**Atmospheric
Chemistry
and Physics
Discussions**

*Interactive comment on* "**Impact of the aging process of black carbon aerosols on their spatial distribution, hygroscopicity, and radiative forcing in a global climate model**" *by* D. Goto et al.

**Anonymous Referee #2**

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In this study, the authors examined how different treatments of BC aging process may affect BC spatial distribution, hygroscopicity and radiative forcing in a global aerosol model. This could be an interesting addition to the literature, as there are still large uncertainty in the treatment of BC aging process. However, I found that the experiments were not well designed, and the results presented also needs further clarification. I therefore can not recommend the publication in its current form.

1.Experiment design: The goal of this study is to examine how different aging treatment affects BC and its radiative forcing. However, in three numerical experiments, there are many differences other than differences on aging treatment. For example, size

distributions are different (the ones used in AGV and AGF are different from the ones used in ORIG). Refractive index of BC is also different in different experiments. Another difference is the mixing state. In AGV, BC and sulfate can be internally mixed, while in ORIG, BC and OC can be internally mixed, while in AGF, BC does not mix with either OC or sulfate. What is more, the initial mixing states are also different. In the AGV and AGF, the WIBC and WSBC compounds constituted 80 and 20% of the total BC emission in all sources, while in ORIG, the assumption on this are different (see lines 1-5, page 29811). Though these additional differences can be interesting by themselves, these make it difficult to interpret the results of the three experiments in the study. It is not clear to me whether the large differences in forcing comes from differences in aging treatment or differences in size distribution, refractive index, or different mixing state with sulfate or OC. I would suggest the authors to eliminate these additional differences and rerun their experiments.

2.The forcing efficiency calculation is quite interesting. However, more clarification is needed, especially in terms of changes in AOT (Figure 11) (see specific comments #22, 23, 24).

Specific comments:

1. p. 29804, lines 12-14: I would argue that, to date, not "some" but many global models take into account the internal mixing and related processes. Listed references are incomplete.

2. p. 29805, Croft et al. (2005): can you highlight the main conclusion of Croft et al. (2005) here? Also, it will be good to refer back to these two literatures when discussing BC burden changes from different aging treatment Section 3.

3. P. 29808, line 16: what is the fixed tau_bc for "AGF"?

4. P. 29810, lines 3-17: It is not clear to me why size distribution and density are updated in AGV and AGF case. I would think this introduced additional complexity

and make it difficult to interpret the differences among different experiments. Also see general comments.

5. P. 29810, line 12: the hygroscopicity of WSBC was the same as that of pure sulfate. Is this for all three experiments? This seems not a good assumption. How about sulfate predicted from Eq. (6)? Is there any difference between AGF and AGV on this?

6. P. 29812, lines 8-29: It is not clear to me how extinction coefficients and absorption coefficients are calculated in different cases. Here are several questions: i) what is the unit of extinction coefficient and absorption coefficients, /gram BC or /gram aerosols?; ii) how are these calculated for WSBC? Will these depend on how much sulfate predicted by the model? Eq. (6) suggested that sulfate can be different for different situations. In AGV, BC is internally mixed with sulfate, while in AGF, BC does not internally mix with sulfate. I would expect this will affect extinction/absorption coefficients in these two cases?

7. P. 29812, line 23: again, difference in refractive index introduces additional complexity.

8. P. 29813, lines 8-10: It is not clear to me how the model treats the internal mixing of BC and sulfate in AGV and the internal mixing of BC and OC in ORIG. Section 2.2.1 seems to suggest that WSBC has the same hygroscopicity as pure sulfate.

9. Section 3.1: I agreed with referee #1 that it will be helpful to show the actual tau_bc generated from AGV method.

10. Section 3.1: a table to compare BC global burden, lifetime, wet deposition rate, and dry deposition rate can be very helpful for readers to understand how three different schemes perform.

11. Figure 2: which aging method is used in model results in Figure 2?

12. P. 29814, lines 11-28: Here the paper presented the differences in sulfate BC mass concentrations among different treatments. But what is missing is the explanation what

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might cause the differences.

13. P. 29815, line 7: project

14. Figures 5 and 6: again, differences among different experiments warrant further explanations.

15. P. 29816, line 14: become smaller →become larger?

16. Figure 7a: the AGV method produces increasing WIBC ratio over the Artic (up to 70%). Is this feature realistic? I would expect WIBC ratio over the Arctic is similar to those over the remote oceans.

17. Figure 9: And why AOD in ORIG is smaller by more than 5% than that in AGV over many regions?

18. Figure 10a: the legend of color map is incorrect.

19. P. 29818, line 25: Fig. 10b →Fig. 9b?

20. P. 29819, lines 25-29: the difference in sulfate explained here again introduces additional complexity

21. P. 29820, lines 4-10: I am not convinced that you can draw this conclusion here. Again, differences other than aging process may play a role here. Also, I do not think the underestimation is because the compounds mixed with BC is ignored. If you mean the difference between AGF and ORIG, it is clear from Figure 6, that BC burden in ORIG is significantly higher than AGF.

22. Fig. 11: several questions here. i) why BC column burden are quite similar among different aging treatment for these 9 regions, while Figure 6 shows large difference? ii) delta AAOT are quite similar (this is consistent with BC burden change), while delta AOT are quite different among three aging treatment. What might explain this discrepancy? iii)the smaller positive forcing in AGF than in ORIG seems due to larger delta AOT in AGF.

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23. P. 29820, lines 22-24: again, I am not convinced by this this conclusion. Fig. 11 suggested to me that the difference in TOA forcing seems due to difference in delta AOT, but not from delta AAOT.

24. P. 29821, lines 11-14: I am not sure I agree with this statement. As I mentioned before, I would think this difference is caused by difference in delta AOT in three experiments, as shown in Figure 11. It is also not clear to me why attached compounds would increase beta n or beta s (noted that delta AAOT are quite similar for three different experiments, as shown in Figure 11).

25. P. 29822, line 27: the second AGV should be AGF?

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