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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.

Anina Gilgen et al.

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We thank Brian Magi for his helpful and valuable comments. Please note that we found a bug in the charcoal code, which affected the results. The following main points have changed:

- For the calibration data set, the Pearson correlations coefficients now show nearly no difference for different parameter sets (before: ranging between 0.21 and 0.32; now: ranging between 0.21 and 0.23).
- We chose a different parameter set with the new simulations, which has the highest variability (old parameter set: $remi2.5, rthr4.9, dens0.6$; new parameter set: $remi5, rthr4.9, dens0.6$).

- The emission factor that is in best agreement with the observations is now 250 (before: 40). This is both due to the error in the code and due to the different parameter set that we chose. We now discuss the effect of the parameter set on the estimated emission factor in the text.

Furthermore, based on the comments of reviewer 2, we decided to create a Supplementary Material, which now includes part of the manuscript and the Appendix. In the following, the reviewer's comments are shown in italic font and our answers in bold font.

A very interesting modeling study to incorporate charcoal particles into the aerosol model using relatively modern charcoal accumulation rates. The work may prompt some thought in the Global Charcoal Database community about how charcoal gets transported in a full circulation model and what that may mean for interpreting charcoal accumulation rates relative to burned area. The authors describe the strengths and limits of the modeling approach nicely. I only have minor comments below that aim at clarifying a few points. Otherwise the results and technical implementation are quite useful.

Thank you very much for your positive comments.

- *p 2, line 10-11 Great statement, but the awkward sentence needs attention: "Open questions still remain, e.g. regarding the complexity needed for global fire models (Hantson et al., 2016). Especially the anthropogenic influence on fires is difficult to simulate."*
- **We agree and deleted the sentence "Especially the anthropogenic influence on fires is difficult to simulate.", which does not really fit in the context here.**
- *p. 2, line 12 Emissions are not calibrated exactly, but rather scaled to modern day. See Van Marle et al 2017 which is probably a paper that should be cited in this paragraph https://www.geosci-model-dev.net/10/3329/2017/*

- **The term "calibrated" is actually used in Rabin et al. (2017), but we agree that the term might be too strong (especially concerning aerosol emissions). We changed the sentence to: "Current fire models are generally tuned to match observations from recent decades, where satellite products give valuable information on the occurrence of fires."**

- *p. 3, lines 4-6 again, the Van Marle et al 2017 GMD paper would be a relevant citation here*

- **We agree that this paper should be cited in the introduction. We added the following text: "To estimate fire emissions from 1750 to 2015, van Marle et al. (2017) combined satellite retrievals, standardised scores from charcoal records, fire models, and visibility observations. The charcoal signal and the output from the fire models were scaled to match average regional GFED (Global Fire Emissions Database) carbon emissions from 1997 to 2003."**

- *p. 3, line 6 This makes it sound like Power and Marlon papers are "circumventing" the problem of comparing with global fire models. Re-word so that it is clear that they are circumventing problems associated with non-standardized data collection methods in the GCD.*

- **We changed the text to: "To circumvent the problem of inhomogeneous data..."**

- *p. 3, line 15 Useful citation here may be https://onlinelibrary.wiley.com/doi/full/10.1002/env.2450*

- **We added this citiation.**

- *p. 8 Radke et al: interesting use of the results from this study!*

- *p. 8, lines 11-18 define the acronymn GFAS here; the text states that ratio of BC to submicron aerosol mass is 10 and that even BC emissions are likely underestimated by another factor of 3.4. Right now, this is confusing to me. I can see scaling up BC mass emissions as a starting point to simulate charcoal mass emissions, and I can see the extra factor of 3.4 or so arising from what may be an underestimate in BC mass emissions, but I cannot make sense of "are comparable to those of submicron particles and thus arrive at a factor of 10 based on the ratios of BC to total submicron particles and to OC". Please check the wording and clarify how SF = 34 is the starting point. Also, please clarify why SF = 40 is not used throughout the paper. For example, at line 18, why not add to the end of the last sentence "and we arrived at SF = 40 after an iterative calibration process."?*

- **We changed the text to make it clearer: "As a starting point for the scaling factor, we assume that the mass emission fluxes of microscopic charcoal are comparable to those of submicron particles. Since BC only contributes relatively little to the total submicron particle mass, we scale the BC mass by a factor $\approx$ 10 (based on the ratios of BC to total submicron particles and to OC; Desservettaz et al. 2017, Akagi et al. 2011, Sinha et al. 2003). Furthermore, scaling aerosol emissions from the Global Fire Assimilation System (GFAS) by a factor of 3.4 leads to a better agreement between simulated and observed aerosol optical depth for both the global Monitoring Atmospheric Composition and Change (MACC) aerosol system and ECHAM6-HAM2 (Kaiser et al. 2012, Hardenberg et al. 2012). Therefore, we use a factor of $10 \cdot 3.4 = 34$ as an initial estimate. Then we adjust this scaling factor until the simulated charcoal fluxes agree with the calibration dataset (Sect. 2.1)." Furthermore, as mentioned at the beginning, the best scaling factor is not 40 anymore. We now show in the supplementary material a plot with different scaling factors for the chosen parameter set.**

- *p. 9, line 19 change "which is like charcoal an inert and unreactive substance" to "which is chemically similar to charcoal" (if this is what you are trying to say)*
- **In our opinion, the term "chemically similar to charcoal" is too general since charcoal and black carbon differ considerably e.g. concerning their molecular composition. Therefore, we kept the words "inert" and "unreactive".**
- *p. 10 section 3.1.4 the section seems overly speculative and distracting given the main goal of the paper. I agree that it might be interesting if micro and macro char were INPs but it seems equally as likely that if charcoal injected above 4 km in figure A1 is rare, then charcoal participating in Bergeron-W-F process is essentially insignificant in the model*
- **We generally agree and shortened the section considerably.**
- *p. 10, line 18-19 how could absorption of light leading to convective lifting of 5-10 micron particles? Can this tiny number of giant particles relative to submicron aerosol really have a dramatic impact on thermodynamic profile? Again, I find this distracting in the context of the main point of the paper, and would suggest simply stating that there is very limited study of the possibility of charcoal as INP. or CCN.*
- **We shortened the section and added the following sentence to point out that the choices of refractive index and INP propensity have probably not a large effect on our results: "We do not expect that these decisions have a large impact on the atmospheric transport of charcoal particles since most charcoal particles do not reach levels where heterogeneous freezing becomes important and the absorption of charcoal particles is likely too small to change the thermodynamic profile of the atmosphere."**

- *p. 11, lines 24-25 define what ACCMIP. stands for, and explain what "calculated online" means since the paper crosses across communities of researchers who may not guess what this model jargon mean*
- **We defined ACCMIP and changed the text to: "Dust, sea salt, and oceanic dimethyl sulphide emissions were calculated within the model at every timestep."**
- *Conclusions: Several researchers in the Global Charcoal Database/Paleofire community published a study echoing some of the ideas in the conclusion that might be a useful citation supporting ideas around, line 10 on p. 15 https://www.sciencedirect.com/science/article/pii/S104061821630831X*
- **We mention now this reference in the conclusions.**
- *Figures 1-5 in the caption, I suggest stating what the threshold radius actually is set to in each caption. Currently this is sometimes done and sometimes not.*
- **We added the used parameters to each caption.**
- *Figure 3 is really interesting!*
- *Figures 6-7 Section 3.1.6/Appendix D are a very useful diagnostic filter for subsetting model output to better match charcoal, but while Figs 1-5 and 8 show results filters for sizes greater than the threshold radius, this is not the case for Figure 6-7. Please spec- ulate on roughly what fraction of the modeled burden might be due to the largest sized giant aerosols, and perhaps include this speculation as a part of the figure caption.*
- **We created additional diagnostics to calculate the contributions to the mass and the number online. Figures 6 and 7 (numbers in the old manuscript) now also show the mass/number of charcoal particles larger**

than the threshold radius so that they are directly comparable to the other figures.

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