

The authors have made some revisions that improve the statements and discussions in the manuscript. However, my main concern regarding the concept and treatment of aerosol mixing state remains unresolved. In their responses to the major comments, the authors repeatedly refer to the meaning of mixing state index and its applications, which is already described in the manuscript and also a known concept for the reviewer, but this does not directly address the core issue raised.

It should be clearly recognized that the mixing state index is derived from HTDMA measurements at specific particle diameters. Therefore, it quantifies the mixing state of aerosols at those diameters, rather than representing the mixing state of the entire aerosol population. In contrast, the activation ratio (AR) reflects the activation behavior of the entire aerosol population. These two quantities are conceptually different and not directly comparable. The manuscript should explicitly clarify this distinction.

Furthermore, when discussing the influence of one factor on another, it is necessary to first isolate the effects of other contributing factors. The AR of the entire aerosol population is determined jointly by aerosol size distribution, chemical composition, and mixing state. It is important to keep in mind that when aerosol mixing state is on the basis of known mass concentrations of different aerosol species. Therefore, to rigorously quantify the impact of mixing state, the effects of size distribution and chemical composition must be first controlled or isolated.

Finally, I understand that the authors aim to convey that aerosol mixing state exhibits substantial variability and that an empirical relationship between AR at a given supersaturation (SS) and the mixing state index may exist. If this is the case, the term "covariation relationship" would be more appropriate than "impact", as the current analysis does not establish causal influence.

Some suggestions are listed below:

- (1) The calibration results of HTDMA and CCN should be presented
- (2) Calculation method of critical activation diameter should be presented.
- (3) Change "summer measurements" to "measurements during summer periods", the short campaign could not represent summer.
- (4) Change usage of words, for example "impacts" to "covariation relationships", "Dependency of the critical diameter ( $D_{cri}$ ) on the  $\chi$ " in Figure caption of Fig.8 to "Covariation characteristics of  $D_{cri}$  and  $\chi$ ". Please also check other places with these suggestions.

Suggestions on the response:

Please respond more directly to the question. It is difficult to identify what specific changes have been made to address the points I raised..

Give an example :

L267-268, biogenic origin is inferred from what clue? At least add the reference  
Re: Revised and See **Lines 319-325**: "...In winter, the Gf-PDF diurnal profiles of both Aitken and accumulation mode particles showed bimodal distribution (Fig. 4e2-g2) as evident by the number fraction of nearly-hydrophobic and more hygroscopic modes (Fig. S5). The NH mode was likely to be the anthropogenic organic matter and biogenic origin from marine mass (Xu et al., 2020), especially for the Aitken mode. The more hygroscopic and sea salt mode was mostly contributed from the nss-sulfate and sea salt in winter (Xu et al., 2021a) ..."

If you have included the requested information or references, please highlight them clearly. For example, highlight the specific clue or the added reference in the revised manuscript. It is very time-consuming to compare the new version with the previous one without clear indications of the changes.

Finally, feel that many of my comments are not directly addressed.