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Review of Lim et al.

Anonymous Referee #2

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-RC1, 2021

<General comments>
This paper discusses the formation mechanism of aerosols in northeast Asia. The authors used in-situ data obtained in Jeju Island from January 2013 to December 2016 and those during KORUS-AQ (May-June 2016) to investigate the variability in aerosol concentrations in the boundary layer. They used the empirical orthogonal function (EOF) analysis to classify the observed features. While the main topic of this paper is suitable for ACP, I do not think that the main conclusions are fully supported by observational evidence. I suggest that the authors largely reorganize the results and discussion and clarify the robust and new findings from this study. I recommend major revisions.

<Specific comments>
Introduction
The formation of aerosols associated with meteorological cycles (e.g., anticyclones, cyclones, and air stagnation) has been extensively studied either in continental source areas or downwind regions. Variability in vertical profiles of aerosols associated with the evolution of the boundary layer has also been extensively investigated. Although this study might be the first to present such data in Korea, I think the fundamental mechanisms are common in many cases. The authors should briefly review previous studies in northeast Asia (e.g., TRACE-P, CARE-Beijing) and also in other regions, and discuss the similarity and difference between this study and previous ones. Here are some examples of the previous studies in northeast Asia.
Weber, R. J., et al. (2003): J. Geophys. Res., 108, 8814, doi:10.1029/2002JD003112.
Matsui, H., et al. (2009): J. Geophys. Res., 114, D00G13, doi:10.1029/2008JD010906.
Takegawa, N., et al. (2009): J. Geophys. Res., 114, D00G05, doi:10.1029/2008JD010857.
Haenel, A., et al. (2012): J. Geophys. Res., 117, D13201, doi:10.1029/2012JD017577.
Wang, J., et al., (2019): Atmos. Chem. Phys., 19, 8845-8861, doi:10.5194/acp-19-8845-2019.

The following review paper would also be useful for the interpretation of NPF events in relation with meteorological conditions.
Kerminen, V.-M., et al. (2018), Environ. Res. Lett., 13, 103003. doi:10.1088/1748-9326/aadf3c.

L144-146: Uncertainty in the coating thickness
The estimation of coating thickness of BC particles from SP2 data, although it has been used by many investigators, may contain significant uncertainties. The authors selected a BC core diameter of 200 +/- 20 nm. Why did the authors select this specific diameter? Is it reasonable to estimate a coating thickness of > 10 nm with the core diameter uncertainty of 20 nm?

L249-251: Boundary layer stability
The dominance of a high-pressure system (subsidence) generally leads to the formation of strong inversions and stable boundary layers. The description in this paragraph seems to be opposite.

L259-269: Entrainment
The authors conclude that the rapid increase in PM2.5 was due to the entrainment of particles from upstream areas. The authors state that elevation of aerosol concentrations is "believed" to occur by the intrusion of pollutants from the upper atmosphere. What is the basis for this statement? I do not think that the descriptions in this paragraph are supported by observational evidence.

L306-318: Gas-to-particle conversion.
The discussion in this paragraph is highly speculative. The partitioning between HNO3 and NH4NO3 should be explicitly investigated to discuss the gas-to-particle conversion for nitrate aerosols. See, for example, Neuman, J. A., et al. (2003): J. Geophys. Res., 108, 4557, doi:10.1029/2003JD003616. Furthermore, the formation of (NH4)2SO4 might be controlled by aqueous-phase reactions in cloud droplets rather than condensation processes. Please reconsider the interpretation.

L353-366: Interpretation of the coating thickness
The authors suggest that the coating thickness of rBC is s useful parameter to understand the formation of secondary aerosols, and also suggest that reducing BC emissions is the effective way to reduce PM2.5 in Asia. I think the descriptions in these paragraphs are also very speculative and not supported by observational evidence. Fig. 7 seems to the basis for this hypothesis, but I find many data points at lower aerosol mass loadings with thick coatings. It may be true that the EOF2 case can be characterized by high PM2.5 and thick coatings, but it does not necessarily mean that the coating thickness is the controlling factor. I would guess the correlation between the PM2.5 concentrations and the coating thickness is rather weak. Please show more convincing data to support the hypothesis. Otherwise I recommend that the authors should remove (at least tone down) this conclusion.

<Minor comments>
L73-74, L182: SO2, NOx, and VOCs are not "condensable" gases but precursors.

L100-121: Please specify the model number of the SMPS, CPC, and OPC. Please also describe how these instruments were evaluated and calibrated. It is not necessary to capitalize the first character of the name of the instruments.

L178-179: I do not think the estimate of GR values has three significant digits.

L331: "peaking below 100 nm" - Please specify number, surface, or mass.