Authors' response to editor's comments for "Pre-deliquescent water uptake in deposited nanoparticles observed with *in situ* ambient pressure X-ray photoelectron spectroscopy"

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We thank the editor, Thorsten Bartels-Rausch, for his dedication to improving this paper. We address his comments below. In addition, we have updated some of the binding energy values that were not updated in the last revision of the manuscript after the new Monte Carlo analysis of the fits. The results of the analysis remain unchanged. We have also made some minor edits to improve the readability of the manuscript.

1. Please mention the possibility of adv.C in the NaCl samples and mention the impact on the attenuation (possibly on page 12-13). I feel that this is important as the audience of this journal might otherwise get the impression that adv. C. is unique to the organic samples.

   We have added a paragraph on the possible impact of adventitious carbon on the NaCl measurements in Section 3.1.3.

2. I still find the reporting of the binding energy shifts hard to understand for a broader audience. It appears that the relative shifts to 0 mbar are compared in absolute shifts for the different samples. My argument here is, that because the 0mbar spectra are less shifted for the organics as compared to the NaCl - for reasons related to the sample holder and/or the sample - the shift when introducing H2O vapour can not be as large as that observed for NaCl. In this understanding the H2O (adsorbed and in the gas-phase) "only" reduces the shift caused by charging - because it establishes a better charge transfer (conductivity) via the gas phase and via the interface. The BE can not change further than typical values for well conducting surfaces. I think we agree on this, don't we? Looking at Figure 3, I get the impression that the shifts you observe (at low RH) for the organics are already 100% of these expected changes in BE. Increasing the RH further, I would argue that you can not detect further changes in

BE even if more water would adsorb. I don't see this explicitly mentioned or discussed in the paper.

I kindly ask you to mention the reason for the BE shift at 0 mbar more explicitly so that a non-expert audience can follow and then shortly elaborate on the aspect I tried to mention above. I think the reader needs to clearly see how much the 0 mbar BE shifted (due to charging), how much the BE can thus change with increasing RH (going back to the uncharged value) and how much change you observed.

We now explain in Section 2 how binding energies can be shifted relative to literature values due to charging and how this shift can be counteracted with the introduction of a gas phase. For the NaCl measurements, we now note in Section 3.1 that the binding energy we report at 0 mbar is shifted relative to literature values due to charging of the silicon substrate. In Section 3.1.1, we discuss how the introduction of water vapor is expected to reverse some of the charging observed at 0 mbar. For the sucrose and malonic acid measurements, we note that the high conductivity of the gold substrate makes it unlikely that the binding energies will be affected by charging.