Atmos. Chem. Phys. Discuss., 7, S3504–S3506, 2007
www.atmos-chem-phys-discuss.net/7/S3504/2007/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "The effect of H_2O adsorption on cloud-drop activation of insoluble particles: a theoretical framework" *by* R. Sorjamaa and A. Laaksonen

Anonymous Referee #2

Received and published: 30 July 2007

In this work the authors discuss a theoretical framework for the activation of insoluble, completely wettable particles. They assume water vapor adsorption to be the dominant mechanism for droplet growth and propose to replace the solute term in Köhler theory using an adsorption isotherm. The paper is fairly well written. The subject is of relevance for the scientific community and lies into the scope of ACP; however some very important issues must be addressed before the manuscript can be published.

The most important issue is that of timescales. It is important to clarify that it is thermodynamic equilibrium to what Köhler theory refers to, rather than droplet growth. Thus the authors technically are not describing droplet growth but rather a three-phase equi-

librium which they propose to be driven by the physical adsorption of water vapor on a solid surface. This is a significant distinction as it leaves the kinetic contribution to the growth of droplets unresolved. Considering this, a question can be raised on why the authors didn't explicitly address the results of Mahata and Alofs (1974, as cited in the paper) where they reject the idea of water vapor adsorption to be important at atmospheric conditions on the basis that it is a very slow process? In other words, what is the time scale of the water vapor adsorption process, and is it relevant for droplet formation?

The authors proposed that the theory may work for organic, completely wettable, insoluble compounds; no attempts however were made to address issues on activation of insoluble organic particles to which a substantial part of the introduction was devoted. Two models of adsorption were tested, however the connection between the physical basis of these models (and specifically the assumptions behind them) to the activation process is not discussed. Therefore, the selection of the two models seems somehow arbitrary.

The study shows the FHH model to work better than the BET model at describing the onset of activation. This can be expected as there is an extra parameter in the FHH theory that allows more flexibility in reproducing experimental data. However the physical reasons for this are not clear, i.e., why an extra parameter is needed? is there anything incompatible in the assumptions of the BET model that would preclude its application in this case? This goes back to the fundamental question of what is the meaning of the parameters A, B, and c and how are they related to the properties of the insoluble CCN.

The authors conclude that the water activity (solute) term can be described by "any" multilayer adsorption model however they show BET not to be appropriate. What would be the requirements of an adsorption model to work well in this case?

Technical Issues

ACPD

7, S3504–S3506, 2007

Page 8142. Line 23: the abbreviation CCN was not previously introduced Page 8144. Line 16: double negation “not entirely impossible” (better say “possible”) Page 8145. Line 16: use “CCN” for consistency Page 8145. Line 27: references should be provided. Page 8145. Line 29: for better understanding surface coverage needs to be defined before equation (1) Page 8146. Line 7. What “potential field” means in this context? Page 8146. Line 19-20. the symbols aw and S have been already introduced and should be used for consistency. Page 8147. Line 5. This definition should go before introducing the adsorption models

---

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 8141, 2007.