

Interactive comment on “Measured particle water uptake enhanced by co-condensing vapours” by Dawei Hu et al.

Anonymous Referee #1

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I have been reviewing this paper for Nature Communications and was surprised that it was not accepted. The authors have reformatted the text to a ACP style and resolved most of my concerns in this version.

This study investigated the role of co-condensation of organic vapors on the water uptake which has important implications for the aerosol-cloud interactions and their climate effects. The authors have cleverly designed their experiment and provided an experimental proof of the importance of co-condensation processes, which is an impossible mission for conventional HTDMA or CCN counter. Overall, it is an interesting and convincing study that may help improve our understanding on the cloud formation processes. Previous work was properly referred and the paper was clear and well written. I would strongly recommend its publication if the authors could address a couple

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of minor issues listed below.

(1) More details needed for the model configuration: Fig. 1 shows that Sorg was not measured, so how was it calculated? How the evaporation of water vapor and organic vapor from the bath was treated in the model? How the loss on the glass reactor walls was considered? It seems that the sample flow has an initial RH of 0.0 but a non-zero Sorg on entry to the reactor (Fig. 1), should Sorg also be zero? Ln 292, "The Sorg profile was used to optimise the fit between simulated and measured GFs.", the authors should give more details, how you get the Sor profile and how to use it for the optimization.

(2) Thermodynamic or kinetic effect? Ln 294, " RH rapidly reaches equilibrium (~ 20 s) in the glass reactor, while Sorg needs longer (~ 700 s) (corresponding to 15 m glass reactor). This is expected since water is more volatile than propylene glycol, and water vapour lost to the walls will be more rapidly replenished from the solution than the propylene glycol vapours" Here again, how to know that Sorg needs longer time if there is no measurement of it? The authors attribute the difference to different volatilities. I'd rather think the difference is due to different kinetics (diffusion, accommodation coefficient, etc) because a difference in saturation pressure should not change the equilibrium time.

(3) Abstract, it appears as if a kappa of 2.64 is for atmospheric relevant conditions. I am not sure if the authors want to say that. I would suggest the authors either to change the formulation or justify it.

(4) Figure 4 panel a, what's the meaning of the symbol in the middle of the figure (Sorg panel)? .

Reference: Murphy, D. M. and Fahey, D. W.: Mathematical Treatment of the Wall Loss of a Trace Species in Denuder and Catalytic-Converter Tubes, *Anal. Chem.*, 59, 2753–2759, 1987. Li, G., Su, H., Li, X., Kuhn, U., Meusel, H., Hoffmann, T., Ammann, M., Poeschl, U., Shao, M., and Cheng, Y.: Uptake of gaseous formaldehyde by soil

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Interactive comment on *Atmos. Chem. Phys. Discuss.*, <https://doi.org/10.5194/acp-2018-586>, 2018.