

Interactive comment on “Significant contribution of organics to aerosol liquid water content in winter in Beijing, China” by Xiaoai Jin et al.

Anonymous Referee #1

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Attached please find the comments.

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The manuscript “Significant contribution of organics to aerosol liquid water content in winter in Beijing, China” present a filed study in 2017 in Beijing China, focusing the aerosol liquid water content (ALWC). ALWC was derived from using the growth factor (GF) measured at 90% RH at three particle sizes in nucleation mode, Aiken and accumulation mode. ALWC was also calculated using ISORROPIA II model with the chemical composition characterized by AMS as input. It was found that generally $ALWC_{\text{model}}$ correlates with $ALWC_{\text{measured}}$ at high RH, but not at RH<60%. Including $ALWC_{\text{org}}$, the ALWC contributed by organics, i.e., $ALWC_{\text{corrected}}=ALWC_{\text{org}}$ improved the correlation with $ALWC_{\text{HTDMA}}$, especially at lower RH. The contribution of organic to total ALWC was 30+/-22%. ALWC correlated with mass concentration of sulfate, nitrate and SOA. Accumulation mode was found to contribute the largest portion to ALWC. In case study, ALWC contributed by organics may play an important role in the initial stage of haze event.

The manuscript addresses ALWC, which an important parameter in atmospheric chemistry. The manuscript fits the scope of ACP. I have several comments which need to be addressed before the manuscript is considered to be published in ACP.

General comments

1. The manuscript emphasized the role of organics in total ALWC. The value fraction 30+/-22% has a large uncertainty, suggesting a large variation of the contribution and likely a much smaller contribution of organics total in some cases. Also the conclusion is in contrast with Z. Wu et al., 2018, showing dominant role of inorganics in ALWC, as the authors also cited. The variations and the difference from the literature needs to be discussed in the manuscript.
2. Some important details are lacking (see specific comments).

Specific comments

1. Line 77 “...its factors...” is confusing. Please specify.
2. Line 156-159, at which three sizes the GF were measured?
3. Line 162, the assumption of constant k in each mode may result in uncertainty in ALWC. It may be worthy discussing this lines 237-253.
4. Lines 237-253, for the chemical composition measured by AMS, do the authors use bulk composition or size resolved composition? This can also contribute to the ALWC derived by ISORROPIA.
5. Lines 204-227, in deriving $ALWC_{\text{org}}$ among inorganics, only sulfuric acid, ammonium sulfate, ammonium hydrogen sulfate, and ammonium nitrate were considered. Actually chloride is also present, as this study found. The can result in uncertainty in $ALWC_{\text{org}}$.
6. Line 281, how is b derived?
7. Line 283, how are POA and SOA derived? Did the authors use PMF?
8. Section 4.2.2, Fig. 4 does not provide new information to the discussion. The accumulation mode contributed dominant hygroscopic materials and ALWC to total ALWC. Naturally, the correlations are better.
9. Line 323-325, such a statement is not necessarily true because many “processed” mineral particles are highly hygroscopic, such as $\text{Ca}(\text{NO}_3)_2$, or MgSO_4 , as shown by many field and laboratory studies.
10. Line 321, why upper limit of Aiken mode is set at 110 nm?
11. Line 320 (and 370), I am not sure whether using PNSD is proper here. The ALWC depends on the amount of hygroscopic materials, no matter which sizes they are present.

Fig. 1.

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