

***Interactive comment on* “Black carbon ageing in the Canadian Centre for Climate modelling and analysis atmospheric general circulation model” by B. Croft et al.**

Anonymous Referee #2

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This paper is presenting a comparison of different parameterizations used in climate models to simulate the ageing of the black carbon particles by conversion from insoluble to mixed/soluble particles.

The work is systematic, nicely written and timely since to my knowledge such a comparison is not published elsewhere.

It is worth publication in ACP.

With some modifications, the paper can gain in clarity and increase its scientific impact.

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Page 1385, line 18: Cooke et al., 1999 gives an exponential lifetime of 1.15 days that makes a half-time of $\ln 2 \cdot 1.15 \text{ days} = 19 \text{ hours}$. This sentence needs to be rephrased.

Page 1390, do eqs (1) and (2) apply one to condensation and the other to coagulation? Please provide the physical meaning of the two terms, if any. Further in the same page, eq. (3) is given as more universal (valid for all internally mixed aerosol number concentration): Why this eq. is not used in the present study?

Page 1391, line 2. The spatially variability of the conversion of insoluble BC to mixed/soluble BC due to oxidation of the organic coating of BC by O₃ has been also studied by Tsigaridis and Kanakidou (2003) (article mentioned in the introduction of the paper- published in ACP in 2003 (1680-7324/acp/2003-3-1849 in ACP, 3, 1849-1869). How the results of the present study compare with that earlier one?

Page 1392, line 21: 'faster' or 'the fastest'

Page 1392, line 22: 'very fast day-time ageing time-scale' why day-time?: a few more lines providing measurable information (characteristic times) for the condensation of H₂SO₄ and HNO₃ on BC will be beneficial for the presentation the paper.

Page 1392, line 25: "small contribution of oxidation to the ageing process" How this conclusion on the relative contribution is derived? How sensitive is the result to the assumptions made in the model?

Page 1393, line 4, 'estimates' add 'of the' Page 1393, line 3, 'dominated' add 'by'

Page 1394, line 10, since the authors blame the high number concentrations calculated by the model on the choice of the mode radius adopted for the log-normal distribution, they have to justify their choice.

Page 1394, line 14, Please explain better the scenario used for the MODB-COND-COAG simulation.

Page 1394, line 19 and Figure 7. Please indicate the threshold ratio of 0.25 in the

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figure. This can be done with a contour line and will improve clarity.

Page 1394, line 24: of 'semi'-volatile organic gases.

Page 1396, line 7: Figure 10 is in logarithmic scale that does not show the difference as much as the liner scale of Figure 9. Please mention the use of log scale in figure 10.

Page 1396, line 8 and later in the discussion page 1397, line 3: IMPROVE data: I do not think that averaging observations over the USA and then comparing to the model results is providing any valuable information. I would suggest that the authors make a scatter plot of all observations versus the model results and then compare statistically.

Page 1396, line 15: FIX-LIFE2 simulation is not yet presented. The authors might consider moving section 5 (sensitivity to emissions) before section 4 (comparison to observations).

Page 1396, line 16-17: the discussion of model versus observations need to be rephrased and pay attention to the log scale of Figure 10; in addition to the NO-AGE simulation the FIX-LIFE2 is also many times far from the observations. Moreover, the mentioned factor of 2 might apply to the annual mean value but does not seem to apply to all monthly values.

Page 1397, end of section 4: How the model compares to observations at biomass burning sites? Could you also add some comments on the seasonal variations?

Page 1399, last sentence: This is a highly interesting result and deserves to be emphasized more and also to be mentioned in the abstract.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 1383, 2005.

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