

## ***Interactive comment on “The two faces of cirrus clouds” by D. Barahona and A. Nenes***

**B. Murray (Referee)**

b.j.murray@leeds.ac.uk

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Barahona and Nenes (B&N) describe a very interesting modelling study of the impact of gravity waves on the formation of cold cirrus clouds. Their motivation is to understand the low ice number densities and high in-cloud humidity in ‘low-temperature’ cirrus clouds. It has been suggested by several authors that heterogeneous nucleation of ice particles can explain TTL cirrus properties. They first examine cloud formation with constant cooling over a very wide range of cooling rates. They come to the conclusion that the heterogeneous explanation is inconsistent with gravity wave cooling rates (I have comments on this below). Their subsequent modelling is then done with homogeneous freezing only. They go on to show that there are two possible ‘states’ in which a cirrus cloud can exist which are controlled by the properties of gravity waves. One of these states produces clouds with low number densities and slow saturation relaxation times.

I agree with the authors (and previous authors) that gravity waves may play an important role in cold cirrus formation and should be investigated, however I think that some of the conclusions of this paper are incorrect. In particular, I disagree that the present manuscript provides evidence that heterogeneous nucleation cannot account for the low ice number densities in TTL cirrus cloud. I have detailed my concerns with the article below:

1) My primary comment is concerned with the conclusion that heterogeneous nucleation cannot explain the low ice numbers in cold cirrus. This conclusion is arrived at by varying the updraft rates (without gravity waves) up to 100 cms-1. Slow updrafts are representative of synoptic cooling and faster ones are representative of the phase of gravity waves in which there is upward movement of air. They show that for glassy aerosol at updrafts larger than 15 cms-1 insufficient ice crystals nucleate to prevent the saturation rising to the homogeneous threshold which leads to high ice densities. This is in good agreement with Murray et al. (Murray et al., 2010) who showed that only a small number of ice crystals formed below 15 cms-1 and a much larger numbers form above this threshold as homogeneous nucleation becomes viable. This simplified model employing a constant updraft over the duration of the model run is appropriate for modelling synoptic cooling, but it is not sensible to approximate the effect of a gravity wave as a constant cooling.

A parcel of air influenced by a gravity wave will only experience rapid updraft for a time within the cycle of the wave. The mean amplitude in temperature associated with these waves, according to B&N, is up to a degree. This would correspond to just 10-20% in RH<sub>i</sub>. Hence the model employing a constant cooling over many degrees (and many 10's % in RH<sub>i</sub>) is not representative of a gravity wave. A more accurate picture is a synoptic cooling with gravity wave driven temperature perturbation superimposed on it (as is described later in the paper when only homogeneous nucleation is considered). In this case and in the presence of glassy aerosol, ice particles would be produced as soon as  $S > 1.2$  during the cold phase of a wave. These particles would then have time

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to grow and deplete water vapour during the warm phase of the wave. The extent of the depletion would depend on the number and size of the crystals and the time that they exist in a supersaturated environment. The number of ice particles which nucleate in the next cold phase of the wave would depend on a number of factors including the extent of depletion. Given a sufficient synoptic cooling rate homogeneous nucleation may be possible in addition to heterogeneous nucleation. This synoptic cooling with the effect of a gravity wave superimposed is very different to a situation in which there is a constant and very rapid cooling. Hence, on the basis of their constant cooling model run B&N should not conclude that heterogeneous nucleation cannot produce low ice number densities based on this model.

B&N have shown that at sustained updraft rates of more than 15 cms<sup>-1</sup> large ice numbers are expected since homogeneous nucleation occurred even in the presence of heterogeneous ice nuclei. This conclusion is not new (Murray et al., 2010). However, the typical gravity waves in the TTL discussed later in the paper certainly cannot provide such an extreme sustained updraft, hence heterogeneous nucleation is most likely an important process in the TTL.

I suggest removing this section (section 2) and concentrate on the two dynamic states of cirrus clouds which as far as I am aware is new and provides a competing mechanism to producing low ice crystal number densities in the TTL.

2) Title: I suggest replacing 'two faces' with 'two dynamical states'. When I first saw the title I thought the paper was about the two crystalline phases of hexagonal ice. After reading the abstract I then understood, but the authors may well miss interested readers!

3) It would be helpful if the authors would explicitly state which region(s) of the atmosphere they are interested in. They refer to cold cirrus, but many of the citations are focused specifically on the TTL and the low ice density issue is certainly a TTL issue. I think the paper is primarily about the TTL, but this is not stated clearly.

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4) In the abstract and conclusions it is stated very boldly that our understanding of cirrus in climate change is reshaped. Are the types of cloud discussed here, subvisible cirrus in the TTL region, really that critical for climate to warrant such a bold statement? other cirrus types in lower and warmer regions are far more important for climate.

5) P30858. 'It is well known that . . . . .primarily by homogeneous'. I do not think this is true. Many studies show that heterogeneous nucleation is also important. In fact, DeMott et al. (2003) which is cited to back up this statement show that there are a lot of heterogeneous nuclei in cirrus regions.

6) Use of 'freeze' throughout the paper. Freeze is by definition the transition from a liquid to a solid. Nucleation of ice and crystallisation of solution droplets is freezing. Deposition nucleation of ice onto a crystalline ammonium sulphate particle is not freezing. I initially thought the authors were talking about immersion mode heterogeneous freezing, but they seem to be misusing the word freeze. This needs to be corrected throughout the manuscript.

7) P30861. Ln 7. Delete 'typically'. This is based on very limited measurements.

8) P30862, Ln 1. Insert 'on sulphate' after 'heterogeneous freezing'. There needs to be a stronger distinction between the heterogeneous nucleation on sulphate and on glass.

9) 30862, Ln 1-3. 30-70% supersaturations. Froyd et al is an inappropriate reference for this, their paper is on the composition of aerosol in the TTL. Also, I do not understand where these numbers come from. Kramer et al.'s figure 3 suggests  $S = 0.3 - 2.0$  below 200 K and fig 7 suggests that the most probably  $S$  is  $\sim 1$  until you get to the very lowest temperature. There are other measurements which suggest larger in cloud  $S$ . However, the uncertainty in the water data in general is too high to rule out heterogeneous nucleation and should not be done. This section needs to be revised.

10) 30862, Ln 7-15. B&N suggest that heterogeneous nucleation on glassy aerosol is inconsistent with Kramer et al.'s saturation data, because they suggest  $S$  would be

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mostly below 30%. As mentioned above Kramer et al. show that the most likely  $S$  between 185 and 200 K to be around 1, and only below this does  $S$  increase. Hence, this does not support B&N's argument.

11) Gravity wave spectrum. How well known is this? I understand that gravity waves are very hard to measure and characterise and our knowledge is limited. What B&N have done in this respect seems sensible, but I worry that the conclusions drawn from this 'shaky foundation' are a little too confident. The paper is written in a very confident style and I think it needs to be accepted that there are still questions and problems and the style needs to be set accordingly.

12) I would have liked to have seen a more comprehensive review of what is known about gravity waves in the TTL region and why this particular parameterisation is suitable.

13) Conflict with Jensen and Pfister (2004). J&P state that the effect of gravity waves is to increase the ice number density. Since B&N based their gravity wave spectrum on J&P (at least in part), they need to discuss this apparent discrepancy.

14) 30877 In 11. Why are clouds in the dynamic equilibrium region less sensitive to IN? As far as I can see the effect of changing IN and introducing heterogeneous IN have not been explored in the wave model.

15) The 'Pulse –decay' and 'dynamic equilibrium' terminology. I find this terminology a little confusing. If you look at Fig3a and b, the curves for the dynamic equilibrium regime look like pulse decays. I know you are not referring to this, but it is confusing.

16) What are the in-cloud saturation values predicted by your model? How do these compare with the field data. I can only see a plot where  $S-1$  is averaged over the layer.

Technical comments:

1) P30865. Ln 15. Symbol 'x' should be non-bold and italic.

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- 2) Fig 1 caption. 'lines' needs to be 'line' in several places.
- 3) Fig 8. I wondered if the two axes might be switched on this plot to be consistent with the preceding plots.
- 4) P 30874 In 13. Are these ammonium sulphate particles solid or liquid?
- 5) P30860 In 29. Should 'depressed' be 'suppressed'?

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Interactive comment on Atmos. Chem. Phys. Discuss., 10, 30857, 2010.

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