidate explain ?

2) The linearisation is done around a vertically sheared background flow in equations (1.14), (1.15) but this term is dropped (or set to zero) in the matrix system (1.26). This should be stated.

3) It seems that the candidate only considers kinetic energy for the horizontal spectra and potential energy for the vertical ones. She could have examined the consistency between the two in light of GW theory (e.g., polarization relations).

4) on p32 : “create a regular grid defined by values of latitude and longitude”. Do you use a local tangent plane for the different subdomains to account for the varying latitude and hence “x” resolution ?

5) p42 figure 3.3 : I am surprised by the striking disagreement between NFFT and FFT, which do not exhibit the same slope. The NFFT seems wrong to me, do you have another explanation ? Also, I understand that you resample vertically every 1 km, then the Nyquist wavenumber should be 0.5 [km-1] instead of 1[km-1] here ?

6) p48: Just a comment that I like the idea of using the actual spectrum to adjust the cut-off.

7) p55-56: On the figure, the potential energy spectrum lies above the saturation curve. You mention that there might be an incorrect scaling, but could that suggest that the short vertical-scale waves are unstable and undergoing dissipation ? A vertical profile of momentum flux at that time would have been helpful to judge that.

9) P57 : You attribute the increase of divergence forcing amplitude with altitude to a possible contribution of secondary generated GWs. Is it not just caused by the decrease in density ? Maybe this factor is already included ? Please elaborate.

10) p57: What do you mean by ‘The attenuation of GWs’ structure in the altitude range 10 – 25 km can explain the specific shape of the vertical spectrum, since the spectrum is computed using exactly this altitude range’?

11) p57: you mention that small-scale waves dominate divergence while large scale waves dominate the wind spectrum. This is what one would naturally expect, since the derivative in the divergence emphasizes smaller scales compared to the wind. Is there more to it than that ?

Typos in the manuscript:

p14 : variance of the Gaussian function → standard deviation of the Gaussian function

p15 : Omega (the angular velocity of the earth) is 2 pi/ 24 hours, not 1/24 hours

p16 : in Eq. 2.14, Omega is a vector and should be in bold font.

P25: There is a missing mass/density factor for Ez to be kinetic energy.

P61: global circulation models → general circulation models

General questions to the candidate :

1) on p40 : “However, the rotational component of energy connected probably mainly with geostrophic modes [5] is present in the full range of wavelengths and, as expected, it dominates the spectrum for wavelenghts longer than about 1000 km.” Does the candidate mean that internal gravity waves have no signature in rotational energy ? This is not true, they are often referred to as inertia-gravity waves. Please correct/clarify and comment.

2) on p 12-13: For illustration, positive k, l, m and omega are taken. To what direction of vertical energy propagation does this correspond ? What is the orientation of the vertical momentum flux with respect to the horizontal wavevector for an upward propagating wave ?

3) I enjoyed the section describing the active wind decomposition. To what balance does Equation (2.17) reduce to for small Rossby numbers ?