

Interactive comment on “Chemical composition and mass size distribution of PM_{1.0} at an elevated site in central east China” by Y. M. Zhang et al.

Anonymous Referee #1

Received and published: 26 June 2014

This manuscript reports about aerosol chemical measurements made at an elevated site (Mt. Tai) in China. The measurement data covers practically all the seasons, and altogether 123 measurement days have been considered. In principle, the gathered data could provide valuable scientific information on mass size distributions of different chemical compounds in the aerosol phase, on the air mass transport pathways for aerosols, as well as on organic aerosol composition. Unfortunately, the paper lacks a deep and proper scientific analysis of the results. Therefore, I cannot recommend acceptance of this paper in its current form. My major criticisms in this regard are summarized below.

First, sections 3.3–3.5 contain the most valuable new information from the conducted measurements. While these sections report the main findings, they lack clear scientific
C4126

conclusions. The few attempts toward this direction have often erroneous interpretations. For example, the authors link low aerosol mass mean diameters to nucleation events (lines 3–5 on page 15200). It is true that less aged aerosols tend to have a smaller mass median diameter, but nucleated aerosols are very unlikely to be responsible for the small mass median diameters. It is rather the other way around: air masses with lower mass median diameters tend to be younger/cleaner, making nucleation more probable. As another example, I do not understand how aerosol hygroscopicity would contribute to its growth (lines 12–13 on page 15200).

Second, I am not fully satisfied with the selection of sites for the comparisons made in sections 3.1 and 3.2. Why this set of sites? I would have liked to see comparisons to aerosol chemistry at other elevated sites all over the world, rather than picking up e.g. urban sites from here and there. One more thing: while the first part of Table 1 contains a short description of the type of site, the second part of this table does not!

Third, the introductions (section 1) have multiple problems. The first paragraph and the beginning of the second one in it are very difficult to understand. More specifically, it remains unclear how the beginning of the introduction motivates the research made in this manuscript. The introduction does not state clear scientific goals for this work either. It is too modest to state that the purpose is to assess regionally representative concentration levels. . . and obtain seasonal variations.

Finally, no figures have been presented on the actual results (all the information is in Tables). This makes it very difficult for a reader to digest the results.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 15191, 2014.