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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Implementing Microscopic Charcoal Particles Into a Global Aerosol-Climate Model" *by* Anina Gilgen et al.

Anonymous Referee #2

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The authors present the technical implementation of charcoal particles into a global climate model, calibrate emissions using a test dataset, and then evaluate their initial model performance against a global set of observations.

Understanding microscopic charcoal emissions, transport and deposition is clearly a relevant topic, both for atmospheric modeling and for climate studies, and the construction of models capable of dealing with this class of particle is very welcome. The paper is thorough and well written, and well suitable for publication in ACP. I do, however, have some comments on the evaluation the authors perform of their model, and the conclusions they draw. Also, as with many technical papers, quite a number of statements and sentences are difficult to understand for a broader audience. Hence, I recommend major revisions before final publication.

Major comments:

My main concern with the paper is about the emission scaling and validation against observations. The authors state very clearly that their initial implementation of charcoal particles fails to capture the full range of variability in the observations. This is quite understandable, and improving this correspondence should a fruitful and important line of research in coming years. Looking at Figures 1 and 2, however, it's clear that both the scaling factor used for emissions, and the parameters chosen for density and mean/threshold radius, don't really affect the correspondence much. I understand that the parameters are guided by observations (section 3.1.2), but both the early figures and Table 1 show that varying them don't really change the correlation.

I believe that the reason for this is that there are physical processes, both in transport and charcoal retrieval, that are not represented in the model - as the authors also comment on. Hence, the whole discussion of parameters and emission factors could be toned down quite a bit. It's relevant as a sensitivity test, and should be mentioned, but as the paper stands it seems to indicate that some conclusion about the "best" parameter set can be drawn - and I don't think the numbers support that.

Further, the emission scaling is confusing. The numbers 34 and 40 are used interchangeably through the manuscript - probably indicating that the lack of wide range correlation with observations precludes a more precise estimate. I would think that sensitivity study of emission scaling factors would be as, or more, important than the microscopic parameters discussed - so here, I would encourage the authors to add a little bit more information on how the scaling was chosen. (Especially since the abstract states that a factor of approx. 40 matches the calibration dataset "best".)

In conclusion, I recommend harmonizing the detail level in the discussions of microscopic parameters and emission factor, and admitting more clearly that the lack of variability in the model results precludes drawing firm conclusions about either. The implementation itself is important enough to warrant publication.

Minor comments:

- Section 3.1.4: Interactions with radiation and clouds is a whole other topic, which is insufficiently covered by this section. E.g. the assumption about spherical particles will have large implications for radiative transfer. I recommend removing this discussion and taking it up more thoroughly in a later publication.

- p3,l1: Should be DM<10 micrometers for microscopic?

- p3,l7-8: "homogenised the variance of individual records with a Box-Cox transformation, rescaled the transformed data to the range (0, 1), and standardised it." This is a good example of a line that is too technical for its context. I recommend making the introduction more accessible to a broader audience.

- p6,l27: "The right-skewed histogram of Clark and Hussey"... is another example. Please explain, so the uninformed reader doesn't have to look though the references.

- p10,l1: Here, the authors describe an ageing process of charcoal particles, which likely influences wet deposition rates. What is the ageing timescale? How sensitive are the results (and the variability) to this parameter?

- p10,l21: "...nobody has measured..." Out of curiosity, could FireLab (https://www.firelab.org/) have performed some relevant experiments here? I saw something at a conference a while back, but can't quite recall the details.

- p11,l12: "...and some uncertain parameters..." Which ones? Please be specific. (It's described below, so it's just a matter of wording.)

- p11,l29: "Pearson correlation coefficients larger than 0.2." Is this the requirement for significance for your number of degrees of freedom? Please discuss the significance criteria in a bit more detail. (It's mentioned in a few places that the correlation becomes significant at the 5% level; according to what test? Which numbers are not significant?) This is related to my main comment above.

- p15,l25: Appendix B: Is there any way, based on the present data, to estimate the contribution of charcoal particles to the global absorption aerosol optical depth? Probably not with great precision, but this is a quite open issue. (See e.g. a recent review here: https://link.springer.com/article/10.1007%2Fs40641-018-0091-4)

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