

***Interactive comment on* “Cloud condensation nuclei properties of model and atmospheric HULIS” by E. Dinar et al.**

Anonymous Referee #1

Received and published: 3 March 2006

The paper presents a new study on CCN properties of atmospheric HULIS, presently identified as a major component of aerosol particles in many different areas . The manuscript discusses useful results on the effect of molecular weight and acidity of humic like substances on cloud activation and uses the Köhler equation to compute dry activation diameters at different supersaturations. The predictions are then compared with measurements performed with a Thermal Gradient Diffusion Cloud Chamber. In particular this paper shows, for the first time in literature, the CCN activity of HULIS extracted from real atmospheric aerosol samples and compares it with the CCN activity of standard SRFA, commonly used as surrogate model compound . There are however some limitations: 1)the use of a constant surface tension (measured at a constant con-

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centration of 1 g/l and thus not dependent on the carbon concentration of the growing droplet) 2) the application an “infinite” solubility to the different HULIS fractions in any concentration range (this is at least my understanding ,since no specific solubility for the different fractions are indicated or discussed in the text). I agree with the comments of the Anonymous Referee #2 expecting much higher concentration than 1 g/l in the growing droplets and underlying that this can strongly change surface tension effect on activation . I would add, that this much higher concentration in growing droplets can also strongly limit solubility of HULIS . So I think that the authors should discuss this limits and stress that this paper is a valuable tool for a relative comparison between SRFA and urban atmospheric HULIS

In addition some minor changes should be addresses

1) Please, state in the abstract that the investigated aerosol is of urban type (influenced by smoke conditions..) : it seems that different aerosol types, smoke and urban aerosol, were collected

2) It should be an error in the Abstract line 15- 17 , please rephrase it: activation diameters cannot be input parameter for accounting activation diameters ! .

3) Introduction, pag 1075 line 14-16 : please change “solubility” with “number of moles in solution ” which depend on both solubility and molecular weight.

4) Please change the title of section 2.2. in “Molecular weight fractionation of \ddot{E} ”. Size fractionation is misleading.

5) Fig. 7: please add in the caption a short explanation of the legend.

6) Table 5: please add the units of activation diameters.

10) Figs. 5a, b and c can be replaced by one figure with symbols for measurements and different lines types for the different simulations . The same is valid for Figs. 6 and 7. This also makes possible the evaluation of the impact of different parameters directly on the graph.

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11) I suggest a check the English by a mother language speaker , since the text, in some parts, is quite complicate to" follow".

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 1073, 2006.

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