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

Anonymous Referee #1

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General Comments/Suggestions:

The theme of this paper, the determination of mass accommodation coefficients and their use in cloud modeling, is scientifically important and suitable for publication as a discussion paper in ACPD. However, the manuscript at this point falls short of being publication-worthy in ACP, in my estimation.

This paper recognizes the age-old disparities in the empirically determined magnitudes of the mass accommodation coefficient of water, and it reiterates the importance of using the "correct" values for the mass accommodation coefficient and thermal accommodation coefficient in atmospheric cloud models. A theoretical formulation based on a coupling of mass and heat fluxes represents a summary of much past work, some of which is already available in textbooks (for example, Seinfeld and Pandis, 1998). Validation of the authors' chosen model is derived from the recent paper by Winkler et al. (2004), in which it is demonstrated that data from a rapid-expansion cloud chamber are best fit with values = 1. It is true, as the authors imply, that a "rigorous transition regime growth theory", verified against careful laboratory experimentation, is a crucial prerequisite for applying the theory to atmospheric problems like those in cloud physics. All of these points are acknowledged as being important for the science, but none of these issues is being raised for the first time in this paper.

The sole apparent purpose of this paper is to "point out" that cloud modelers should use a value of unity for the mass accommodation coefficient of water in order to maintain consistency with the authors' results. This paper provides no new insight into the discrepancies that still exist in the literature, so it is not clear that their results are more applicable to droplet growth than those of anyone else. The issue of disparate accommodation coefficients remains unresolved. One can readily see that the droplet-growth theory based on the transition-regime corrections for mass and heat transport given by Fuchs and Sutugin (1970) works well for the rapid-expansion chamber data of Winkler et al. (2004). It may also be that the formulation is applicable to high supersaturations in all growth regimes. The conclusion of the authors that such results are valid for atmospheric clouds is nevertheless based on a very large extrapolation of findings derived from experiments lasting tiny fractions of a second to the atmosphere, where droplets evolve slowly (time scales of hundreds of seconds) at supersaturations at least an order of magnitude less than those employed in the cloud chamber. Excellent agreement in one setting does not automatically imply agreement in all settings.

Specific Suggestions:

a. The last sentence of the Abstract is misleading. A variety of theoretical expressions are used in cloud models, I believe, so it is should not be concluded that experimental results yielding low values of mass accommodation coefficient are necessarily based on theoretical expressions not used in cloud models.

b. Figure 1 (discussed on page 7286) differs from the corresponding graph in the cited reference (Winkler et al., 2004), despite wording that suggests it comes directly from the paper. Any differences between the experimental data and theoretical curves presented here and in the cited work need to be explained in detail. The delta-t in the figure needs to be defined and explained as well.

c. Table 1 extends the implications of the experimental work of Winkler et al., but too little information is provided to make the results useful. A full explanation of model assumptions and calculations is needed. If the results are from some other paper, then that work should be cited.

Technical Corrections:

The sentence starting on line 13 and ending on line 17 on page 7286 is grammatically incorrect. Perhaps two simpler sentences would improve the flow of ideas.

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