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Interactive comment on "Gravity-wave effects on tracer gases and stratospheric aerosol concentrations during the 2013 ChArMEx campaign" *by* Fabrice Chane Ming et al.

Anonymous Referee #3

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General Comment

In their paper, the authors address the effect of mesoscale gravity waves on the distribution of aerosol and ozone in the Mediterranean area during July 2013. Vertical profiles of balloon-borne Light Optical Aerosol Counter (LOAC), M10 meteorological global positioning system (GPS) sondes, ozonesondes and GPS radio occultations are analyzed to identify fluctuations caused by gravity waves (GWs). By applying different techniques GW characteristics are derived, and by backward ray-tracing a jet-front system is identified as the source of the observed GWs. Finally, the phase relationship between GW induced fluctuations in ozone, aerosol concentrations and wind perturba-

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tions is discussed.

The effect of gravity waves on trace gas or aerosol distributions is a topic that has seldomly been studied and is therefore of relevance for readers in the fields of both atmospheric chemistry and atmospheric physics. There are, however, several concerns, two of them major, that should be addressed before publication of the paper in ACP.

Major Comments

(MC1) Multiple waves are identified in Table 1. For the case discussed, this would be expected because there is a strong wind reversal at 18 km altitude, and the wave parameters in Table 1 show considerable spread.
Nevertheless, when reading the text, I had the impression that sometimes while writing, you were thinking of just one single wave being detected. Therefore it is not always very clear and/or consistent which of those waves you are discussing.

This is most evident in Sect. 5.3 where you attribute wave parameters over a large altitude range to just a single wave, which obviously is not correct.
In addition, the discussion of the backward ray traces also somehow suffers from this issue.

Regarding this issue, please carefully check the whole manuscript for clarity and consistency.

(MC2) For the three RS and RO temperature profiles presented, the phase difference method cannot be applied to extract wave properties because the time differences are way too large! Therefore I would suggest to either select other profiles, if possible, or to just drop the related discussion because it is not necessarily needed.

Please find more detailed comments below.

Detailed Comments

(1) p.2, l.30+31: here you write
"...are now capable to capture some characteristics of GWs using proper GW parameterization..."

This statement is somehow misleading. By parameterization only the "effect" of GWs is captured, not the "characteristics" because the waves are not resolved

Nevertheless, a certain portion of the GW spectrum will indeed be resolved by global models. However, by comparison with observations, it has been shown that the resolved GWs are usually under-represented (Schroeder et al., 2009). This is somehow mentioned later in the text in l.32–34, but should be more clearly stated.

Reference:
Schroeder, S., Preusse, P., Ern, M., and Riese, M.: Gravity waves resolved in ECMWF and measured by SABER, Geophys. Res. Lett., 36, L10805, doi:10.1029/2008GL037054, 2009.

(2) p.2, l.31+32:
Please check! I don't think Shutts and Vosper (2011) mention that convection would be resolved!
Just the opposite is true for the two NWP models they discuss. Convection in the MetOffice model and in the ECMWF IFS is parametrized (their Sects. 2.1 and 2.2), and in their Sect. 3.1, they state:
"Note that, since convection is parametrized in both models, it seems unlikely that the amplitude of these waves will be well represented.",
and later in their Sect.6:
"tropical gravity wave activity is under-represented in the model-derived gravity wave fields since parametrized convection is a rather ineffective forcing agency."

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(3) p.5, l.28 onward:
Since you are using RO (wet) temperatures at altitudes as low as 3km, are there significant differences between dry and wet temperatures?

In the lower troposphere the effect of water vapor on GPS bending angles becomes increasingly important. Therefore the bending angle signal caused by water vapor fluctuations could map into temperatures and could somehow bias the GW signal in the temperatures. (Of course, GW fluctuations in both wet and dry temperatures would be biased because usually for wet temperatures water vapor is taken just from reanalysis or climatology.)

(4) p.8, l.30 until end of Sect.3.3 — not entirely correct...
Horizontal and vertical wavelengths in Wang and Alexander (2010) or Faber et al. (2013) are global distributions of "average" values. Of course, their methods can also detect shorter scales, similar as in your study!

(5) p.8, l.26 until p.9, l.3:
For the three RS and RO temperature profiles presented, the phase difference method cannot be applied because the time differences are way too large! Therefore I would suggest to either select other profiles, if possible, or to drop this paragraph.
Still, the vertical wavelength information provided later in Fig.6 is quite useful.

(6) p.14, l.2: Here it is somehow unclear: Are you discussing just one dominant wave, or multiple waves, or different effects and one effect forgotten??!

waves are both filtered $\rightarrow$ the observed wave is filtered ??

Please clarify!

(7) caption of Fig.6 and elsewhere:
units like $C^2\,km^{-1}$ are not SI standard and even ambiguous, please either use

°C^2 km^{-1} and similar, which is still not SI standard but OK, or better use K^2 km^{-1} and similar, throughout

(8) p.15, l.20:
Here, it should be more emphasized that obviously different waves are detected.

(9) p.15, l.28: Is this a strong event? How does this momentum flux compare with other findings?

For example, you should mention that in summer midlatitudes GW momentum fluxes are usually low in the stratosphere, as indicated by satellite observations (for example, below around 0.001 Pa at 25 km over Europe as shown in Ern and Preusse, 2012). Your value of 0.05 m^2 s^{-2} at altitudes 13–20km corresponds to about 0.008 Pa which is well beyond this value, even taking into account that the altitudes are somewhat different.

Reference:
Ern, M., and Preusse, P.: Gravity wave momentum flux spectra observed from satellite in the summertime subtropics: Implications for global modeling, Geophys. Res. Lett., 39, L15810, doi:10.1029/2012GL052659, 2012.

During summer, GW momentum fluxes obtained from radiosondes over North America are about 0.02 m^2 s^{-2} around 17 km altitude (for example, Zhang et al., 2014). Usually, during summer momentum fluxes over North America are somewhat higher than over Europe. Nevertheless, your values over Europe are twice as high as those reported over North America. This further supports that your case represents a stronger GW event.

Reference:
Zhang, S. D., Huang, C. M., Huang, K. M., Yi, F., Zhang, Y. H., Gong, Y., and Gan, Q.: Spatial and seasonal variability of medium- and high-frequency gravity waves in the lower atmosphere revealed by US radiosonde data, Ann. Geophys., 32, 1129—1143, 2014.

(10) p.15, l.31: Here you state: "which means an excess of wave energy near the inertial frequency" — This statement is too strong and needs some explanation!

Hertzog et al. (2002) mention that p should be in the range of about 1–2 (theoretically about 5/3), and they find values in the range 1.5–2.2 for the "standard" power-law part of their observations.
Your values of 2.6 to 2.9 are only somewhat outside this range and not necessarily something special. Further, your statement is based on the observation of just a few waves, and your analysis involves a certain error range.

Please note that Hertzog et al. (2002) mention values exceeding p=5 as quite high and not fully understood. I am not sure whether they would be worried about values $p<3$. They suggest that exceedingly high values could be caused by enhancements of the velocity spectrum near the inertial frequency. Indeed, they find enhancements in two out of three balloon flights. These enhancements over the power-law spectrum are, however, a factor of TEN, which is far beyond your values.

Therefore you should add some explanation about the expected range of p, and you should use a weaker statement.

(11) p.16, l.12 until end of Sect. 5.2:
The time difference between the different soundings of the "triad" is too large for deriving horizontal wavelengths by the phase difference method.

Several criteria have to be matched to obtain useful information of the horizontal wave structure by applying the phase difference method. A more detailed discussion is given in Schmidt et al., 2016.

Reference:
Schmidt, T., Alexander, P., de la Torre, A.: Stratospheric gravity wave momentum flux from radio occultations, J. Geophys. Res. Atmos., doi:10.1002/2015JD024135, in print, 2016.

One of the preconditions is that there should be no significant phase progression of the wave due to its frequency. Otherwise phase differences are no longer dominated by the horizontal wave structure. Schmidt et al. recommend time differences of no more than 15 minutes. The three soundings you are using, however, are spread over 13 hours. This time span is much too long compared to the wave periods in your case, which are in the range 10 to 16.4 hours, as listed in your Table 1.

I would suggest to either select other profiles, if possible, or to delete this whole part of the subsection.

(12) p.17, Sect.5.3: Here you state:
"The spectral parameters (a vertical wavelength of 2.6 km,...) at heights of 12.8 km with a height range of 14 km are used to produce synthetic RS profiles with the signature of such a dominant mesoscale GW..."

Obviously, here you assume there is just one single dominant wave in a whole range of 14 km altitude, and for this wave you claim:
"Because of the variation of the Brunt-Väisälä frequency with height, the horizontal wavelength, computed using the GW dispersion relation, has values of 225.3 $\pm$22.6 km and 499.9 $\pm$45.7 km at heights of 3–7 km and 13–20 km respectively."

This kind of approach is not correct!
Variations of the BV frequency in vertical direction will NOT influence the HORIZONTAL wavelength! They will only have effect on the VERTICAL wavelength! See the refraction equations (Eq. 2.12) in Olbers (1981):

$dk_i/dt = -\partial\Omega/\partial x_i$

Reference:
Olbers, D. J.: The propagation of internal waves in a geostrophic current, J. Phys. Oceanogr., 11, 1224–1233, 1981.

Similar reasoning regarding the GW raytracing equations is found in Marks and Eckermann (1995), Appendix A:
dk/dt = ... $(N^2)_x$ ...
dl/dt = ... $(N^2)_y$ ...
dm/dt = ... $(N^2)_z$ ...
where the subscript means the gradient in the respective direction (x,y,z).

Changes in the horizontal wavenumbers are not related to vertical gradients in the BV frequency. Different from this, assuming just one wave with a fixed vertical wavelength in this whole altitude interval containing the tropopause with a strong jump in the BV frequency and strong changes in the zonal wind of more than 20 m/s may be over-simplified!

Differences (or even unexpectedly unchanged values!) in wave parameters listed in Table 1 should be caused by observing different waves at different altitudes.

Sect. 5.3 with the simulated profiles should therefore be revised!

(13) p.17, l.27 and later:
Same issue: for a given GW, the horizontal wavelength should not change much.

Therefore statements like:
"The horizontal wavelength (period) decreases (increases) slowly from 440 km (10 h) at 17 km heights to 290 km (14 h) at 10 km heights."
need further explanation.

Is the wavelength mentioned here the wavelength of the GROGRAT component with maximum amplitude at a given place and time?

What does this change in wavelength mean? Is this just an effect of the raytracing technique and the selection of input parameters, or do you think this provides information about the GW distribution for the case discussed, for example where waves with a given combination of wave parameters could preferentially be observed?

As far as I understand, the main finding of your simulation is that almost all rays can be attributed to the frontal source, even if the exact wave parameters are not known. Therefore it should be more emphasized that waves with a whole range of parameters could be excited by the front and propagate to the location of Ile du Levant.

This part of Sect. 5.3 should also be revised accordingly.

(14) p.21, l.10/11: Again, here you write: “when a mesoscale inertia GW produced by the jet-front system was identified during a jet-streak event”

Again, it is misleading to talk of just a single wave! From your analysis, it rather looks like different waves are seen at different altitudes, or even at the same altitude.

Other Comments

- p.2, l.4+5:
  European Center for Medium range Weather Forecasting (ECMWF) → European Centre for Medium-Range Weather Forecasts (ECMWF)
- p.2, l.7: oscillations → oscillations of
- p.2, l.19+20:
  for momentum transport and deposition → by momentum transport and momentum deposition
- p.3, l.25: The microphysical → Their microphysical ??
- p.4, l.12: Results on → Results of
- p.4, l.22: The campaign occurred → The campaign was carried out

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- p.5, l.4: 3000 particles → 3000 particles cm^{-3} ??
- p.6, l.27: of iterated so that → of iterations such that ???
- p.12, l.13-16: Please check!
  According to the caption of Fig. 3, the aerosol class shown in Fig. 3 is 0.2–0.7 μm, and not 0.2-50 μm as stated in the manuscript on p. 12.
- p.13, l.21: enhances ozone peak → would enhance the ozone peak
- p.15, l.6: Figure 6b indicates that spectral → Figure 6b indicates spectral
- p.15, l.27: 16 J.kg^{-1} → 16 J kg^{-1}
- p.21, l.22: lead → leads
- caption of Fig. 8: It looks like for (a) blue and green lines are interchanged in the caption. Please check!

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