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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Characterization of biogenic primary and secondary organic aerosols in the marine atmosphere over the East China Sea" *by* Mingjie Kang et al.

Anonymous Referee #1

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This manuscripts reports on molecular composition, abundances and spatial distribution of sugars and biogenic secondary organic aerosols in the MABL of East China Sea (ECS). A total of 51 samples were collected in May-June 2014, day-time N=25 and night-time N=26. Authors state that (page 21, line 647) the contribution of each source changes over time and varies with distance from the continent. In this context, concentrations of the measured species are not representative on temporal and spatial scale for ECS and NW Pacific. Although the reported data set provides some novelty, there are number of issues with the discussion and interpretation presented by the authors.

For a general reader, authors should clearly state constituents referred as secondary organic aerosols (SOA) measured in this study. At times authors are addressing these constituents as "biogenic SOA tracers" and/or "biogenic SOA markers". This is confusing to a general reader.

Title: "Characterization of biogenic primary and secondary organic aerosols—-". What primary biogenic aerosols are being referred and measured in this study? Authors have mainly discussed biogenic secondary aerosols.

Abstract, lines 29-30: Authors state that "Biogenic SOA tracers and sugars exhibit higher levels in the samples affected by continental air masses, suggesting the terrestrial outflows of organic matter to the East China Sea". This is a redundant statement. If high concentrations of measured species are associated with continental air masses, then continental outflow of organic matter is much obvious (rather than "suggesting the terrestrial outflows of organic matter to the ECS").

Abstract, line 31: Concentration of glucose given is given as 0.31 ng/m3. Is this concentration significant to 2nd decimal unit?

Abstract, line 32: should read as "All sugar compounds show higher (not showed) ——

Abstract, lines 33: The concept of night-time and day-time variability in concentrations is misleading. This cannot be attributed to long-range transport from continental sources. This needs to be adequately addressed in the discussion section. There are high concentrations of total sugars in day-time samples.

Abstract, line 34: English!!! —— one high-oxidation tracer——

Abstract, lines 35-36: How high abundance of only two species is attributed to transport of aged organic aerosols through long-range transport?

Abstract, lines 36-38: What are the errors associated with Fungal-spore-derived, sesquiterpene-derived and biomass-burning-derived OC?

Abstract, lines 39-41: A very obvious conclusion!!! What else is expected other than transport of continental aerosols along the cruse track over ECS? What are biogenic OCs?

Page 4, lines 117-118: English!!! “The ECS is susceptible to the outflow of continental OM ——. Oceanic regions downwind of pollution sources are influenced by the chemical species.

Page 5, Section 2.2: No details are provided for the determination of total OC in aerosol samples collected over ECS.

Page 9, lines 272-273: Based on observed correlation one cannot conclude that marine sources contribute to the particulate glucose over ECS.

Page 9, line 274: English!!! ——fructose was obviously related to glucose——

Page 10, lines 299-300: —suggest that the major source of SOA tracers over the ECS is probably of terrestrial origin. A very obvious inference to make from measured SOA tracers.

Page 16, lines 489-490: What are the errors associated with fsoc used in the study? What is their significance to 3rd decimal units?

Page 16, lines 496-500: What is the rationale of invoking photo-degradation by free radicals in the atmosphere or other atmospheric dilution mechanism during long-range transport to the western North Pacific? Higher concentrations of biomass burning OC are not an evidence to invoke the process of photo-degradation. What is “other atmospheric dilution mechanism”?

Page 19, lines 594-597: Authors should have used Mg2+/Na+ ratio for deriving contribution from sea-salts due to chloride depletion in MABL. The measured SO4 should be corrected for contribution from sea-salt (to use non-sea-salt SO4)

Page 20, lines 605-609: Likewise, Ca2+ should be corrected for contribution from sea-

salt (nss-Ca2+). The anthropogenic sulphate does react with crustal aerosols (mineral dust containing CaCO3) to form Ca2+ and SO4 ions but does not form metal sulphate CaSO4 or MgSO4. CaSO4 is a primary mineral – gypsum.

Page 21, lines 635-638: English!!! Previous study once pointed out ——. Transport pathway could be similar for NOx and BSOC.

Page 21, line 647: If the contribution from each source changes in space and time then the concentrations measured for only few days (N=21 samples) over ECS are not representative.

Page 21, lines 652-654: This is conceptually wrong. Contribution from sea-salt is not an evidence for oceanic emissions of BSOA.

Page 21, lines 654-656: What is primary and secondary OM? For the study site, continental sources are dominant is a primary objective of carrying out sampling over ECS.

Page 22, lines 662-663 and lines 670-672: These are very obvious conclusions. What else is expected other than contribution from continental sources.

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