Atmos. Chem. Phys. Discuss., 8, S6028–S6030, 2008
www.atmos-chem-phys-discuss.net/8/S6028/2008/

Atmospheric
Chemistry
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Discussions

Interactive comment on "Determination of the evaporation coefficient of D_2O" *by* W. S. Drisdell et al.

W. S. Drisdell et al.

Received and published: 15 August 2008

Reviewer 4, finds our manuscript to be well written and informative but also to have considerable overlap with our previous work. He questions whether our paper should be published in this form or as an abbreviated technical note.

While we recognize that there is considerable overlap with our previous work, we believe that the results presented here are new and important in their own right. Although it turned out that the evaporation coefficient we observe for D2O is indistinguishable from that for H2O, this need not have been the case. Much of the text is necessary to understand the context of these new (and challenging) measurements. We hope the editor will side with the first two reviewers who did find the paper interesting and deserving of publication in ACP. Reviewer 1, at least, is quite familiar with our previous work and did not find this paper needlessly redundant.

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Other Comments The reviewer takes issue with our description of the solution to a pair of coupled heat and mass transfer equations. We proceeded exactly as described by the reviewer (and in a way that was apparently clear enough to reviewer 3 that he/she could reproduce our calculations and test variations on them). In the revised manuscript we will try to clarify the language along the lines suggested by the reviewer.

Reviewer comment: I have a few questions regarding Fig 4, which is a calculation done for a drop 6.65 microns in size - is this the size of the drop produced by the VOAG? Is it assumed constant (it clearly must be shrinking, after all)? If so, can you justify it? If not, can you show how much the drop has decreased in size between production and the laser view volume? Is the final derived gamma,e strongly sensitive to this assumed/measured size?

Response: 6.65 microns is indeed the initial size of the droplet produced by the VOAG. On the timescale of the experiment, the droplet radius shrinks by ˜5%. This is accounted for by our cooling model, which considers the mass loss due to evaporation at each time step and rescales the droplet and the spherical shells accordingly. The surface area of the droplet, which is the primary factor affecting the evaporation rate in this case, is therefore rescaled properly as well.

Reviewer comment: Lastly, I know very little about the partition function/transition state stuff used at the end, so I could not really evaluate that. This ignorance probably leads to the (naive?) question regarding why rv; values less than unity are reported (8574, lines 16 to 17). I can not imagine this makes physical sense.

Response: Values of rv; less than unity would imply that the evaporation coefficient for D2O is higher than that of H2O. There is nothing unrealistic about this possibility. As is discussed in the manuscript, the evaporation coefficients of the two species are found to be identical due to competing entropic and energetic effects. If the entropic effect were larger in magnitude than the energetic effect, D2O would have a larger evaporation coefficient. It is important to note that a larger evaporation coefficient does

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NOT imply a faster evaporation rate; the theoretical maximum evaporation rate of D2O is smaller than that of H2O as per Equation (2) of the manuscript. A larger evaporation coefficient for D2O would merely imply that D2O evaporates at a larger fraction of its maximum theoretical rate than H2O does. This point will be clarified in the revised manuscript.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 8565, 2008.