

## ***Interactive comment on “Determination of the evaporation coefficient of D<sub>2</sub>O” by W. S. Drisdell et al.***

### **Anonymous Referee #4**

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#### Overall Comments

Note: I have avoided looking at other reviewers' comments until after my comments are submitted. Any overlap is generated independently!

The paper in general reads well and the work seems sound. While I'm not an expert in the technique, it seems like an elegant method for getting at a parameter that has been quite difficult to measure in the past. My one major concern about the manuscript is that it is extremely similar to a previous paper from the same authors (Smith et al., 2006, hereafter S06) which uses the exact same experimental method and analysis, but for regular water. The authors acknowledge this, but the very strong similarity wasn't clear to me until I looked at S06 and found that all of the figures are almost identical (there are

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small changes, and obviously the data isn't the same either). The part that is newer in this manuscript examines the differences between H<sub>2</sub>O and D<sub>2</sub>O evaporation, but this framework has been previously developed by many of the same authors in Cappa et al. (2007; hereafter C07). So the new parts of this paper seem to be the new value of  $\gamma_e$  for D<sub>2</sub>O instead of H<sub>2</sub>O and trying to understand the origin of  $r_\gamma$  values that deviate from unity using the model from C07. To me, it seems that the new parts could be stated in a technical note of 2 to 3 pages saying that we used the same method as S06, we find  $\gamma_e = 0.51$ , and then add the newish stuff (the first half of the discussion). This avoids much of the redundancy between S06 and this paper as currently written, which is pretty large (practically everything up to Section 4 discussion, plus the latter part of the Discussion as well where comparisons with previous experimental values are discussed). While there is clearly room for interpretation, I believe that this manuscript doesn't quite meet the ACP requirement that "Research articles report substantial new results and conclusions from scientific investigations of atmospheric properties..." due to this large overlap. This manuscript is a useful contribution to the literature, but I think a different manuscript type is more appropriate.

### Other Comments

The way the energy balance model (used to interpret the spectroscopic results) is described could be, I believe, more rigorous. I'm not sure if there is an actual error in the analysis, or if it is merely that the description gives such an impression. Starting with Eq. 6, the  $M_d$  in the denominator isn't well-explained. The text states that it is the part of the droplet mass that is cooled by evaporation, but it's not as if there's a line you can draw at some radius  $r_t$  for which  $r < r_t$  is unaffected, and  $r > r_t$  has some uniform temperature change, which is how the text reads. The bigger problem with Eqs. 6 and 7 is that they're strictly not accurate, as evidenced by the existence of Eq. 8: the loss of energy at the drop surface leads to diffusion of heat towards the surface. The balance between these two energy terms (and perhaps radiative heat loss? I doubt that it's relevant but maybe worth quickly checking) determines  $T(t)$  of the surface. In

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the end, I would scrap this somewhat confusing (at least to me) framework and go right for the proper equations and use a description something more along the lines of: (a) the mass transfer equation (Eq. 3) gives evaporation rate; (b) the conduction equation for a sphere, which is simply Eq. 8, but in terms of temperature, describes the temperature evolution of the droplet; then (c) we solve the conduction equation using a boundary condition (that's the key concept that seems to be missing here) that is essentially the latent heat transfer associated with the mass transfer (i.e. turn Eq. 3 into a heat flux, which is more or less Eq. 6, but without the  $M_d$  term). Now state that you solve the coupled mass and heat transfer differential equations simultaneously (they each depend on the other) so that they're both satisfied, and you're done (and I'm hoping this is actually how these equations were solved!). That's the right way to do it, and (at least to me!) a much more straightforward and accurate description. If you need some guidance, you can try, among others, the text by Seinfeld and Pandis.

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?

Lastly, I know very little about the partition function/transition state stuff used at the end, so I couldn't really evaluate that. This ignorance probably leads to the (naive?) question regarding why  $r_\gamma$  values less than unity are reported (8574, lines 16 to 17). I can't imagine this makes physical sense...

If you have any questions, please feel free to contact me.

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

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