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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Hygroscopic properties of Amazonian biomass burning and European background HULIS and investigation of their effects on surface tension with two models linking H-TDMA to CCNC data" *by* E. O. Fors et al.

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The authors extract HULIS from atmospheric samples collected at two different geographical locations (Brazil and Hungary) during different season and conditions. Using a fixed extraction procedure by which they isolate "HULIS" material, they subject the extract (reforumlated into aerosol) to subsaturated and supersaturated hygroscopicity studies. The methods of analysis by which they characterize the extract material is by 1) representing subsaturated growth by a set of single-parameter models of water uptake, 2) using volume additivity to compare with and tease out relative contributions of aerosol components (defined by elution method) to hygroscopicity, and 3) estimating ef-

fective surface tensions of HULIS aerosol. The work is an important contribution to the atmospheric science community in its effort to characterize and understand the contribution of various chemical components to aerosol water uptake, and the manuscript is well-written. This reviewer recommends the manuscript for publication with minor modifications.

p. 26931 - What is meant by "and known portions of the isolated organic matter were re-dissolved in water prior to H-TDMA and CCNC measurements"?

p. 26391 - Are there possibly compounds chemically dissimilar to HULIS that can be mis-classified as HULIS using this extraction procedure?

p. 26940 - There is an implicit assumption about the solubility of HULIS, though oftentimes the HULIS often discussed in the community is limited to a water-soluble subset of humic-like substances (as discussed most prominently by Rudich and coworkers). As HULIS in this manuscript is defined by extraction protocol (requiring methanol elution) instead of chemical properties, the authors should state up front some assumptions or supporting arguments regarding the expected solubility of their HULIS in water, particularly after the extraction procedure is discussed or before the various representations of the Kohler model are introduced. While the single parameter formulations used in the manuscript effectively combine dissolution and dissociation of the solute, it is not clear that the original form of the Kohler theory from which they are derived fully captures the effect of low or limited solubility on the volume of water in the aerosol (see for instance Roberts et al., 2002, and Huff Hartz et al., 2006), or in the expression of surface tension (partial dissolution will also affect the way Szyszkowski-Langmuir equation is parameterized on particle volumes, even without regard to surface-partitioning effects). Without a priori knowledge about the solubility of HULIS material (and other components in the system), it is difficult to separate the solubility effect from surface tension modification in a parameter estimation approach as used for this study. The authors note this along with the possibility for delayed deliquescence by some components which makes predictions for CCN activation challenging, but could perhaps

expand this discussion a little more (especially regarding the indication that the HULIS "most often were dissolved").

The authors should include a discussion of the work by Wex et al. (2009) and possibly Petters et al. (2009) to provide context for predicting CCN activity based on hygroscopicity parameters derived from subsaturated grow conditions.

Figures 2,3: These are hydration curves? From context this is clear, but the authors mention the H-TDMA setup also permits efflorescence measurements in the text.

Semantics/grammar:

Define acronyms for field campaigns (e.g., LBA-SMOCC)

p. 2631 - "In this work the ISOM fraction is hereafter referred to as HULIS, in consistency with literature" - which literature? Also, Ziese et al. (2007) is not included in the list of references and should actually be Ziese et al. (2008).

p. 26935 - "That approach has the drawback of requiring growth factors at three or more RH values, in contrast to the other models requiring growth factors at only one RH." I would not call that a "drawback" so much as an inconvenience, perhaps, but should in general yield a more robust estimate of the parameter to be estimated. However, it is noted that the requirement for more measurements can be unfavorable under certain circumstances (though not this one?).

p. 26936 - "The critical water vapour supersaturation, sc, (Sc-1) can be approximated by the 10 following equation is determined by the approximated expression of the Kohler theory, similar to that derived in Seinfeld and Pandis (2006):" (rephrase)

p. 26937 - "and [phi] is the osmotic coefficient."

p. 26936 - "The critical water vapour supersaturation, sc = (Sc-1), can be approximated"

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 26925, 2009.