

## ***Interactive comment on “On cloud modelling and the mass accommodation coefficient of water” by A. Laaksonen et al.***

**Anonymous Referee #2**

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Laaksonen et al. show that a common formulation of droplet growth rate is consistent with unit accommodation coefficients for mass and energy. The experimental result which is the basis for this conclusion is the observation of light scattering from a population of droplets, inverted through Mie theory.

Of importance is the fact that droplet growth theory needs to be validated. Results presented here show that the two free parameters in the theory, the mass and energy accommodation coefficients, are constrained and that these values should be utilized when prognosing droplet growth rates in models.

Unfortunately there is not consistency among experimentally-derived values of the

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mass accommodation coefficient. We are told that efforts aimed at reconciling accommodation values based on scattering and the uptake of isotopically-labeled water are not successful. Should this inconsistency be reconciled, there is still the issue of reconciling unit accommodation with the results of Shaw and Lamb (1999).

Short of doing detailed examinations of the experimental data, which is not the intent of this manuscript, we need to move on. In this regard I feel that the recommendations of Laaksonen et al. (use the Fuchs and Sutugin formulism with accommodation coefficients equal to unity) should be heeded.

Specific comments -

I. Several assumptions are needed to arrive a (1): A) The temperature-dependence of saturation vapor pressure is linearized. B) The mass continuity equation which is the starting point for the derivation of (1) assumes that local changes in vapor concentration are small relative to the flux divergence (steady-state assumption). C) Latent heat is assumed independent of temperature. D) A balance between latent and sensible droplet heating is assumed. E) Diffusivity and conductivity are assumed independent of space. Perhaps these are reasonable things to assume, but these approximations and the inaccuracy they introduce needs to be explained.

II. The transition from equation (1) to (2) is too abrupt. What is the basis for (2) (and (3))? Why should we worry about terms introduced in proportion to the square of the Knudsen number when the minimum size detectable in the lab study (Figure 1) is 0.5  $\mu\text{m}$  ( $\text{Kn} \sim 0.2$ ).

III. On two occasions we are told about the coupled system of equations (mass and heat), but we are only shown the combined equation (1). Inclusion of both “coupled” equations would obviate the need for approximations “A” and “C”. Surely, this approximation leads to error at sufficiently high, or low, ambient RH. The reference to Fladerer et al. (2002) suggests not, but this point is not entirely convincing.

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IV. I am surprised that the work of Nori Fukuta is not referenced.

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