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.


The manuscript “Significant contribution of organics to aerosol liquid water content in winter in Beijing, China” presents a field study conducted in Beijing, China focusing on 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$_{HTDMA}$ correlated with ALWC$_{ISORROPIA}$ at high RH, but not at RH<60%. Including ALWC$_{org}$, the contribution of organics, i.e., ALWC$_{ISORROPIA+ALWC_{org}}$, improved the correlation with ALWC$_{HTDMA}$, especially at low RH. The contribution of organics to total ALWC was 30±22%. ALWC correlated with mass concentrations 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 events.

The manuscript addresses ALWC, which is 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 in some cases. Also the conclusion is in contrast with Z. Wu et al., 2018, showing a dominant role of inorganics in ALWC, as the authors also cited. The variations and the difference from the literature need 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$_{org}$, 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 contribution and the difference from the literature need to be discussed in the manuscript.
6. Lines 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 Ca(NO$_3$)$_2$ or MgSO$_4$, as shown by many field and laboratory studies.
10. Line 321, why upper limit of Aitken 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.