

Interactive comment on “One-year observations of carbonaceous and nitrogenous components and major ions in the aerosols from subtropical Okinawa Island, an outflow region of Asian dusts” by B. Kunwar and K. Kawamura

Anonymous Referee #2

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This manuscript describes long-term measurements of OC, EC, WSOC and major ions in atmospheric aerosols from Cape Hedo, which is located in the northwestern edge of Okinawa Island (Japan) and in the outflow region of Asian dusts. Although the site was used before for air pollution studies, the manuscript describes the most extensive and comprehensive set of measurements about aerosol chemistry performed so far at Cape Hedo. Results are interesting and improve our knowledge about the composition of the atmosphere in southern Japan and about air pollution transport from China to the North Pacific. Interesting to note in this study is the time variation of aerosol composition

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which was strongly dependent on the seasonal variation of air trajectories and type and strength of emission sources. The overall quality of this study is good. The scientific approach is sound and the text is well written and is easy to follow. I recommend publication of the manuscript in ACP after the following comments are addressed.

Specific comments:

Line 219 - I have doubts whether these two sources can explain the spring maxima for OC and WSOC because photochemical activity and biogenic emissions from plants normally peak during summer. Emissions from fossil fuel and biomass burning in China cannot be neglected as significant sources of OC and WSOC.

Line 228 - Solar radiation alone cannot explain the high SOC levels during summer. What are the sources of gaseous precursors? Back-trajectories suggest an ocean source of VOC's. Is there any evidence of significant marine VOC sources in the area? The authors should explain it better.

Line 234 - Airborne pollen is not expected to be as important in summer as it is in spring. The increase in the contribution of WIOM to TSP seems to be the result of a lower abundance of inorganic species during summer.

Line 271 - This statement seems to be unfinished. The authors should clarify the usefulness of WSOC/OC ratios.

Line 291 - The argument that “the lower WSOC/OC ratio may be caused by the sea-to-air emissions of water-insoluble organic compounds that are produced by marine phytoplankton” must be sustained by references to previous studies about these emissions.

Line 296 - Section 3.2.3, describing water-soluble ionic components, should be placed after comparison of EC and OC with previous studies in East Asia (section 3.2.4).

Section 3.2.3 - How do you explain the seasonal variation of MSA-? A comparison with other studies describing the seasonal variation of MSA- or DMS over or near the

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Pacific waters would be interesting.

Lines 315 to 326 - Discussion about OC and EC concentrations should take into account that the size of sampled particles was different. TSP was sampled at Cape Hedo and PM2.5 or PM10 were sampled at the other sites. Therefore, a simple direct comparison is not possible.

Line 409 - How relevant are shipping emissions in the East China Sea? Are they significant contributors to nssSO₄²⁻ concentrations at the Okinawa sampling site?

Technical corrections

Line 209 - The authors want to say “figure 4” instead of “figure 3”.

Line 349 - The authors wanted to say “section 3.2.7”

Line 777 - What is the meaning of UF in figure 5?

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 22059, 2013.