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Atmospheric Chemistry and Physics Discussions

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

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General: We thank Referee 2 for the thoughtful comments.

Specific comments:

I. Several assumptions are needed to arrive at (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*

The assumptions mentioned do not lead to serious inaccuracies and their validity have been tested and discussed in several earlier papers (see e.g. Vesala et al., 1997). We desired to present a formula which is convenient for cloud modelling purposes, that is, not too complicated, but still accurate enough.

In brief, A) The linearization of the vapor pressure is for the small temperature interval between ambient and droplet temperatures, and will therefore introduce no big error; B) It is true, that the equations used are steady-state but they are applied in quasi-steady-state manner, that is, the boundary conditions are updated after each time step due to changes in the ambient conditions and to droplet growth itself. Quasi-steadiness can be confirmed by estimating the characteristic times for sub-processes;

C) The latent heat is assumed independent of temperature, but as with the vapor pressure, the temperature interval is so small that no big error is introduced; D) The assumption has been justified by comparing the full growth models (calculating explicitly the droplet temperature from sensible and latent heat fluxes together with changes of the droplet heat storage); E) Diffusivity and conductivity are not assumed independent of space, they depend on temperature and vapour mole fraction, which in turn depend on distance from the droplet. Thus, the spatial dependence of these parameters is implicit.

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 Ian study (Figure 1) is 0.5 μm (Kn 0.2).

The transition may be somewhat abrupt, however, the Eqs. cannot be derived in this paper without diverting from our main point. Instead, we decided to discuss the Eqs. briefly and give relevant references. The squared term may be insignificant but in our opinion it is better to present the full most accurate form of the expression, especially

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as the square term does not make the formula more complicated in practical use.

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 lowm ambient RH. The reference to Fladerer et al. (2002) suggests not, but this point is not entirely convincing.

Again, in this paper we wanted to be as brief and to the point as possible, and we therefore have avoided detailed discussion on the various assumptions as such discussion has been given in several previous papers. In the experiment of Fladerer et al. (2002), the difference between ambient and droplet temperatures was estimated to be 7 K, and for such a temperature interval, especially the assumption of linear vapor pressure should lead to some error. Indeed, Fig. 2 of Fladerer et al. shows that at different growth regions, the approximate treatment either underestimated or overestimated the mass flux given by the full coupled Fuchs-Sutugin expressions by about 10%; however, the resulting difference in droplet growth rates was small.

Concerning our main argument, the important thing is that the full coupled equations with a mass accommodation coefficient of unity reproduce experimental growth rates to very high supersaturations. Additionally, the approximate Eq. (1) certainly gives results in excellent agreement with the full coupled equations at low supersaturations, and seems to work reasonably also at high supersaturations.

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

The paper by Fukuta and Walter (1970) has been very influential especially in the cloud modelling community. Again, for the sake of brevity, we left out references to the works of Fukuta, as well as others who have presented diffferent models for transition regime droplet growth such as Sitarski and Nowakowski (1979), Dahneke (1983), and Loyalka (1983).

References:

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