

Interactive comment on “Uncertainty in aerosol hygroscopicity resulting from semi-volatile organic compounds” by Olivia Goulden et al.

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The manuscript “Uncertainty in aerosol hygroscopicity resulting from semi-volatile organic compounds” by Goulden, et al. is quite long and difficult to read, in my opinion. A complete rewrite with an eye toward tightening (an overall decrease of 25% seems possible and desirable) and clarity of language and logical construction would probably greatly improve the readability. The paper conducts a sensitivity analysis (a relatively mature mathematical method) of an existing set of parameterizations for cloud droplet formation. The core is a standard non-volatile aerosol activation model by Nenes which the authors have extended to allow co-condensation of SVOCs resulting in an apparent increase in the hygroscopicity of the “dry” particles (in some cases because the core particle is made more hygroscopic by the inclusion of the SVOC and in all cases – even those where the particle is made less “water loving” – because the particles are larger

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during the cloud updraft). In the interest of full disclosure, I am more of an experimentalist than a modeler, but I think the work on the parameterizations is very important and interesting, while this treatment of the aggregate uncertainties of the model is less so.

Some specific areas that I would focus on, if it is determined that a major rewrite is needed are: 1) As noted above, the concept of varying input parameters over likely ranges and determining the sensitivity of the resultant products (S_{max} , D_{min} , κ) to this is not foreign to most readers, so a terse explanation and tabulation of the ranges used would probably suffice. I would particularly recommend minimizing the discussion surrounding the core model (Sec. 3), where few new physical insights were produced. 2) A separate discussion of the modeling (Fig. 3 and surrounding text) with and without SVOC effects seems unwarranted – I would submit that a more concise discussion of the full implementation that notes the logical intermediate “off ramps” explored in this paper would probably be easier for most readers, even those unfamiliar with the concept. 3) In my opinion, there is too much discussion of the intermediate test cases (e.g., Knocc) and much of it is presented in an odd “event drives cause” manner that I found pervasive throughout the manuscript, for instance “For levoglucosan, the mixing rule has a less dominant effect than the increase in size at cloud base ...” 3a) The presentation of the levoglucosan results should be strongly caveated, since the results are apparently contradictory to the general thrust of the paper. Clearly this is an extreme case where a very hygroscopic core is exposed to a relatively less hygroscopic SVOC and the final product is still an apparently easy to activate particle. 4) Finally, and probably most importantly, I would recommend more/clearer discussion of the proposed use of an “effective hygroscopicity” in parameterizations used in larger scale models. It appears to me that the authors recommend simply “adjusting” the hygroscopicity of well-characterized particle types upward to account for the SVOC/water co-condensation, apparently without regard for the amount or nature of the SVOC that the aerosols are likely to have been exposed to. In my opinion, this makes as little sense as not accounting for the co-condensation in extant models and will probably

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result in a significant overestimation of the cloud formation and importantly also the sub-critical water uptake, resulting in a distortion of the optical properties. If this isn't what the authors are suggesting, I believe they should clarify this point.

Because the subject matter of this manuscript is of clear importance (although I do not think the work here is central to that effort, as it seems to be offering little new physical insight) I would think it is publishable. But I highly recommend an effort at recrafting it to make it a tighter, easier to read paper.

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