Reviews for acp-2017-288-version4

This is the second review of the manuscript entitled “Long-term (2001-2012) trends of carbonaceous aerosols from remote island in the western North Pacific: an outflow region of Asian pollutants and aging”. The revision is ok and can be acceptable for publication in ACP.

However, in this title the phrase “and aging” is little bit absurd. It is suggested to remove it. As a suggestion, the title “Long-term (2001-2012) trends of carbonaceous aerosols over a remote island in the western North Pacific rim: an outflow region of Asian pollutants” will be fine hopefully.

And little minor revisions (i.e., addition of couple of suitable references) are still needed as follows before the final and formal acceptance of the manuscript as follows:

Line 403: “EC scatters the short-wave incoming solar radiation less than OC………”
Although it is well known still the authors need to include some suitable references

Ex: Magi, B. I., 2009. Chemical apportionment of southern African aerosol mass and optical depth. Atmos. Chem. Phys., 9, 7643–7655.

Magi, B.I., 2011.Corrigendum to “Chemical apportionment of southern African aerosol mass and optical depth” published in Atmos. Chem. Phys., 9, 7643–7655, 2009. Atmos. Chem. Phys., 11, 4777–4778.

Line 405-407: “The single scattering albedo (SSA), defined as the ratio of scattering to the extinction coefficient of aerosols, is an important property for determining the direct RF (Pani et al., 2016).”

It is suggested to add here the reference “Pani et al., 2016a” also

Pani, S.K., Wang, S.H., Lin, N.H., Lee, C.T., Tsay, S.C., Holben, B.N., Janjai, S., Hsiao, T.C., Chuang, M.T. and Chantara, S. (2016a). Radiative effect of springtime biomass-burning aerosols over Northern Indochina during 7-SEAS/BASELInE 2013 campaign. Aerosol Air Qual. Res., 16, 2802–2817.

Good Luck.

Revision #2

- Marine Aerosol

The authors did tone down the language of the marine discussion some to use "suggests" instead of "shows", so it is better. But, it is still flawed, it not a main point of their work, and it should probably just be removed.

The authors state that the larger ratio of WIOM to WSOM is an indicator of fresh organic matter from the ocean (because the cited works associate WIOM with primary marine emissions). But then, WIOM, which is supposedly primary, does not correlate with sea salt (not to mention the lack of wind speeds high enough to actually produce primary marine aerosol). The authors state that the lack of correlation then suggests that the source of organic is still from the ocean but from a source independent of wind speed and sea salt. This would then be the secondary organic which is associated with WSOM (according to the cited papers), making the whole argument circular and contradictory.

The authors should state that this may be from a marine source but that it is far more likely that it is from a non-marine source. There may be some marine particles mixed with those from other non-marine (anthropogenic) sources. It is incredibly difficult to perfectly separate marine air masses and sample only marine emissions. The only evidence that these particles are marine are the back trajectories from over the ocean.

Additionally, there is no explanation on how Na+ was measured. If it is from a different paper, it should be still discussed briefly here.

--CCN Discussion

Where did the concentration of sea salt come from? If this is the same that was mentioned as calculated from Na+, this should be noted here. It is also unclear that "sea salt is not a major source of WSOC". Should this instead be "sea spray" or "ocean sources" instead? Sea salt is not a source of WSOC in general.

The new discussion is better in that it shows the correlation of sea salt to CCN, in addition to WSOC to CCN. The slight increase in the correlation with WSOC+sea salt might actually be the most interesting point here. Together, they represent a larger fraction of the actual particle mass and may influence CCN activation. (The text states $r = 0.69$, and the figure says $r = 0.68$.) However, it is clear that the sea salt concentrations control that correlation, even when WSOC is added, since WSOC concentrations are so much smaller.

It is good that the authors noted that particle size is not included here, but more discussion should be added – size plays a really important role in CCN activation.

-- Grammar

This paper still needs to be edited for proper grammar. Some additional suggestions are below (but there are many more that need to be addressed).

L. 403: Remove "that" from "less than OC and that EC"

L. 317: Remove "the" from "source for the both sea salt"

The following sentences are still unclear and need to be rephrased or edited.

L. 247-248: "This result suggests that the dominance of SOA in carbonaceous aerosol over the western North Pacific."

L. 301-303: "On the other hand, lower ratios of WSOC/OC in summer may suggest that the primary emission of OC from the ocean surface via sea-to-air flux due to the dominance of pristine marine air masses."