

Interactive comment on “Comment on evidence for surface-initiated homogenous nucleation” by J. E. Kay et al.

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I will just state a few more facts, which were misunderstood by Kay et al..

Nowhere in the Tabazadeh et al. and/or Djikaev et al. articles have we claimed to be the first to describe the condition of partial wetting. What is new about what we have derived is that, if the condition of partial wetting holds for just one crystal facet, then the surface mode is THRMODYNAMICALLY favored over the volume mode. This derivation is not debatable if the capillarity approximation is applied to derive the free energy change for the nucleus formation. For the authorŠs information, both the classical volume- and surface-based nucleation theories, which are commonly used in atmospheric science, rely on capillarity approximation. According to this approximation, the properties of the nucleus are estimated using bulk density, bulk surface tension, etc.

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Thus the comment, stating that the facet for a small body could not be defined and/or surface tension has no meaning for a small nucleus (as stated in the original comment) are groundless in the context of the capillarity approximation. Also it is well known in the literature that the condition of partial wetting improves as temperature is lowered (see Djikaev et al articles for references on this fact). Thus if ice at zero degrees Celsius has some facets that are partially wetted, as shown in the Elbum et al. study, then one can be sure that as the temperature is lowered this condition will certainly improve.

I state again that we will be happy to reply fully to Kay et al. comments regarding our PNAS paper if her comments are submitted to the appropriate journal.

Interactive comment on Atmos. Chem. Phys. Discuss., 3, 3361, 2003.

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