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Atmos. Chem. Phys. Discuss., referee comment RC2
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Comment on acp-2020-1247

Anonymous Referee #1

Referee comment on "Regional characteristics of fine aerosol mass-increase elucidated by long-term observations at a Northeast Asian background site" by Saehee Lim et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2020-1247-RC2, 2021

General Comments:

This manuscript describes in situ observations of aerosol size distributions and composition at a rural site on the Korean peninsula, with occasional vertical aerosol concentration information provided by nearby instrumented balloon launches. The work highlights two characteristic daily patterns of aerosol size distributions, a new particle formation EOF and a haze or accumulation-mode dominated EOF. A key question raised by the authors is what drives the development of high PM2.5 loading in this region, and the size distributions observations are compared with meteorological conditions as well as physico-chemical observations of black carbon aerosol.

My main comment is that the argument that meteorological differences define the two EOF features is not well-supported by the analysis presented in the paper. In fact, the meteorological description of the periods is not consistent with the description provided on the same measurement period in this work, which is cited once in the present work:

Peterson, DA, et al. 2019. Meteorology influencing springtime air quality, pollution transport, and visibility in Korea. Elem Sci Anth, 7: 57. DOI: https://doi.org/10.1525/elementa.395

Peterson et al. characterize the period of May 17-22 as "stagnation under a persistent anticycle" and the period May 25-21 as "dynamic meteorology, low-level transport, and haze development", whereas in the present work (to the best of my understanding) that earlier period is described as "persistent anticyclone" (associated with EOF1) and the 2nd period as "synoptic-scale stagnation" (associated with EOF2). There seems to be a disconnect here. The caption of Figure 6 is consistent with the Peterson paper, but the abstract and perhaps the rest of the present manuscript are not. Furthermore, to my (perhaps untrained) eye, the meteorological patterns plotted in Fig 4a. and 4b. do not appear to be very different. Both appear to be fairly dynamic, quite distinct from the stagnant/blocking pattern shown in Fig. 4c. of Peterson et al.

A related issue is that the terms EOF1 and EOF2 are used fairly loosely in the manuscript. I understand them to be defined by a statistical treatment of the size distribution data and refer to two specific patterns of aerosol size development over a day. But these terms are used to represent actual time periods as well, e.g. in Figure 4 where the geopotential height averaged over EOF1 and EOF2 is given. What time periods are actually represented

there? Are they EOF1 and 2 periods over the multi-year data set or during the KORUS-AQ measurement priod? I advise the authors to use different terms to define time periods in the multi-year data set and the KORUS measurement period.

I found the discussion of black carbon coating thickness as a useful diagnostic tool for the prevalent aerosol formation processes to be a very interesting concept and well-supported by the observations presented.

My bottom line for publishing this work in ACP is that the authors need to either do a lot more work showing the relationship between the characteristic aerosol EOF periods and synoptic scale meteorology, or they need to significantly de-emphasize claims of a relationship between them in the paper. In any case the time periods described need to be more clearly defined and not always tagged simply as EOF1 or EOF2.

Specific comments:

Separating the figures from the captions makes the figures difficult to review.

line 72 seems to imply all aerosol particles start from nucleation. Suggest rephrasing.

line 76 suggest change to "the level of pre-exisiting particles". As it stands, "a level" seems to imply that there's a minimum threshold of CS to achieve NPF, and I suspect that's not what the authors mean.

Line 169 and Figure S4. How were EOF1 and EOF2 periods determined? Is it just chance that 143 days each were found, or was that purposeful? Is there some threshold PCA value that causes a given time period to be included in the EOF1 or 2 bin?

Line 177 not sure what exactly is meant here. Are there >10^4/cm3 particles when only considering 20-30 nm particles?
Line 188. Same comment as line 177.

Line 193 "It turned out..." This sentence is very broad and isn't immediately supported by the details of what is meant so it seems out of place.

Line 243. What is meant by a mid-low cloud base height?

Line 251. Can you elaborate on why you consider EOF2 to correspond to "stagnant" conditions? To me this implies that in EOF1 there may be higher windspeeds, but this was not observed according to Table 1. In general, I find I am not convinced about the clear meteorological differences between the two cases. To my eye, the geopotential height and wind vector plots look fairly similar for the two EOF cases. This issue arises in Table 2 as well, where the boundary layer is just described in words without any analysis.

Line 308. "burst of particle(>3.5 nm) above 10^4" needs to be stated more clearly, at least give units for the concentration.

Line 313-314 "number of >3.5 nm particles tended to be backed up"- not sure what backed up means here.

Line 306-318. It may be helpful to define a particle size range of >3.5 nm to 0.3 um. It's a little confusing talking about >3.5 nm particles (which includes the 0.3-0.5 um and 0.5-1.0 um particles) as distinct from these other size ranges. I understand most of the number in the >3.5 nm particles must be below 300 nm, but you could make this

paragraph significantly clearer by removing >3.5 nm particles and including >3.5-300 nm paticles as a size class.

Line 356. How would the weather conditions have suppressed condensation of volatiles onto particle surfaces? Please be more specific. The temperature was lower during EOF1, which seems like it would support more rather than less condensation.

Line 364-366. The claim in the second sentence is a big claim and it does not follow from the first sentence in this paragraph. It is an interesting claim, and I would encourage the authors to expand upon it. What number fraction of the particles is made up of BC particles? If they were not present, what would happen to the materials that would otherwise condense on them?

Figure 4. Maybe the continent outlines could be in a thicker pen? It's a little hard to make them out. Please give units for the geopotential height. What timescale do these back trajectories cover? Please state that as well.

Figure 7. It's a little difficult to know how to compare the sizes of the circles and squares (i.e. volume vs width). Maybe alongside the scale for the circle size vs. coating thickness you could do the same for the squares in the EOF1 case.