2nd Review of Petrenko et al., 2024, Reviewer 2.

The authors have adequately answered most of my concerns (going through the responses, I've marked most of them with "Ok", "OK, good.", "OK, good point", "Yes, this makes sense", etc). At this point it's down to the "very minor/technical comments".

I should mention that the first paragraph of the response had me wondering whether my suggestions/comments had been followed – though I could see they had been, in the more detailed responses which followed.

The minor technical issues that remain:

(1) Please modify "In addition to the frequent, global AOD and aerosol type that can be provided by satellite aerosol remote-sensing, this necessitates systematic aircraft measurements of detailed microphysical properties for the major aerosol airmass types near-source as well as during transport and aging" in the Conclusions." to replace the words "microphysical properties" to "microphysical and optical properties". I could see this had been done elsewhere in the revised text; the authors might have missed this instance.

(2) The Conclusions sentence "Other aspects of model treatment of aerosol microphysical and optical properties, such as size distributions, mixing states, hygroscopic properties, and MEE will also affect the BB AOD calculations, but these effects are expected to be less significant in the BB source regions" needs either an extension or another line where the authors explain why they believe the last part, "these effects are expected to be less significant in the BB source regions" to be the case. The statement appears to contradict other parts of the modified text, e.g. elsewhere in the Conclusions "This often points to differences in model treatment of physical and chemical processes such as plume injection height, aging time, removal mechanisms, and secondary aerosol formation, as well as aerosol microphysical and optical properties such as particle size distributions, mixing state, hygroscopic growth rates, and mass extinction efficiencies" – which seems to suggest that the latter considerations ->are<- important or could be significant.

(3) Table 1 helps address my concerns about mentioning the optical calculations carried out in the models. Two things worth adding, either in Table 1 or in the accompanying text: (a) was some form of Mie scattering (or a lookup table based on same) used for all models, and (b) please include in the text that the models using internal mixing all assumed a homogeneous mixture (mentioned in the Response to my comments, but not in the revised paper, I think). Also, can the authors please identify which of the references in the right-hand column of Table 1 include the optical calculation details (e.g. identify the optical calculation details reference in italics), for readers who may want to follow up on this.

(4) Page 8 of the Author's response to my comments mentions "…we are studying primarily smoke plumes close to the source (see more on how cases were defined and treated in the responses below), where dispersion and particle settling are less likely to be significant." With regards to particle settling – the authors may want to qualify the portion of the statement about particle settling – this is very dependent on the particle size. e.g. the deposition velocities (last stage of particle settling) for particles > 10 um diameter is 10 to 100 cm s-1. i.e. for the big particles, you have a substantial at-source reduction in

particulate matter (see for example Emerson et al., 2020 PNAS paper). I don't know whether any of the models include large particles, however.

(5) Injection heights – thanks for including the approximations used in Table 1. I'm surprised that none of these calculated plume height explicitly based on the energy release and the temperature lapse rate (cf the Anderson et al 2024 reference). I understand the authors point – at-source, the column total is less likely to be affected by this. I nevertheless wonder about a 6 km high plume versus a 1 km high plume being subjected to very different advection losses at the plume top.

(6) Thanks for the OA/OC discussion in the response and the revised Figure R2. – yes, this sort of thing comes out of doing comparisons like this (agree that's its an important result of the current work, that the OA/OC variation can affect models results).

(7) Line 273 response (page 11): can this portion of the response to the reviewer please be placed in the Supplement and mentioned in a line in the text?

(8) Final suggestion: can the authors please include their definition of Diversity the first time it appears in the revised text. In the original manuscript, it was in the Figure 8 caption (and I missed it, the first time around); their response to my line 468 comment made me do a search on Diversity – and in the revised manuscript it now appears before Figure 8, in Table 2. So defining it the first time it's mentioned, in the text, would help readers to connect the dots, and avoid the confusion I faced in trying to figure out how it was calculated.