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Interactive comment on "Formation of ice supersaturation by mesoscale gravity waves" *by* P. Spichtinger et al.

P. Spichtinger et al.

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Response to referees' comments on paper ACPD-2004-si05011

Referee #1

Ad *General points*

section 3.1:

It is true that the radiosonde data are assimilated into the ECMWF operational analyses, hence it might be not such a surprise that there is an agreement. But the assimilation uses uncorrected humidity data while we use corrected radiosonde data (using the Lindenberg correction scheme, see Nagel et al., 2001). Hence, it is not self–evident that the profiles agree. Nevertheless, we have included a remark about this issue in

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the text.

section 3.2.2 and appendix:

Using the method described in the appendix we are able to determine whether the main contribution to the relative humidity tendency along a trajectory is due to the temperature change or due to a change of the specific humidity. If the dominant influence on RHi is due to temperature changes (as in Spichtinger et al., ACPD, 2004) one can conclude that (adiabatic) large scale ascent or descent motion induced the changes of RHi. If the main contribution to the tendency of RHi is due to changes of specific humidity, the case is more difficult and definite conclusions are not so straightforward. The change in the specific humidity could be induced by, e.g., cloud processes that could, perhaps, be observed in satellite images. Moreover, in our case the change of specific humidity along the ECMWF trajectories originates from motions that are not resolved in the 6–hourly ECMWF analyses. These are the mesoscale wave motions that become apparent in the MM5 run. The new fig. 10 shows what happened. While the MM5 trajectories rise slowly between $t = -6$ h and $t = +6$ h, the ECMWF trajectories remain flat ($|w| \leq 0.01 m/s$). The mesoscale ascent of the air implies upward moisture transport, such that on a constant altitude or pressure level the specific humidity rises by factors between 1.5 and 1.75 (while along the MM5 trajectories the specific humidity is conserved); since the ECMWF trajectories are flat, the actual upward transport of moisture explains the *apparent* increase of specific humidity along the ECMWF trajectories.

Of course, the failure of the trajectory calculation based on ECMWF analyses in this case does not imply the presence of gravity waves. So the method described in the appendix can either show that a certain humidity tendency was caused by adiabatic cooling (a rather obvious case) or it can point at cloud processes or at the failure of the trajectory calculation due to unresolved motions.

We have clarified this issue in the text and add ECMWF trajectories in Fig. 10 to show the differences. Additionally, we have omitted the last sentence in the appendix. However, we prefer to let the appendix where it is, because it is of technical nature. We have reformulated the appendix to clarify the use of our method.

section 3.4.2:

We have extended the text in this section to clarify this point. But we think that a table of the wave parameters is not necessary.

section 5:

Usually, in the ECMWF data synoptic scale motion, as for example large scale ascents, are resolved quite well (see e.g. Spichtinger et al., ACPD, 2004). Hence, a large scale uplift should be visible in the ECMWF trajectories. But as one can see in the new Fig. 10 there is almost no *large* scale uplift which could explain the formation of the ISSR. The vertical velocities are very small ($|w| \leq 0.01 m/s$) and are not sufficient to induce ice nucleation. The "large scale uplift", which is visible in the MM5 trajectory is in fact due to the superposition of the different wave trains.

For an explanation of the strong increase of specific humidity see comment to section 3.2.2 and appendix. We agree that vertical motions induced by gravity waves conserve the specific humidity, if nothing else (e.g. cloud formation) happens.

specific comments:

1. The ECMWF data are interpolated on a regular $0.6^\circ \times 0.6^\circ$ Gaussian grid. We have included this information in the text (section 2). We prefer to not move the contents of section 3.4.1 here, since in this section only the data sets are described. We think that a description of the model boundary conditions is more appropriate in the modelling section.
2. We follow the recommendations.
3. We have slightly changed the text (including "mostly").

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4. We have reformulated this phrase.

5. We have included an explanation in the text.

6. We have omitted the acronym “MRF”.

7. We have reformulated the phrase, following the recommendations.

8. The text has been changed accordingly. Inspecting of time loops of the simulation results show indeed upward propagating waves above the tropopause level. See also comments to section 3.4.2 above.

9. See comments to sections 3.2.2 and 5. We follow the recommendation and have included the ECMWF trajectories.

Referee #2

Ad *General points*

The essence of the general comments of Rev. 2 is a demand for an assessment of the robustness of our numerical simulation results. The numerical solver MM5 has been used extensively by our group and international partners to simulate inertia–gravity waves excited either by mountains or by jet streams. References can be found in the list. We applied MM5 only to resolve the dynamical features of gravity wave excitation and propagation which are not resolved by ECMWF analyses. The numerical setup of the simulations is based on the experiences documented in a long series of papers. Therefore, we consider it beyond the scope of a short article to provide a thorough model sensitivity study. With respect to spatial and temporal resolutions and the choice of the turbulence parameterisation we used values from successful runs related to this subject.

Furthermore, Rev. 2 asked for results of specific humidity. We intentionally excluded the discussion of the microphysical part of our mesoscale simulations for the following

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reason: the results of specific humidity depend on the particular microphysical scheme chosen for the MM5 simulations. In this way, we would bring new uncertainties in the discussion. Moreover, the existing schemes are not very well suited for the nucleation processes in ISSRs, especially the formation of ice crystals due to homogeneous nucleation (see Koop et al., 2000) is not correctly parameterised.

At present it is not possible to check the horizontal extension of ISSRs with radiosondes. As mentioned in the text we use corrected radiosonde data for our investigations, and the Lindenberg radiosonde data is the only corrected data set sofar.

Specific comments:

1. Traditionally, humidity measurements in the upper troposphere are considered rather difficult and errors of the order of at least 10% generally must be accepted. However, the Lindenberg observatory has developed a technique to gauge and correct humidity measurements from routine RS80A radiosondes such that the results are claimed to be very precise even in the cold and dry tropopause region. Nagel et al. (2001) claim a precision of the humidity measurement of 1.9%RH. This may be believed or not. For the present investigation we do not require that humidity measurements are much more accurate than the mentioned 10%.

2. Clarified in the text.

3. In our opinion the zonal wavelength is rather $\lambda_h \approx 200 - 225$ km than $\lambda_h \approx 250 - 300$ km. We have changed the text in this way.

4. See comment to Rev. 1 and the changes appearing in the text. We kept Fig 8 as this is the only observational evidence of the presence of inertia gravity waves. The model results from times 2300 UTC till 0200 UTC all show a similar structure of the waves, thus, the choice of 0100 UTC was arbitrary. To be correct (as the DWD starts the radiosondes one hour early), the radiosonde was launched at 22:50 UTC!

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5. See comment to Rev. 1, section 3.2.2 and section 5. We have additionally included ECMWF trajectories in Fig. 10.

6. What we mean is the region around Lindenberg where the superposition of waves from different sources leads to a larger scale uplift as usually done by one wave train.

7. Some discussion is included in the conclusions section, and we think that the recommended discussion section concerning the robustness of the model simulations would go beyond the scope of this article.

8. See comment to Rev. 1, section 3.2.2 and section 5.

9. See reply to the general comments.

10. Section 4 and its corresponding figures 11 and 12 are included as an illustration of where ISSRs were probably located above Europe for this special case. The section can be regarded to give supplementary information to the description of the general meteorological situation given earlier in the paper. These figures do not allow to draw conclusions about the gravity waves, neither this was intended: first, they are plotted from ECMWF data which do not show the gravity waves; second, figure 12 shows the upper ISSR which lies directly below the tropopause and which is probably the location of the thin cirrus that can be seen on the AVHRR image. This ISSR had nothing to do with the gravity wave.

technical comments:

1. We use only one ISSR interval

2. Corrected.

3. We use only one tropopause pressure (p=183.2 hPa)

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4. Agreed, and corrected.
5. Corrected.
6. Corrected.
7. Yes, we change λ_x into λ_h.
8. No, we mean $t = +12$ h, although we show $t = 0$ h in the Figure. We have included a remark in the text.
9. Corrected.
10. This is a matter of taste. We think that the large scale dynamic is indicated quite well in this standard geopotential plot.
11. Corrected.
12. Corrected.
13. We prefer to not introduce more isolines in this plot, as there is already now almost too much information in this plot.
14. These values are taken from the model levels (not pressure levels, corrected in the text).

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 67, 2005.