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Comment

Interactive comment on "Interpreting aerosol lifetimes using the GEOS-Chem model and constraints from radionuclide measurements" *by* B. Croft et al.

B. Croft et al.

croft@mathstat.dal.ca

The authors wish to thank the second anonymous referee for the comments and suggestions, which have helped to improve the manuscript. Below is a point-by-point response to indicate how we have addressed each comment.

RC: The authors offered a clear discussion on various definitions of aerosol lifetime and presented corresponding values from GEOS-Chem calculation. . ..However, in many cases including the one discussed here, the equilibrium between source and sink is not established. Thus, a tau bearing specific temporal and spatial scales would have to

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be derived from the sink term, or [-C/tau]. All the derivations listed in Sect. 2 are mostly approximations better for cases with unknown sinks. However, with a model this would not be a problem. . .. The local sink would be easily calculated for all the corresponding grids, and thus the tau defined with different scales (whether mean or instant) would be obtained (assume dry deposition is negligible). In other words, once the precipitation distribution is known, all the arguments to explain the discrepancy of interest can be offered rather straightforwardly. For instance, the global mean tau commonly used in global models simply reflects an accumulation of all the grid-derived sinks.

AC: The equations presented in Section 2 have been re-formulated to provide a clearer description of our methodology. The approach suggested by this referee was included in our original methodology, but this is now more explicitly indicated with the revisions to all equations in Section 2. The revised text in Section 2 also more clearly indicates our use of the sink term. Additionally, we have modified the text related to Fig. 6 (last paragraph of Section 4.3) to explicitly indicate that the mean lifetimes presented are determined using the sink term. These simulated mean lifetimes are calculated with respect to wet removal.

RC: Page 32398, Line 15: Figure 2 seems showing the CTL integration results from time zero. My understanding is that the CTL should be a cold start run with zero initial concentration of 137Cs. If this is true, please indicate the time interval between the analysis time zero and the actual simulation time zero.

AC: Yes, you are correct. We have allowed a one-month spin up of the model after a cold start with near-zero initial 137Cs concentrations. Figure 2 is intended to only show the time since the onset of the radionuclide emissions. These start-up conditions are now indicated in the revised text of Section 3.1, first paragraph.

RC: Page 32399, Line 3-5, "Differences between. . .attributed to the efficient aerosol removal. . .close to the FD-NPP site": The absolute removal should be determined by how the precipitation and tracer distributions match each other. The total sink

strength within a given domain divided by the domain-average mole fraction would derive domain-average tau. I assume in the discussion the authors actually was referring to a global domain. So, here are two factors, one is the removal strength, another is the averaged abundance of the tracer (note that USFC is a pulse run that loads the total emissions once at all grids).

AC: Yes, the referee is correct that the discussion pertains to a global domain. We agree that the absolute removal is determined by how the precipitation and tracer distributions match each other. The revised text in Section 4.1 (third paragraph) now explicitly indicates that the global mean removal strength is different between the two simulations due to the different spatial distribution of the 137Cs over the first few weeks of the simulation relative to the precipitation distribution.

RC: Page 32401, Line 23, "we chose days 20-80 after the onset of emissions for the fit' this has not been explained clearly or I might miss some statements given in somewhere else. Is this a scale consistent with that of Kristiansen et al. analysis for some reason?

AC: This scale is used to provide a fair comparison of our simulated e-folding times with the measurement-based e-folding times presented by Kristiansen et al. 2012. We now indicate this clearly in the revised text of Section 4.1 (last paragraph).

RC: Figure 4, the authors have barely touched the issue of model-observation comparison (Table 3 seems indicating a clear discrepancy between the two), only comparisons between different model runs were presented.

AC: We agree that the issue of model-observation comparison is an important one. Indeed radionuclide measurements provide one of the most useful constraints on the wet removal of aerosols. This is highly relevant for global models. A more detailed model-observation comparison than our Table 3 is the subject of an upcoming manuscript that is in preparation with colleagues. The focus of this current manuscript is to explain the reasons behind mean aerosol lifetimes and e-folding time differences and to quantify

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these differences. We have revised the text to provide an indication of future work and now include an explicit discussion of the value of a more detailed model-measurement comparison. This can be found in the Conclusion section (fourth and also final paragraphs) and in the abstract (second paragraph).

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