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Interactive comment on "Alteration of the microphysical properties of black carbon through transport in the boundary layer in East Asia" *by* Takuma Miyakawa et al.

Anonymous Referee #1

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The manuscript discusses ground-based measurements, with several instruments, of black carbon (BC) near an industrial source region and at a location removed from the source to study the effects of precipitation on the size distribution and properties of the BC-containing particles. The manuscript is well written and competently explains the study, but several of the arguments do not seem supported by the data. If the comments below are addressed I would recommend that the manuscript be accepted for publication.

The title refers to "microphysical properties," which is true, but perhaps "size distribution and amount of associated non-BC material" would be more accurate, as the former term implies a host of properties that were not addressed.

Line 56: The sweeping statement that "washout cannot substantially affect the lifetime of atmospheric BC-containing particles," even with a reference to Seinfeld and Pandis, seems difficult to justify. Do the authors mean that because most of the BC-containing particles have diameters of several hundred nanometers, their ability to be scavenged by falling precipitation is not very large? This would seem to depend on the intensity of precipitation.

Line 148: Rather than "lower and upper boundaries" it would be preferable to state "outside the diameter range . . ." so that it is clear what size is being referred to.

Lines 152-154: Some discussion of why the EC and rBC concentrations differ, and especially why the rBC concentration is less, seems to be necessary.

Line 168: Some justification for the selection of 0.5 as the collection efficiency for sulfate in the ACSM is required.

Line 206: Some discussion of how sensitive the results are to different choices for the percentile (i.e., does the background value change if concentrations lower than the 10th percentile were averaged?) would be helpful, or better yet, a distribution of the CO concentrations should be shown.

Line 277: The statement that the ACSM-SO4 and the IC-SO4 "generally agreed well" is true, but from Fig. 5c there appears to be little variability in either at concurrent times when comparison could be made.

Line 284: It is not clear why the positive correlation of SO4 and CO suggests that the SO4 was secondary and that SO4 contributed to the BC coatings; more explanation of these assumptions/conclusions is required.

Line 290: The authors note "the small variability of SO4/CO ratios," yet Figure 6b shows that these ratios vary considerably.

Lines 294, 297: The two "experiments," which consisted of two brief time periods out of a month of data, were used to justify conclusions regarding flow patterns. While the results are indeed consistent with the arguments made, it seems difficult to justify such conclusions on the basis of one comparison.

Line 317: The authors refer to the SO4/CO ratio, but does this really refer to the delta-SO4/delta-CO ratio? It was unclear to me here and a number of places elsewhere in the test whether the CO and SO4 values referred to delta-CO and delta-SO4 values or not. For clarity, I would recommend using "delta-" values throughout.

Lines 317-319: The difference in slopes shown in the inset to Figure 6b doesn't seem sufficiently large, given the scatter of the data, to be significantly different, and certainly not to justify the conclusion that the controlling process is rainout.

Line 343: Here and elsewhere the argument is made that aging leads to growth of BC particles, which is well accepted, but such aging can also lead to loss of larger particles through rainout, het size distributions in Figure 7 doesn't show much of a difference between size distributions for air masses with BC loss and those without, and certainly not more of a difference for larger BC particles than for smaller ones. This discrepancy requires explanation.

Line 345: The statement that "small BC-containing particles were scavenged by larger particles in the coagulation process" is a hypothesis, but stated as truth. It would seem that concentrations are too low for much coagulation over the brief period (a few days), especially for particles that are many tens of nanometers in diameter. Calculations or a simple model would be required to support this hypothesis.

Line 353: It would be preferable, and less ambiguous, to rephrase "BC size of 0.2" to "BC diameter of 0.2".

Line 368: The discussion focused on transport pathways of particles in the particular region of the study, but I was expecting more discussion on the results, what they mean, and so forth. There seemed to be little relevance to the second paragraph of the discussion.

Line 372: The decrease in the peak diameter of the mass size distribution is very small, and within uncertainty.

Line 373: The statement that the evidence implies selective removal of large BC-containing particles is not supported by Figure 7, which shows a very slight difference in the size distribution between "with BC loss" and "without BC loss" but not apparent selective decrease of larger particles. If there were selective removal, I would expect the size distribution to not be lognormal, but to have a deficit on the large side below what a lognormal would be.

Figure 3a is very difficult to read; could it be made larger? Figure 3b requires units for q_v to accompany the scale. Figure 4a should be made larger also, if possible. Figure 5b: it is difficult to distinguish the COSMOS and SP2 BC values; perhaps make one red and the other black? Figure 6a: do the axes refer to delta-CO and delta-BC? If so, they should be labeled as such. Figure 6b, inset: what does "all data" refer to? If this is to label the gray dot, then it is not clear.

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