

Interactive comment on “Aerosol hygroscopic growth parameterization based on a solute specific coefficient” by S. Metzger et al.

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Apart from a lot of long known theoretical considerations, and a review on (5 ?) water activity representations (from line 15, p. 24819 to line 21, p.24823), this paper is without much scientific content. The parameterization equations (equations 15, 16a to (20)) do not content better information than the experimental or model curves they are drawn from. The newly found "single solute specific coefficient" ν_i has no specific physical meaning or gives no better physical insight. According to the authors' findings, it could be considered as a solute specific constant (lines 14 p.24814, and 7 and 21 p.24824). So, the interest must be elsewhere.

The authors want to introduce a new and simpler parameterization for the water activity in function of the solute molality. The problem is that this relation depends on ν_i and two other functions (or best fittings (line 13 p 24827) A and B (equations (18 to (20)) themselves depending on ν_i . The solution proposed by the authors themselves (lines 12 to 19, page 24828) is to start with the molality and water activity "at saturation" to determine A and B. And afterwards to use these values to determine ν_i . The problem is

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they don't have a starting value for ν_i and so, in their own scheme, they can't calculate A or B. This won't work.

Of course, we could think of putting the equations of A and B into (16b), and RHD being known, solve it for ν_i by some numerical recipe. Then, ν_i being considered almost constant (line 14, p. 24814), we could use (16a) ... But this looks not practical to me: we do not know if the numerical method will always converge and if it does, how long it will take and if the solution is unique.

A deeper look could yield the following conclusions:

- A and B are functions of ν_i and μ_s only, saturation or not
- (16a) is an expression between three variables : RH, μ_s and ν_i , with the 2 (unexplained) expressions A and B (computed for saturation or not),
- (16b) is (16a) computed for saturation only, and leaving (RH= RHD known, $\mu_s = \mu_{s,sat}$ known) only one variable ν_i , with A and B,
- replacing A and B into (16b) makes it an expression with only one variable, ν_i , but valid for saturation only,
- so the value of ν_i , on is calculating with (16b) is for saturation only,
- therefore, if you want to use it elsewhere, it is essential to claim that $\nu_i = \text{constant}$ for the whole domain.

I'm afraid you cannot do otherwise than use an equation like (16b), because you need measurements at saturation where RH= RHD and $\mu_s = \mu_{s,sat}$ are both known.

The further development is based on a lot (too much ?) of assumptions and simplifications for the results to have any practical use:

- A, B are functions of ν_i and saturated molalities only (p. 24827)
- ν_i is constant (line 14, p. 24814),
- considering two kinds of single solutes in binary solutions (line 1, p. 24817),
- ideal solutions (line 7, p. 24814),
- large relative humidity (line 7, p. 24814),
- volume additivity (line 1, p. 24819),
- $K_e = 1$ (line 16, p.24828); seems not necessary).

In my opinion it is not necessarily good tactics to delay confidence in a paper until the disclosure of results in "companion papers" :

- Explicit derivation (lines 13 to 15, p. 24827),
- EQSAM4 (lines 22 to 23, p. 24814, and line 11, p.24817),
- Box model inter-comparison (lines 22 to 25, 24817)

Specific comments:

1. (line 6, p. 24814) : The " (1) wide applicability" has not been shown in this paper.
2. (line 6, p. 24814) : The " (2) simplicity" is only true if the assumptions hold and if one knows beforehand the values of the different ν_i .
3. (line 6, p. 24814) : What does " (3) analytical nature" mean here ?
4. (lines 14 and 15, p. 24814) : What are these findings, where do they come from ?

5. (lines 24 and 25, p. 24817) : Wouldn't it be better not to make reference (p.24843) to a rejected paper ?
6. (lines 13 to 18, p. 24818) : In my opinion, the term "proportionality" is not correctly used here. The related explanations are not convincing.
7. (lines 13 to 15, p. 24824) : 1 mole of pure solute dissolving in 1 kg initially pure water is indeed 1 mol/kg but it is not an "initial concentration" of any kind : initial to what ? μ_s^0 is just some additional constant with units.
8. (lines 1 to 4, p. 24828) : Why "it should be possible" ? Is there a hidden limiting factor somewhere ?

In my opinion, this paper needs a deep rewriting.

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