Response to Prof. Straka's comment

We thank Prof. Straka for his constructive comment. We have made changes and our responses are given below. The referee's comments are shown in bold with our responses in plain text.

The paper for consideration by Cui et al. (2012) is a technically and scientifically sound and useful paper for theoretical, observational, and model work.

I would appreciate a very straight-forward purpose statement both in the abstract and right up front in the introduction. This would be useful to place all of your hard work into a certain mind set.

We believe that the abstract is as clear and straight forward as we can make it. The last few sentences of the Introduction also provide a straight forward statement.

Perhaps the most important aspect of the this short but information packed paper is the potential usefulness of the exceptional wealth of data to compare three-dimensional bulk and bin microphysics parameterization simulations of wave cloud simulations at warm temperatures of this sort that form without ice, with in situ observations.

We agree. This has been mentioned in the discussion and conclusion section.

With this in mind, I think a table of p, t, td, u, v would be exceptionally useful to readers of this paper. First it puts the structure of thermodynamics and dynamics of the atmosphere in perspective. Second, the vertical structure of the atmosphere in the vicinity of these and similar warm wave clouds is largely unknown. It is important that if you simulate these clouds, that others have the opportunity to do so too for comparisons. As a courtesy it would be very useful that you make it possible for others to do so with the appropriate information directly and quickly available from your paper.

We added a table in which the pressure, height, temperature, dew-point temperature, and wind speeds of u and v are provided.

Also it is essential that any simulations that are done of these particular types of clouds be done in three dimensions, as the energetics in two-dimensional models often is subject of much controversy (see Fjortof 1953, Tellus, 5, 225-230, this wonderful classic is widely available on the web for free from Springer).

We agree with the point that three-dimensional simulations are important to avoid the controversy. We added the following in the discussion and conclusion: "It should be pointed out that a model should be run on a three-dimensional grid since the spectral distribution of kinetic energy in a two-dimensional flow differ fundamentally from a real three-dimensional turbulent flow where the square of vorticities is not obliged to be conserved (Fjortoft, 1953).

It would be interesting if a cloud-drizzle bulk parameterization scheme is able to capture the essence of such simulations as a might be found with warm cloud/precipitation bin parameterization model.

We added a tables in the revised manuscript with the profiles of pressure, height, temperature, dew-point temperature, and wind speeds. Those data can be used to simulate the wave clouds with models with both bulk- and bin-schemes.

Some basic questions include:
Do the size and number concentrations of drop when integrated over the spectrum produce cloud amounts consistent with those observed?

The CDP is an open-path probe and detects forward-scattered light from small particles. The CDP-LWC is an integrated quantity based on the size spectrum. We have calculated the CDP-LWC using the size distribution, and the sample volume derived from the real aircraft speed. The values of the LWC are smaller than the provided CDP-LWC which used constant aircraft speed. Figures 4a, 6a, 7a and 8a were re-plotted using the new LWC. Both Nevzorov and JW probes are instruments of hot-wire type. The results of our comparison in the manuscript show that the CDP-LWC is generally larger than the values measured with the Nevzorov and JW probes, which was discussed in the revised manuscript as suggested by Dr. Heymsfield. Please see the reply to Dr. Heymsfield's comment for detail.

What fraction of aerosols that were observed with FAAM BAe-146 are nucleated and what are the composition of nucleated and un-nucleated aerosols? Last year you published a paper with Dr. Blyth on aerosols on ice clouds (Cui et al. 2011, ACP).

We intended to add a table which can present the aerosol size distributions. However, we have checked the aerosol concentration and size distribution measured with the PCASP. Since the concentration in the first two bins were higher than expected, the concentrations likely suffered from noise resulting in very high counts. For this reason, we did not provide the aerosol measurements and carry out this direct comparison.

Can some explanation be given for the development of drizzle size drops? I suppose that one could argue that long growth trajectories allows the development of 100-200 micron size drops. Can you estimate with the time of the growth trajectories might be from simple microphysical calculations of diffusion growth to see what role collision-coalescence might have via differences between diffusion growth and what is observed.

This is an interesting point. The aircraft penetrated the wave clouds at several levels and made measurements. The wind speeds alone could not provide the information one needed to estimate the trajectories. It is definitely worth studying this in the future.

What is the potential for getting mobile radar observations at maybe x-band of these clouds? I do not know if these are as widely available for research in Europe as they are in the States.

There is now an X-band mobile dual-polarisation radar in the National Centre for Atmospheric Science that can be used to explore this issue in the future.

Do you have similar observations but in colder, ice laden wave clouds available from the general region available for comparisons.

We do not have similar observations of mixed-phase or ice wave clouds at lower temperatures.

Finally a new definition of wave cloud was promised. Could this be very prominently stated?

We now think that the sentence, "A new definition of a mountain-wave cloud is given, ...", in the abstract is impropriate. Rather there is a difference between microphysics and dynamics from the accepted model. The sentence in the abstract has thus been changed to say: "The measurements presented here and in previous recent studies suggest a different interaction of dynamics and microphysics in wave clouds from the accepted model."

References

Fjortoft, R.: On the changes in the spectral distribution of kinetic energy for two-dimensional non-divergent flow. Tellus,5, 225–230, 1953.