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A simple model for the time evolution of the condensation sink in the atmosphere for intermediate Knudsen numbers

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This paper addresses the accuracy in calculated values of the condensation sink (CS) obtained when using an approximate expression for the Fuchs-Sutugin coefficient (FS). It is shown that this expression, obtained by keeping only the first order term when FS is expanded in terms of *1/Kn*, is sufficiently accurate for *Kn* values down to 0.5. The authors show that this approximate expression for CS facilitates obtaining an analytic solution to the aerosol general dynamics equation (GDE) by the method of characteristics. This solution accounts for condensation but neglects coagulation, sources, transport, mixing, and other processes that are sometimes important in the atmosphere. The analysis was expanded to include first order losses, leading to an exponential decay in concentration (see Eq. 21). The paper includes results that will be of some value for atmospheric modeling, and was written with reasonable care. It also includes nice mathematical approaches that are not new but are seldom applied. I recommend that the paper be published after the authors consider the following suggestions:

1. Section 2.1 would read better for me if the authors had first shown the solution for time-dependent vapour pressure (solutions given by Equations 10 & 11) followed by the simplified solutions pertinent to constant vapour pressures (Equations 4 & 7). This change is not necessary, but would be an improvement in my view.

2. Figure 9 compares model predictions with observations made on July 24 in Hyytiälä. A first order removal term (τ_{loss}=7 hours) was included in the GDE to account the observed decrease in number concentrations observed at night (see Figure 7). Initial growth rates of 2.6 nm/hour at night and 12 nm/hour in the daytime were input parameters for the model. The authors argue that the model satisfactorily explains the nighttime and daytime observations of CS(t) and $d_p(t)$, but not the large decrease in CS beginning at about 07:00, presumably due to breakup of the inversion after sunrise. While this might be an interesting heuristic result, I question its applicability to the atmosphere. More effort would be needed to demonstrate that all pertinent atmospheric processes have been properly taken into account. For example, does diameter increase linearly and monotonically with time during the daytime on all days at Hyytiälä? If not, it would appear that the authors picked a day with observations that could be explained, not necessarily because the model correctly describes what happened. I would be more comfortable with the paper if Figure 9 were deleted.

3. Figure 3 shows the relationship between CS calculated using the authors' simplified expression and the value of CS calculated using kinetic theory for a range

of aerosol properties pertinent to the atmosphere. The default method for calculating CS for atmospheric data is by using the full FS expression, not the kinetic approach. The only rationale I can see for including this figure has to do with the relationship between the kinetic CS and the extinction coefficient mentioned in the conclusions. As that is not the focus of the paper, I would recommend either that Figure 3 be deleted (my preference) or that it be replaced with a figure that compares the CS_{corr} to CS_{FS}.

Minor points:

1. I recommend that in the first sentence following equation (1) the authors explicitly define $n_d=n_d(t,d_p)=dN/dd_p$. Other forms of the distribution function are often used (see, for example, Figures 4, 5, 7 of this paper).

2. Sentence following Equation (5): should be "down to $Kn\approx0.5$", not "up to $Kn\approx0.5$". The approximation is valid in the high Kn (low $1/Kn$) range.

3. Line 27, page 7: "The correction results in a 5.5% increase in CS, in accordance with Fig. 1." I suggest rewording this sentence. Figure 1 shows that for Kn>0.5, the approximate expression for CS exceeds the full expression by an amount ranging from 0 to 8%. So the direction is consistent and the observed 5.5% discrepancy is consistent. That's all we know for certain.

4. p. 8, line 1: "reflects the error due to approximation." The paper addresses several approximations. The text should be clarified to specify which approximation is responsible for the large differences in this case.

5. p. 8, line 5: "(see Fig. 4, right panel, at 00:30 to 02:00, when the aerosol mode characterized by low number concentration but large particle diameters sporadically appears)." I have gone back to look for this several times in Figure 4. It is not apparent to me.

6. p. 9, line 2: "feature makes is difficult" should be " feature makes it difficult"

7. Figures 4, 5 & 7 compare values of CS calculated from "measurements" (green lines) with those calculated from "theory [i.e., Eq. 16]" (red lines). For clarity the authors should revise this terminology for the following reasons: (1) both of these calculations require the use of measurements, (2) the results calculated directly from measurements (green) are more theoretically correct than those calculated from the simplified model (red). I believe this is shorthand language the authors used to communicate among themselves, but which is not helpful for the reader.