Dear Dr Lahoz,

Please find below the reply to both reviewer comments.

We want to thank both reviewers for their general favorable comments and their numerous suggestions, which help to strengthen the paper. We thank reviewer 1 very much for bringing to our attention a number of references which are highly relevant for this paper. We thank the second reviewer for suggestions to the presentation of the material. In reply to his/her questions we have included two appendices showing the quality of the data and demonstrating the effect of the observational filter.

A detailed reply to all reviewer comments is given below. In the reply the original comment is given in bold face, the reply in normal style and excerpts from the modified paper in italics. New text passages in the modified paper are marked by blue color. This includes a few minor corrections made while rereading the paper for consistency after including the requested changes.

With best regards

Peter Preusse

We highly thank the reviewer for pointing out a larger number of references with relevance for this study. Including these references in the study will strengthen the case of the paper. In particular the paper of Hertzog et al. (2001) provided some (for me) new ideas for this study and will probably lead to a follow-on study.

The reviewer has two major points:

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 is biases) is not always clear enough in the text (in particular in section 4.2), leading to a certain confusion.
We thank the reviewer for pointing this out. It is very important to make clear in section 4.2 that we are discussing features of GWs resolved by the ECMWF model. We have accordingly modified this section and inserted at several points such specifications. (Please see the modified text.)
We also agree with the reviewer that the properties of the waves discussed must not be mistaken as the typical behavior of tropical waves in nature. However, we disagree with the view that these GWs are non-realistic per se. As the in situ observations by Leena et al. (2010) show such waves exist in nature. Also the spectra presented in Fig. 8 (prev. Fig 9) show that some long-horizontal wavelength GWs really exist. Furthermore there is more indirect evidence from the spatial distribution of satellite data. Therefore, also in this case we can actually learn from ECMWF about nature, but the fact remains that a more important part of the spectrum is missing.
We therefore conclude section 4.2 now by:
In summary, all evidence presented in this subsection is pointing to the fact that the majority of tropical GWs in the ECMWF model are excited above the convection but not in the convection. As discussed above, this is also the altitude of strongest wind shear where the Richardson number minimizes. This indicates that both wind shear and convection underneath are required for the forcing of the low-latitude GWs in ECMWF, which have very

long horizontal wavelengths and comparably low frequencies. In situ observations provide evidence that such GWs also exist in nature. However, whether they are representative for the low-latitude regions must be decided from global observations.
In fact, they are realistic but not representative, but this is explained later in the following section.
In order to make readers aware of these points at the beginning of the summary we will introduce after the first sentence:
By this analysis we infer properties and sources of GWs resolved by the ECMWF model. Where ECMWF-resolved GWs are realistic, this also provides valuable insight for GWs in nature.

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.
Again, thank you very much for the suggestion of additional references. Please refer to the reply to the specific comments for how these were integrated in the text.
We are particularly grateful for pointing out the backtracing study of Hertzog et al. (2001). We have tested this for the most strong event over northern Norway and included a few sentences in this paper. In future we may search peaks in several parameters in the backtraces in order to infer the true source height for case of spontaneous imbalance rather than using LTA.
The new included text reads:
Following Hertzog et al. (2001) we considered the wave parameters along the backward trajectory of these largest events "observed" over northern Norway at 25 km altitude. In several parameters, that is vertical wavelength, GWMF and WKB parameter (Marks and Eckermann, 1995) we find a maximum in the altitude range 4-7 km. Also the wave attains a much slower vertical group velocity below this altitude. Hertzog et al. (2001) interpret this as evidence that the "true" source of the wave is close to the altitude of this peak, i.e. in our case around 5 km in the mid to upper troposphere. Further evidence is that below 5 km the horizontal wavelength decreases and assumes a value of less than 200 km at 4 km altitude and of only 100 km close to the ground. This is below the resolution of the model, i.e. the wave could not have existed at altitudes below ∼4 km and must be generated above in ECMWF.
The waves with ray origins over the open sea are clearly related to the approaching storm system and hence related to excitation by jets and fronts as described by Plougonven and Zhang (2014) and briefly mentioned in the introduction. The case is very similar to the one described by Hertzog et al. (2001) and spontaneous adjustment is the most likely source process. Because the true source is at a higher altitude also the location is not identical with the ray origins shown in Fig. 4b but closer to the Norwegian coast. In this region we find coherent wave crests. While this is clearly not a mountain wave, the orography of the Norwegian alps may play an indirect role in the generation of the wave.

The generation of GWs by storms merits further consideration. In particular, implementing an algorithm identifying automatically peaks in the ray-traced parameters, one may actually infer in a systematical way the true source location instead of the location of the ray termination. This could also provide further valuable input to the investigation of the storm system. This, however, is beyond the scope of this paper.

Since a source altitude of 5 km for this case also somewhat altered the location of the ray origins we have deleted:

Gravity waves are more susceptible to wind filtering, if wave fronts and background wind are not perpendicular. The larger the angle between the wave vector of a GW and the wind is, the lesser is the likelihood for that GW to reach the stratosphere. Looking for sources of stratospheric waves, this explains why backtraces of GWs and, in particular of GWs with larger momentum flux, mainly point to regions where tropospheric patterns are almost perpendicular to the main wind, even though in these regions amplitudes in the divergence fields may be less pronounced than in other parts of the storm.

Specific comments:

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 generated 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.

The new version reads:

In replacing the standard non-orographic schemes in CCMs by physical sources, progress was made for GWs from convection (e.g., Beres et al., 2005; Song and Chun, 2008; Richter et al., 2010). An overview of GWs from jets and fronts was given recently by Plougonven and Zhang (2014). Several processes are involved in the generation of GWs by jets and fronts. First, convection associated with the fronts is an important mechanism of GW generation (e.g. Fovell et al., 1992). This may be covered by the convective parameterizations. Second, GWs may be generated by a cross-front circulation and resulting isentrope oscillations (e.g. Griffiths and Reeder, 1996; Reeder and Griffiths, 1996) motivating Charron and Manzini (2002) to launch GWs from fronts in cross-frontal direction. Third, GWs are generated in jet-exit regions which develop in baroclinic life cycles, as has been shown by O'Sullivan and Dunkerton (1995) and many follow-up studies (e.g. Plougonven et al., 2003; Zuelicke and Peters, 2006). Due to spontaneous adjustment (formerly called geostrophic adjustment) in consequence of baroclinic instability waves are emitted in the upper level jet. The wave vectors of these waves are pointing roughly in the direction of the wind at the source location though different directions may occur at the edge of the jet (O'Sullivan and Dunkerton, 1995). Furthermore, there can be a positive feedback be-

tween the waves and diabatic heating by precipitation as suggested by Uccellini and Koch (1987). Parametrizations for the latter processes are still at a very early stage and not yet applicable in GCMs. However, even for processes where source-based parametrizations are available, these parametrizations present new uncertainties:

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;

The text has been modified:

(at the time of writing the actual version is Cy40r1, which was implemented in November 2013 and has a resolution of T1279, L137).

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:

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.

We implemented these references:

Further papers discuss GWs from jets and fronts. For instance, Moldovan et al. (2002) and Plougonven and Teitelbaum (2003) investigate radiosonde measurements from the Fronts and Atlantic Storm-Track EXperiment (FASTEX; Joly et al., 1997). They find wave structures similar to those observed by radiosondes also in the ECMWF temperature and horizontal wind divergence fields. Hertzog et al. (2001) interpret lidar measurements of a GW by backward ray-tracing. They conclude that spontaneous adjustment close to tropopause altitudes is the most likely source. This is caused by baroclinic activity, similar as in the case of O'Sullivan and Dunkerton (1995). In the likely source region they also find GW signatures in horizontal wind divergence fields from ECMWF. Tateno and Sato (2008) investigate the source of two waves observed by the Shigaraki radar, also by ray-tracing. They found indication for GW excitation by spontaneous imbalance in the jet southward of the observation site and comparable waves in ECMWF.

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 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 and Vosper, 2011).

Thank you for pointing out these references. In particular Shutts and Vosper well prepare the issue of problems with convectively generated GWs. I should, however, note that I think that both papers show that GWs are underestimated in ECMWF and that the good agreement found is a bit fortuitous and partly a compensation of effects.

Variations of GW potential energy during the 2009 stratospheric sudden warming were investigated by Yamashita et al. (2010) on the basis of ECMWF global fields. In order to assess the realism of these variations the ECMWF data were compared to several-year climatologies of GW potential energy inferred from lidar data at Rothera and at the South Pole. In addition, GW potential energies from GPS radio occultations for the latitude range 65°N to 70°N are compared in a 30-day time series. In both cases the magnitude and temporal variations agree very well. However, the temporal removal of the background was based on a shorter integration time for the lidar, and for the GPS data the observational filter (Preusse et al., 2002; Lange and Jacobi, 2003) was not taken into account. This means that, if potential energy from ECMWF were inferred in the same way as in the observations, ECMWF would be lower and, as a consequence, this may indicate too low GW potential energy in ECMWF.

Shutts and Vosper (2011) find good correspondence between global distributions of GWMF from ECMWF and from HIRDLS observations (Alexander et al., 2008). Since Alexander et al. (2008) also does not correct for observational filter effects, this also is indication for some underestimation (for a detailed discussion of observational filter effects for GWMF from infrared limb sounding see Ern et al. (2004)). Furthermore, Shutts and Vosper (2011) note an underestimation of GWMF at low latitudes where convection is the most important source.

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.

Marking the discussed regions was explicitly requested by reviewer 2. We think these marks are indeed helpful for following the discussion and therefore we would like to retain the ellipses.

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.

The nature of tropical waves in reality is debated. There is evidence for different processes and for different scales. In particular, there is also evidence that those long waves found in ECMWF may exist in nature (cf. the quoted radar measurements). What is not realistic is the dominance of these waves.

Accordingly a sentence has been introduced in the summary, where the relevant part now reads:

This gives evidence that the likely source is related to convection. The tropical GWs in ECMWF are generated in the region of highest shear aloft the convective system. Such waves have been observed in case studies from observations. However, comparison to other modeling studies and satellite data shows that they are not representative of the tropics. Instead, resonant forcing is assumed to be the most important process in generating convective GWs ...

The vertical wavelength depends on the Doppler shift by the background winds. Therefore a dominant vertical wavelength does not exist. For instance, the vertical wavelength changes and will approach zero when the waves are filtered by the QBO (e.g. Ern et al., 2014). The backward rays are stopped because a critical layer is reached from above. This means that in the region of excitation these waves have short vertical wavelengths. This again is compatible with radar measurements. Therefore the second argument is not true: the waves considered here have shorter vertical wavelengths in the excitation region. We will introduce this point where we introduce LTA.

It should be noted in this context that in nearly all cases where the rays are terminated above ground the reason is that the traced GWs approach a critical level from above. In this case the vertical wavelength of a GW becomes small and a supposed vertical wave packet assumes a small vertical extent compatible with a well defined altitude. This will become relevant in particular in section 4.2 where we discuss GW excitation around the tropopause.

Concluding: the discrepancy to nature is not the fact that the described forcing mechanism does not exist in reality, but a) that it may be overestimated in ECMWF and more important b) that other types of wave sources exciting GWs of a few hundred kilometer horizontal wavelength are missing.

The discussion of the whole issue will be shortened because Figure 7 is omitted from the paper. The detailed discussion of Figure 6 is necessary to keep this part self-explanatory for non-specialists.

p11983, l22: origins → originates in 'originates from convection'

and

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...

and

Figure 7: to make the figure more readable, one could suggest ...

Reviewer 2 requests to delete figure 7. Accordingly we will also erase the related text from the manuscript, i.e. these comments do not further apply.

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 deecrease of GWMF (dissipation of waves) rather than generation... I may be misunderstanding the whole point here.

Figure 6 shows that a large number of rays match regions of convection in the altitude range of 14 to 17 km. In the former Figure 7 it was clearly seen that this is the altitude range of the minimum Richardson number. This is indication that somehow both shear and convection underneath are needed to generate the GWs (by the way in accordance with Pfister et al., 1993)

Also in response to the second reviewer the section now concludes (cf. above)

In summary, all evidence presented in this subsection is pointing to the fact that the majority of tropical GWs in the ECMWF model are excited above the convection but not in the convection. As discussed above, this is also the altitude of strongest wind shear where the Richardson number minimizes. This indicates that both wind shear and convection underneath are required for the forcing of the low-latitude GWs in ECMWF, which have very long horizontal wavelengths and comparably low frequencies. In situ observations provide evidence that such GWs also exist in nature. However, whether they are representative for the low-latitude regions must be decided from global observations.

The last sentence introduces the following section.

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.

We will be more specific:

In convective updrafts vertical winds can be as strong as several 10 ms^{-1} *and velocities exceeding 10 ms*$^{-1}$ *are frequent (e.g. Wu et al., 2009; Collis et al., 2013). However, the modeled vertical velocity strongly depends on the use of the microphysics and boundary layer schemes as well as on the spatial resolution of the model (an adequate horizontal grid-spacing would be less than 1 km (e.g. Wu et al., 2009; Del Genio et al., 2012)). Still, typhoon simulations performed for investigation of the emission of stratospheric GWs with a resolution of 25 km (e.g. Kim et al., 2009,Kim et al., 2012) show updrafts of several ms*$^{-1}$*. Compared to these values, vertical winds in the ECMWF model which runs at a similar resolution as the typhoon simulations are tiny.*

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

p11993, lines 10-28: this discussion on the intermittency of waves dues 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 a the more 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.
Thank you very much for this correction! You are right, some key-statements are already made in Plougonven et al., 2013. The text has been modified
The much larger variability in regions dominated by orographic GW excitation has been also quantified statistically in terms of an intermittency factor, both from satellite and superpressure balloon measurements (Hertzog et al., 2008, 2012) as well as from quasi-hemispheric mesoscale modeling (Plougonven et al., 2013).
The accuracy and data density of current-day satellites or superpressure balloons is insufficient to calculate meaningful daily averages. In order to infer the impact of single events on the variability of GWMF in a wider region we therefore have to rely on model data. Plougonven et al. (2013) have shown that the Antarctic Peninsula dominates the variability of GWMF in the latitude range 90°S to 50°S and can cause day-to-day variations of a factor of 2 or more. Our study shows that the variability in the Northern hemisphere may be even higher and we find bursts in the total hemispheric flux by a factor of 3. It should be noted that such bursts of GWMF may be even underestimated in ECMWF or WRF data due to the fact that short horizontal wavelength GWs are missing.

We thank the reviewer very much for his favorable review and the constructive and helpful comments.

The reviewer has two major points:

A) The reviewer points out that a figure which has a large number of caveats should be omitted from the discussion, in particular, if this figure does provide only limited additional information.
B) The reviewer inquires whether we can show simulated HIRDLS measurements, in particular, in case that this effort would not be too high.

With regard to major point **A**:

We have deleted Figure 7 and the according discussion, i.e. we have deleted the text from the top of page 11983 to line 5 on page 11985 from the discussion (line numbers referring to the ACPD manuscript). This requires, however, a few minor modifications to the text below. First, we have to retain some reference to the Richardson number. This is now provided by starting the following paragraph by
In the UTLS region at altitudes where Fig. 6 indicates many wave sources also the Richardson number minimizes (calculated for this study, but not shown). Are waves with similar properties than those seen in the ECMWF data also observed in nature? Generation of GWs in strong wind shear near the tropopause in Monsoon regions ...
References to the previous Figure 7 in section 6 are replaced / deleted.

Major point **B**:

The 2nd suggestion: If not too computationally costly, I suggest you recompute and replot Fig. 9 using HIRDLS viewing geometry (i.e., filtering window) to filter the ECMWF resolved GWs. Right now it's more or less an apple-to-orange comparison. If you can't, it's better to justify how similar ILI filtering window is compared with that of HIRDLS, and what the potential uncertainties are that would be caused by the differences between two instruments.
Figure 9 (now Figure 8) compares spectra without any observational filter from ECMWF with spectra from HIRDLS for which, of course, the observational filter inherently applies. (Please note that we are not discussing a full instrument simulation of an ILI here. This has been clarified in the manuscript, see also the specific comments below.) We have described the expected effects of the observational filter in the text. It should be emphasized that the effects in the case of the subtropical waves are so clear that the application of the observational filter does not affect in any way the interpretation in the paper. However, the reviewer is correct that it would be helpful to demonstrate the effect of the observational filter in the manuscript, too.
Unfortunately the effort for a full end-to-end simulation of HIRDLS would be indeed too high. However, we have developed a comprehensive observational filter for application

to any data set which fully characterizes the waves. We originally intended the filter to be used on ray-tracing results, but it can be applied to the S3D method as well. The observational filter takes into account: The visibility filter in the direction of the line-of-sight due to radiative transfer and retrieval in linear approximation (cf. Preusse et al. (2002)), some filtering mimicking the MEM/HA analysis, the projection of the horizontal wavelength on the tangent point track and, finally, aliasing. These effects are introduced by e.g. Preusse et al. (2009) (cf. Figure 3 in Preusse et al. (2009)).
The comprehensive observational filter will be described in a dedicated paper (Trinh et al., manuscript in preparation for AMT) and is too complex to be introduced here in detail. Since we have no description in the literature so far, we will not give the results in the main body of the paper, but add an appendix in order to illustrate the effect of the observational filter.
The appendix will be introduced in section 4.3, page 11988, line 5 of the ACPD manuscript:
In order to illustrate these points quantitatively, we have applied an observational filter mimicking the HIRDLS observations to the ECMWF data in Fig. 8e and show the results in Appendix B. Because of these effects the ...
For the text of the appendix, please refer to the revised manuscript.
In accordance to this appendix we have weakened a statement in the summary:
but also overestimation of the long horizontal wavelength part of the GWMF spectrum is indicated.

Minor comments:

pp 11964, L27: "incomplete, second," → "incomplete. Secondly,"
Modified

pp 11964, L28: "simplifying" → "simplified"
Modified

pp 11971, L12-17: why not show a figure in the appendix? Also, do you consider realistic noise level? You need to remove instrument noise, signals from turbulence, trapped GWs, etc. before the calculation.
An according appendix has been added. As to the various points:
This is not a full instrument simulation as will be noted in a footnote:
Please note that we only sample to a different grid and do not perform a full instrument simulation. Therefore, the sampled data still retain the characteristics of ECMWF data and do not contain additional noise and are not affected by an observational filter.
ECMWF only resolves waves in the mid-frequency and low-frequency range. Therefore reflection can occur only as partial reflection. There is some indication for partial reflection in the stratosphere, e.g. at the stratopause, but this is several wavelengths above the ground which is the only really "hard" surface for the waves. Unlike for the troposphere, where trapped waves may occur between the ground and the tropopause, there is no likelihood for trapped waves around 25km altitude.

The generally good correspondence between GWMF from temperatures and from winds shows that turbulence or processes other than GWs are not a major issue. We add a sentence with respect to this at the end of the paragraph.
An example showing a common correlation for 34 test days and 25 km altitude is shown in appendix A. The good correspondence between GWMF from temperature and winds also confirms that the majority of analyzed mesoscale events obey the polarization relation of GWs and that therefore the implicit assumption that the majority of these structures are due to GWs is correct.

pp11974, L1: Doing this every 150 km would introduce wave duplication/neglecting as the GWs are inhomogeneously distributed geographically. That would introduce an artifact to the density of Fig. 5 (left column). Could you add on your consideration?
In every volume a wave is fitted, only the amplitudes may vary. We trust that in this way we get a statistical representation of the likelihood of waves at 25 km altitude. Of course, some wave events/wave packets in ECMWF may cover a larger area and thus be represented by several rays. This, however, also reflects that the area of such waves is larger and that they hence contribute more to the GWMF per unit area. Therefore, GWMF per unit area should be well represented by the rays. We have added a short paragraph after page 11974, line 17 (ACPD).
Inferring GWMF values at equal distances along the track provides a statistical measure of the GWMF per unit area for the analysis altitude of 25 km altitude. This is independent of the fact that in this way some wave events may be sampled by several analysis cubes. In the same way, the density of rays or ray origins in a certain region combined with the GWMF magnitude associated with the individual rays provides a measure for the effectiveness of source regions with respect to the GWMF at the analysis altitude.

pp11978, L25: a naive thinking is that since convective source itself is likely to be unstable inside the updraft/downdraft, you can't trace your GWs back to the middle troposphere anyway. Is that possible in your case?
This is an interesting suggestion and we have studied the point in some detail for the case of January 29. GROGRAT returns a statistic for the reason of ray termination. The overwhelming majority of the rays is either terminated because they reach a boundary or because their group velocity becomes extremely small ("ray stalled"; This is caused by reaching a critical level). We have then further considered the buoyancy frequency at termination altitude. For a very few events this became almost zero. However, these events were not associated with typical convection regions. In the convection regions we find very consistently a typical tropospheric value of $N^2 = 0.0001\ \mathrm{s}^{-2}$. The answer is therefore that convectively unstable regions do not affect the discussed source distributions.
The point is interesting, but we feel that it could confuse the readers to include this discussion in the text.

pp11976, L25: "to the ground the ray-termination" → "to the ground, the raytermination".

Comma introduced

Section 4.2, 1st paragraph: you can move the entire paragraph to the figure caption. It's not good to have an entire paragraph of explanation of a figure at the beginning of a section. Starting from the 2nd paragraph, you can note the line style/symbol colors in the parentheses.
We do not agree with the reviewer in this point: the figure caption of F5 is already lengthy and we need to refer to the text for further explanation. Therefore we would like to keep the text as is.

pp11981, L1: "(not shown) a slightly higher" → "(not shown), a slightly higher".
Comma introduced

pp11982, L15: "This we tested" → "We tested this".
Modified

pp11982, 1st paragraph: You cannot completely rule out the mid-troposphere excitation mechanism, as tropical deep convection top ($\sim$ 15 km) is always priorer to meet your extrapolated path.
I would follow your argument, if the trajectories were steep. Then of course a backward trajectory would likely often encounter first the rim of a convective system (identified by a low threshold value) but may end at much lower altitudes in the same convective system. This, however, is not the case: the trajectories are very flat/oblique. They propagate several hundred, often thousands of kilometers before they reach the mid troposphere after passing over a convective system.
In order to test this in a bit more detail we have recalculated the histogram for two further settings: A) we have searched for each encounter the altitude where the ray leaves the region of convection (i.e. used the lowest altitude in the same region of precipitation) and B) used the highest precipitation anywhere along the ray. B) is certainly a conservative estimate: If a wave travels several thousand km and afterwards intersects a further convective region, it is much more likely that the latter system is the true source.
In case A) 53% of the total (including waves never intersecting convection) GW momentum flux are from the UTLS region, i.e. in case A) the UTLS contributes still the clear majority of GWs from convection. The contribution of mid-tropospheric altitudes is flat (i.e. almost independent of altitude) and around 2% in each 1km interval. In case B) also a clear peak in the UTLS is formed.
There is no convincing evidence for GW excitation in the mid troposphere, but very convincing evidence for GW excitation in the UTLS.
Discussing this in detail would add too much to the length of the paper, but we have introduced a few sentences.
Finally, one could imagine that the low threshold generates rather large, continuous areas of precipitation. In this case we hypothetically might identify at tropopause height an intersection of the ray with the convection region at its rim despite the fact that the GW would be really generated at a lower altitude in the center of the convective system. However, the

vertical group velocity of these GWs in the ECMWF model is very small and the rays are therefore very oblique. We have tested for this hypothesis and do not find indication for a major contribution of GWs from lower altitudes.

pp11985, L20: It is very abrupt here to make the statement about the "altitude of strongest wind shear".

The text now reads (also in response to reviewer 1)

In summary, all evidence presented in this subsection is pointing to the fact that the majority of tropical GWs in the ECMWF model are excited above the convection but not in the convection. As discussed above, this is also the altitude of strongest wind shear where the Richardson number minimizes. This indicates that both wind shear and convection underneath are required for the forcing of the low-latitude GWs in ECMWF, which have very long horizontal wavelengths and comparably low frequencies. In situ observations provide evidence that such GWs also exist in nature. However, whether they are representative for the low-latitude regions must be decided from global observations.

pp11989, L13: "That" → "Thus".

Changed pp11979, L5 (cf. **pp11989, L12-16**) to

The fact that the way in which convection ...

pp11989, L12-16: Redundant. Same sentences reappeared at pp11979, L5-8.

My apologies, this happened while restructuring the paper. The sentences are modified to

Given the known sensitivity of modeled stratospheric GWs on the convective parametrization (Ricciardulli and Garcia, 2000; Kim et al., 2007) and given that the parametrization in ECMWF is particularly optimized to produce the correct amount of rain, we discuss the ECMWF convective parameterization in this section. This parametrization contains ...

pp11990, L1: Due to cloud inhomogeneity, it is very difficult to have such a large "w" in the entire grid. Is there another parameter other than the grid averaged vertical velocity that adds on the description of the distribution of vertical velocity within a grid box (e.g., the spread of vertical velocity)?

ECMWF only calculates the spread of the ensemble forecast but does not calculate any spread of vertical winds inside a grid cell. The fact remains that WRF model simulations at similar resolution show not vertical velocities of tens of meter/sec, but at least several meter/sec in individual grid cells. ECMWF values are much smaller than this. Also in reply to reviewer 1 we have rewritten this point more specifically:

In convective updrafts vertical winds can be as strong as several 10 ms^{-1} and velocities exceeding 10 ms^{-1} are frequent (e.g. Wu et al., 2009; Collis et al., 2013). However, the modeled vertical velocity strongly depends on the use of the microphysics and boundary layer schemes as well as on the spatial resolution of the model (an adequate horizontal grid-spacing would be less than 1 km (e.g. Wu et al., 2009; Del Genio et al., 2012)). Still, typhoon simulations performed for investigation of the emission of stratospheric GWs with a resolution of 25 km (e.g. Kim et al., 2009,Kim et al., 2012) show updrafts of several ms^{-1}. Compared to these values, vertical winds in the ECMWF model which runs at a similar resolution as the typhoon simulations are tiny.

pp11990, 1st paragraph: So you don't suggest any remedy here anyway?
Sentence added:
It is therefore important that both weather-forecast and middle atmosphere aspects are investigated in detail and simultaneously, if NWP models shall be employed for seamless climate prediction.