**Atmospheric
Chemistry
and Physics
Discussions**

Interactive comment on "Determination of the evaporation coefficient of D$_2$O" *by* W. S. Drisdell et al.

Anonymous Referee #3

Received and published: 16 July 2008

General comments

The work by Drisdell et al. is a continuation of a series of their elegant experiments pertaining to evaporation of water in kinetic regime. Using modern experimental methods, the authors skillfully extended experiments undertaken by Knudsen at the beginning of 20th century, to volatile liquids. In previous works they investigated water isotopomer evaporation fractionation factors and inferred about their (relative) evaporation coefficients. They have investigated ballistic evaporation of H$_2$O and this work completes the set providing the results for D$_2$O. The results are additionally valuable since D$_2$O was utilized also by other investigators (e.g. Li et al., 2001).

The obtained value of D$_2$O evaporation coefficient, nearly identical to that previously

found of H_2O, is of great interest in view of recent measurements by other authors. Recently Zientara et al. presented their new results which coincided nicely with those of (Li et al., 2001) not only to the value but also as far as the temperature dependence is concerned. They also pointed to the effect of thermal molecular pressure (thermal effusion) as the possible source of results' divergence, and suggested that by taking this effect into account, for instance, the results of (Winkler et al., 2004) and (Fukuta et al., 2007) can be fit in. However, all those results would be roughly by a factor of 2 smaller than these of Drisdell et al. which on top of that exhibit negligible temperature dependence. Since the effect mentioned above was elegantly avoided in the experiment by Drisdell et al. I think that some more fundamental effect manifested. Consequently, I believe that following the experimental way exemplified by the work of Drisdell et al. shall lead to deeper understanding of evaporation in kinetic regime and effects at interfaces.

Specific comments

I have done a few numerical experiments with the evaporation model used by the authors, also having tried a few hypotheses that might have (but did not) explained the disagreement with aforementioned results, and have found only a minor technical error which I will address later. However I would like to make a few comments/questions leaving it to the discretion of the authors whether these might be useful to a potential reader not thoroughly acquainted with the phenomena involved.

1. Am I correct that the cubical vacuum chamber walls were at room temperature? I tried to account for the radiative energy transfer (up to 40 K temperature difference !) and found it negligible, am I correct?

2. Droplets emerging from VOAG most probably exhibit shape oscillations (also high multipolarity capillary waves) which might lead to a whole family of phenomena significantly influencing evaporation (surface area change, saturation pressure change, cavitation). I checked that these oscillations decay within a few μs which

is negligible in the experimental timescale. Similarly I believe the circulation of liquid in the droplet is of no importance. Is fast rotation of the droplets emerging from VOAG possible? In vacuum they would not be damped.

3. The droplet radius change was accounted for by the authors (page 8572, line 6). I understand that it also covers the "moving boundary effect" for internal heat transfer (compare (Fukuta et al., 2007)).

4. A few words about droplet interaction with light field in the laser beam focus might be instructive.

5. And finally a more general remark. Since this experiment concerned rather vacuum-liquid than gas-liquid interface and additionally an interface off-balance, it would be interesting to address the differences. I find it most promising direction of investigation.

Technical corrections

On page 8567, line 14, m is denominated as "molecular mass". In view of the formulas used, it might be somewhat misleading. If it is, as I think, the mass of a molecule, than in formulas (6) and (7) $\sqrt{m}$ should be in the numerator. This mistype seem to have propagated from authors previous works.

I think that (Jakubczyk et al., 2007) should be substituted (or preceded) with:
M. Zientara, D. Jakubczyk, K. Kolwas, and M. Kolwas, Temperature Dependence of the Evaporation Coefficient of Water in Air and Nitrogen under Atmospheric Pressure: Study in Water Droplets, *J. Phys. Chem. A*, **112**, (2008), 5152-5158, doi: 10.1021/jp7114324

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