

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

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RE: Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2018-586>

Dear Editor,

Please find our reply and revised manuscript in response to the comments of the 2 reviewers. We are most grateful for their constructive suggestions. The manuscript “Measured particle water uptake enhanced by co-condensing vapours”, has been revised according to reviewers’ suggestions.

The manuscript is a revised submission with new line and page numbers in the text, with all changes marked in red bold. We confirm that the submission of this revised version have been approved by all of the authors listed on this manuscript.

Referee #2 (A. Laaksonen):

RC2: This paper presents an experimental study of the effect of three different organic vapors on ammonium sulfate particles' equilibrium growth at varying relative humidities. As such, it relates to the co-condensation effect in cloud drop formation, whereby semivolatile organic or inorganic vapors add soluble mass to an aerosol population undergoing cloud drop activation, which can result in enhanced cloud drop number concentrations. I find this study a welcome addition to the literature, and have only a few comments, mostly relating to past work.

In the abstract, it is stated that the “. . . enhancement of particle water uptake through co-condensation constitutes the first direct measurement of this process. . .” Similarly, in the end of Section 4 it is claimed that the authors have “observed for the first time that co-condensation of organic vapours can significantly promote water uptake of aerosol particles. . .” I don't think these statements are quite correct. Wagner and coworkers have published results of binary vapor condensation rates (both water-nitric acid and water-propanol) from which the co-condensation enhancement can be directly seen. See Rudolf et al., J. Aerosol Sci. 22, S51, 1991; Rudolf et al., J. Aerosol Sci. 32, 913, 2001.

RC2 answer: We thank the reviewer for providing the additional literatures which related to our work. We have cited them in Line 60-61 on Page 3. We think the reviewer is right, and we have modified the related sentences.

Line 10-11 on Page 1, the sentence “Until now, there has been no direct observational evidence of this process” was modified to “Until now, there has been very few direct observational evidence of this process”

Line 28-30 on Page 2, the sentence “This enhancement of particle water uptake

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through co-condensation of vapours constitutes the first direct measurement of this process, which. . .” was modified to “This enhancement of particle water uptake through co-condensation of vapours constitutes the direct measurement of this process, which. . .”

Line 59-61 on Page 3, the sentence “There has been no previous direct measurement evidence for this process in either inorganic or organic systems and existing instrumentation . . .” was modified to “There has been very less previous direct measurement evidence for this process in either inorganic (Rudolf et al., 2001) or organic systems (Rudolf et al., 1991) and existing instrumentation . . .”

Line 423-424 on Page 18, the sentence “This current study has observed for the first time that co-condensation of organic vapours can significantly promote water uptake of aerosol particles, . . .” was modified to “This current study has observed that co-condensation of organic vapours can significantly promote water uptake of aerosol particles, . . .”

RC2: CCN counter experiments and their explanation. I think the authors should refer to Romakkaniemi et al. (AMT 7, 1377, 2014) who studied the evaporation of ammonium nitrate and condensation of nitric acid inside the DMT CCN counter.

RC2 answer: We thank the reviewer for providing the additional literature which related to our work. We have cited it and add one sentence in Line 278-280 on Page 12. “the same result was also observed by Romakkaniemi et al. (2014) in their investigation of the evaporation of ammonium nitrate and condensation of nitric acid inside the DMT CCN counter”

RC2: It is said on lines 345-346 that the absolute magnitude of co-condensation depends on the organic saturation ratio and not the absolute concentration. I think it should be clarified here that this refers to equilibrium growth. At cloud drop activation, the absolute concentration of the co-condensing species matters a lot.

RC2 answer: We thank the reviewer for clarifying our statement. Yes, the reviewer is

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right, we should refer this statement to the situation of equilibrium growth of droplet. Line 347-348 on Page 15, the sentence “The absolute magnitude of co-condensation depends on the saturation ratio, . . .” was modified to “During the equilibrium growth of droplet, the absolute magnitude of co-condensation depends on the saturation ratio, . . .”

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