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## ***Interactive comment on “Characteristics of gravity waves resolved by ECMWF” by P. Preusse et al.***

### **Anonymous Referee #1**

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The paper analyzes gravity waves in the ECMWF analyses, taking advantage of the high resolution of the latter. For the most part, the waves are analyzed using a sampling and methodology that mimicks what could be done with a high-resolution 3D limb sounder. Individual wave packets are identified and traced back to the lower stratosphere, to the troposphere or to the ground with a ray-tracer. Several issues are considered (comparison with observations and realism of the analyses, sources of the waves, importance of lateral propagation, high values of gravity wave momentum fluxes in the Southern high latitudes, intermittency of the momentum fluxes). The paper is very well written in general, the analysis is thorough and well discussed. There are certain parts which may be clarified, or which may be improved, but overall this is an excellent study, describing a large amount of work and discussing carefully a number of relevant issues. I have only two concerns, and recommend publication once these are addressed.

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## Major Point

1. In analyzing the waves in the ECMWF analyses, the authors are obtaining information about several things: part of the ECMWF waves are realistic, but part of the waves are also spurious regarding some of their characteristics. For example, the authors very clearly discuss the bias toward long wavelengths and low frequencies. Hence, the analysis sometimes teaches us something about the real GW field (for those aspects which are realistically described) and sometimes teaches us about the biases of the ECMWF model (which is not quite as interesting and concerns a smaller fraction of the readers). For instance, the discussion in section 6 or the comparison with HIRDLS (figure 9) identifies robust features of the GW field. In contrast, much of section 4.2 is based on backward trajectories of waves which have a bias toward long wavelengths and low frequencies. The emphasis on the large distances for horizontal propagation is here misleading: it tells something of the ECMWF model and its biases rather than about convectively generated waves. The separation between the two (information that is presumably robust for the real GW field, and information on the ECMWF model and its biases) is not always clear enough in the text (in particular in section 4.2), leading to a certain confusion.

2. A number of studies that have discussed the question of the realism of the gravity waves present in the ECMWF analyses are not cited. It would be better to include them and provide a fuller description of the context of this study, see below. In some other parts of the text, relevant references are suggested below.

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p11964, par 2: what exactly is meant by 'spontaneous adjustment' (there are no reference here to make the authors' meaning more precise)? Research on spontaneous emission has been motivated by the need to understand non-orographic gravity wave generation, in particular from fronts and jets. In studies following from O'Sullivan and Dunkerton (1995), investigating waves in baroclinic life cycles, the spontaneously gen-

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erated gravity waves that are studied come from the jet/front system that develops with baroclinic instability. Hence, contrasting gravity waves from fronts and 'spontaneously generated' waves is a bit confusing.

p11965, l15: give a precise date, in addition to 'at the time of writing'; the resolution has increased again since, to 137 levels I believe;

p11966, l6-7: earlier studies already showed the presence of gravity waves in the ECMWF analyses: although the ECMWF analyses had too coarse a resolution to resolve the waves, these studies showed that relevant information was included (location, orientation, intrinsic frequency) of the waves emitted by jets. Maps of the horizontal wind divergence calculated from the ECMWF displayed clearly identifiable wave patterns, consistent with those from different observational platforms. These earlier studies should be included in the discussion:

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Moldovan, H., Lott, F. and H. Teitelbaum, Wave breaking and critical levels for propagating inertio-gravity waves in the lower stratosphere, Quarterly Journal of the Royal Meteorological Society, 713-732, 2002.

R. Plougonven and H. Teitelbaum (2003), Comparison of a large-scale inertia-gravity wave as seen in the ECMWF analyses and from radiosondes. Geophysical Research Letters, 30(18), 1954.

A. Hertzog, C. Souprayen and A. Hauchecorne (2001), Observation and backward trajectory of an inertia-gravity wave in the lower stratosphere, Annales Geophysicae, 19, 1141-1155.

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p11966, l25: two other references that are missing in the present discussion: is a more recent study which used ECMWF analyses to investigate explicitly resolved gravity waves during a stratospheric sudden warming (Yamashita et al 2010). An effort was

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made to validate ECMWF GW with observations. The other precisely deals with the use of high-resolution NWP output to investigate GW, with a comparison of ECMWF analyses and satellite measurements over the Andes (Shutts & Vosper 2011).

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Yamashita, C., H.-L. Liu, and X. Chu, Gravity wave variations during the 2009 stratospheric sudden warming as revealed by ECMWF-T799 and observations, *Geophys. Res. Lett.*, 37, L22806, doi:10.1029/2010GL045437, 2010.

G.J. Shutts and S.B. Vosper, (2011): Stratospheric gravity waves revealed in NWP forecast models, *Quarterly J. Roy. Meteor. Soc.*, 137, 655, 303-317.

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Figure 4: panel b: red ellipses 1 and 2 are superfluous and make the figure a little more difficult to read. It may be best to remove them.

p11979-11978: the discussion on the LTA of rays corresponding to waves due to convection should be reduced, for two reasons: the main issue discussed is whether the resonant forcing mechanism or the moving mountain mechanism is more relevant. The authors emphasize that rays terminate above the troposphere rather than in the troposphere as one would expect from the resonant forcing mechanism. However, as the authors themselves emphasize, the mechanisms at play in the ECMWF are probably not the same as in the real atmosphere near convection. Secondly, it is not entirely clear how to interpret the altitude of a ray for a wave which has a vertical wavelength of 8 or 10 km, as is typical for waves from convection.

p11983, l22: origins -> originates in 'originates from convection'

p11984, lines 1-2: the statement concerning the overestimation due to the neglect of the factor  $(1-f^2/\omega^2)$  applies to those waves which are described in ECMWF. In reality, it is expected that the majority of the waves emanating from convection in the Tropics have high frequencies, for which this factor is very close to 1 (see for instance

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Jewtoukoff et al 2013 in the references, who use balloon measurements giving access to intrinsic frequencies). Hence for real waves the calculation of  $u'w'$  is very likely adequate, and the statement here applies is relevant to the analysis of those waves present in the ECMWF model, for which there is a bias toward low-frequency waves.

Figure 7: to make the figure more readable, one could suggest two changes: 1- keep the same aspect ratio for both sections (in the upper panel and in the lower panel). In consequence, the plots in the upper panel would be taller. 2- recall one or two relevant contours of the GWMF in the lower panel, for instance as a white line, so that the comparison between the two types of sections is easier.

p11985, end of section 4.2: the meaning of the authors is not completely clear here; they stress in preceding pages that for the Tropics it is consistent to consider convection as the main source of waves. Here they emphasize shear as playing a role, with reference to two studies (one for the Tropics, one for the extra-tropics) and with the support of figure 7. The correspondence between the patterns in  $Ri$  and in GWMF is not so clear in this latter figure, and the strong shear rather seems to be associated with a decrease of GWMF (dissipation of waves) rather than generation... I may be misunderstanding the whole point here.

p11990, l1-2: given the resolution of the ECMWF model, it is expected that the values for updrafts will be weak; intense updrafts of several m/s are found only on rather short spatial scales, and it is known that the vertical velocity field is one of the most sensitive to resolution. The discussion could be better formulated.

p11990, line 8: the authors should be more precise for 'long-term prediction'.. should this be seasonal prediction?

p11993, lines 10-28: this discussion on the intermittency of waves due to orography is very reminiscent of one in Plougonven et al, 2013, which described similarly the overwhelming contribution of individual wave episodes above orography (the Antarctic Peninsula) to a polar cap average of momentum fluxes. This contrasted with the more

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steady contribution from non-orographic events integrated over the Southern Ocean. Though not on a hemispheric scale (only poleward of 50 degrees), that discussion should be cited. This whole discussion relates to the importance of the intermittency of orographic waves, which Hertzog et al (2012) have proposed to quantify using the Probability Distribution Function of GWMF.

Hertzog, A., M. J. Alexander, and R. Plougonven On the intermittency of gravity wave momentum flux in the stratosphere, *J. Atmos. Sci*, 69, 3433-3348,2012.

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Interactive comment on *Atmos. Chem. Phys. Discuss.*, 14, 11961, 2014.

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