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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Estimation of black carbon emissions from Siberian fires using satellite observations of absorption and extinction optical depths" *by* Igor B. Konovalov et al.

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We are very grateful to the Referee for the positive evaluation of our manuscript and for the useful comments and suggestions. Below we describe our point-by-point responses to the Referee's comments.

<u>Referee's comment:</u> *Sensitivity to SOA: Authors discuss this subject as a potential uncertainty, although they write that it is not represented in CHIMERE (pp11, line 10). Also authors write that uncertainties in SOA could affect the optimal estimates of F∧OC (pp 20, l16), they suggest it does not affect BC as long as 'simulated AOD values are fitted to the AOD observations'. Finally, on pp 27 l14 the authors suggest that this uncertainty could explain some of their differences in OC emissions compared to GFED. I am still not fully satisfied about uncertainties due to this aspect. It is well known that (biomass burning) SOA budgets are poorly constrained (e.g. Spracklen et al., ACP 2011). By not adequately representing them in CHIMERE there is at least uncertainty in dependency of the EC/OC ratio depending on the lifetime of the plume, with fresh plumes, with comparatively little SOA contribution to OC. Can authors please expand on this aspect a bit more? It would be very interesting if the authors are able to test this uncertainty through an actual sensitivity experiment where SOA contribution to aged [OC] would be enhanced.*

Indeed, the uncertainty in the SOA budget affects the EC/OC ratio and its dependence on the BB aerosol age. However, the corresponding errors in our BC emission estimates are mostly compensated by similar errors in the simulated AOD values. The discussion of this point is expanded in the revised manuscript.

In particular, the compensation of possible errors in the EC/OC ratio and AOD is demonstrated (see Sect. 2.2.4 and Eq. 11) by simplifying the original empirical relationship (given by Eq. 6) between the BC and OC column, AOD and AAOD. Furthermore, as suggested by the Referee, we performed a sensitivity experiment, in which the yields of all of the SOA species from oxidation of the major volatile SOA precursors (such as toluene, xylenes, isoprene and terpenes) were enhanced by a factor of 7 with respect to the base case, whereas the reaction list and the reaction rates (as well as all other simulation settings) were kept the same. As a result of this model modification, the relative enhancement of the averaged (over the whole period and region considered) particulate organic matter (POM) column amounts due to SOA formation increased from only 2.6 % (in the base case simulation with the optimized emissions) up to 27 % (in the test case). The increased SOA contribution to POM in our test case simulation corresponds to the upper bound of the wide range of the BB POM enhancements observed in aging BB plumes in several field studies in North America. This numerical experiment is discussed in Sect. 3.6 of the revised manuscript as the test case No. 2. We found that the optimal BC emission estimate changed rather insignificantly, having increased only by $\sim$ 5 %. It is not surprising, however, that our estimates of the OC emissions turned out to be much more sensitive to the SOA formation; specifically, the top-down estimate of the total OC emissions dropped by $\sim$15 % in the test case as a result of an increase in the BB aerosol abundances and a slight decrease in the mass extinction efficiency. We would like to note that, to the best of our knowledge, any aerosol modeling scheme which could ensure quite adequate simulations of the evolution of the organic fraction of BB aerosol in Siberia or elsewhere is still not available: presently, the elaboration of such a scheme is an active research problem.

Referee's comment: *Optimization: The authors compute a single monthly mean optimization factors F for the complete region. They find rather different values per month (Table 2). In part this appears to be associated to different type of fires, particularly for grass land compared to boreal forest fires (Fig. 14). I wonder if the method couldn't easily be expanded to optimize these scaling factors but for two sub-regions (e.g. below/above 57N), such that they can better be associated to particular fire types. It would be interesting to see if the scaling factors would then show a more homogeneous value for different months.*

Unfortunately, the optimization method employed in this paper could not be easily expanded to optimize the scaling factors for two (or more) sub-regions: we would need to use a different cost function, a different optimization procedure and perform more model runs. However, if a sub-region is sufficiently large, aerosol transport between the sub-regions can be disregarded, and the correction factors for the BB emissions in a given sub-region can be evaluated by using the same optimization method.

Accordingly, we performed an additional analysis using the AAOD and AOD observations only over a selected sub-region (50-57N, 60-115E) where the relative contribution of agricultural and grass fires to FRP was much larger and more uniformly distributed across the different months than in the whole region. The estimates of the F^{BC}/F^{OC} ra-

tio obtained for this sub-region show much smaller variations between different months, as compared to the corresponding estimates for the whole region, whereas monthly variations of the F^{BC} and F^{OC} factors themselves even increased (which may be due to monthly variations of the conversion factor). Therefore, our additional analysis supports (although does not prove) the possibility that the monthly variations of the F^{BC}/F^{OC} ratio are associated with different fire types and thus indicates that that the emission factors specified in the GFED4 inventory and in our simulation for either BC or OC or both are biased in case of at least one fire type. The analysis is described in Sect. 3.1 of the revised manuscript and in a new section (Sect. S3) of the Supplementary material.

Following the Referee's suggestion, we also performed a similar analysis for a sub-region above 57N. However, we found that a contribution of agricultural and grass fires to the integral FRP in May in this sub-region (32 %) was not much smaller than that in the entire study region (41 %). For this reason, the analysis of the second sub-region could not provide any conclusive results in regard to the question raised by the Referee and thus is not presented in the revised manuscript.

Referee's comments: *Technical comments:*

-pp11, l2: "Usually"-> "As usual"

- Konovalov et al. (2017a): Please check and complete reference details

-pp21, l17: "Figure 4"-> "Figure 5"

-pp22, l18: "an artifact of": suggest to change into "enhanced due to"

-pp24, l18 "also" (remove)

-pp26, l 29 "their": to what does this refer to? GFED?

-pp29, l24: "predicated"->"predicted"

-pp30, l26 "the mean AAOD": consider to change to "the mean observed AAOD"

-pp32, l1 “are” (remove)

We thank the Referee for these technical comments. All the suggested changes and corrections are introduced in the revised manuscript.

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