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

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We thank Referee #1 for thought-provoking comments.

General:

1. In the referee's opinion there is nothing new in the issues being raised in our paper, and the "sole apparent purpose of the paper is to "point out" that cloud modelers should use a value of unity for the mass accommodation coefficien of water in order to maintain the with the authors' results".

This paper is admittedly more of a commentary that a regular research article; however, since we are not commenting any specific papers, we submitted the manuscript as an article. If the Editor feels it appropriate, we are willing to change the title of the paper to

"Commentary on cloud modelling and the mass accommodation coefficient of water".

Whether or not the issues being raised are new, we believe that they are very important from the cloud modelling point of view. It is not our purpose just to to point out that growth theories with alpha $<$ 1 are inconsistent with our own results. What we want to point out is that the droplet growth rate is the primary quantity of interest in cloud modelling, not the mass accommodation coefficient. Hence, droplet growth rate measurements yield experimental information which can be used in direct verification of growth rate theories, while measurements of the mass accommodation coefficient using e.g. the droplet train apparatus do not. And all information from growth rate experiments - ours and others' - indicate that the transition regime growth rate theory is an excellent theory if the value alpha = 1 is used.

2. The referee states that our conclusion about the transition regime growth theory with alpha = 1 being valid for atmospheric clouds is based on a very large extrapolation from experiments carried out at higher supersaturations in which the growth times are much shorter compared with clouds.

It is true that at the moment there's no direct experimental verification for droplet growth rates at atmospheric supersaturations. However, we can still make very good use of the information derived from the growth rate experiments without making mere extrapolations.

Let us first think of a theory (e.g. transition regime growth theory with alpha = 0.1) which is known to yield incorrect results compared with experiments at saturation ratios between 1.378 (this work) and 12 (Fladerer et al., 2002). How likely is it that such a theory would yield correct growth rates at saturation ratios around 1.01? We do not think that the chances are very high.

On the other hand, we have a theory (transition regime growth theory with alpha = 1) which has been shown to yield excellent predictions of growth rates at the interval S = 1.378 - 12. How likely is it that this theory would yield correct growth rates at

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saturation ratios close to 1.01? From all that we know of transport phenomena, it is very likely. There is no fundamental difference in the growth theory which would make high supersaturations and rapid condensation different from low supersaturation and slow condensation, that is, the laws of mass diffusion and heat conduction remain the same. In fact, slow condensation is less prone to errors, since the common assumption of the quasi-stationary growth is less accurate for the case of rapid condensation. Naturally, the growth dynamics (such as changing environmental conditions) are different in real clouds than in laboratory measurements, but this is taken into account by a cloud model itself and has nothing to do with the growth theory per se.

Thus, we maintain that the use of mass accommodation coefficient values below unity in transition regime growth theories is inconsistent with all available experimental information of droplet growth rates.

Specific:

a) We agree that cloud droplet growth could be described with theoretical expressions used e.g. in analysis of the droplet train experiments, and in this sense Referee #1 is correct. However, the analysis of the experiments also involves elements that have nothing to do with cloud droplet growth. An example is the usage of the diameter of the drop-forming orifice instead of droplet diameter in the Fuchs-Sutugin expressios (Li et al., 2001). In any case, we will remove the last sentence of the abstract from the revised version of the paper.

b) The experimental datapoints are actually different from those shown in Winkler et al. (2004). We will clarify this in the revised paper. Delta t indicates the time from the end of the expansion, i.e. the length of the period during which the supersaturation stays constant.

c) The results in the table were calculated for this paper, but the cloud model has been described in prior publications, e.g. Kulmala et al. (1993).

Technical: We will correct the sentence.

References:

Fladerer et al., J. Aerosol Sci. 33, 391 (2002)

Kulmala et al., J. Geophys res. 98, 22949 (1993)

Li et al., J. Phys. Chem. A., 105, 10627 (2001)

Winkler et al., Phys. Rev. Lett. (2004)

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 7281, 2004.

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