Atmospheric Measurement Techniques Discussions

Atmos. Meas. Tech. Discuss., referee comment RC3
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Reply on CC1

Anonymous Referee #1

Referee comment on "Inferring the absorption properties of organic aerosol in Siberian biomass burning plumes from remote optical observations" by Igor B. Konovalov et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2021-151-RC3, 2021

I am sorry that I did not like you paper and I am sorry that I did not provide you with sufficient material to improve the paper. I have a couple of comments on your comments that I hope will help you to understand my view.

Beginning on line 152, you
"... assumed that BB aerosol consists of spherical BC particles covered by a weaklyabsorbing coating."

You also say
"...The mass concentrations of the particle components were distributed among 20 size sections spanning the particle shell diameters from 10 nm to 10 um. The particle size distribution was assumed to be lognormal, unimodal, and representative of the accumulation mode. Taking into account that the contribution of coarse particles to the BB aerosol optical properties in the UV and visible wavelength ranges is likely small (Reid et al., 2005b), it was disregarded in our simulations."

Setting aside the confusing statement that you model particles in bins up to 10 um dia but then discaard the coarse mode (size cut for coarse mode unspecified), this is a set of microphysical properties that is not consistent with the AERONET model. That is fine, because the AERONET retrievals are ill-posed and there are undoubtedly multiple solutions to each set of extinction and radiance measurements. However, you have to do a little more work to convince readers that this as a viable approach, in my opinion. The strength of AERONET is that it is constrained by the radiance field; your model, on the other hand, does not have this constraint.

Now, I have thought about this after I sent my review (even before your latest note), and I agree with you that constraining your model with radiance measurements is too high of a bar. On the other hand, your only constraints are two pairs of AAE, and the SSA at *one wavelength*. Your Fig 2b indicates that the computed difference between the two AAEs is less than the expected RMS measurement error that you report (< 0.12), so I don't believe that the two AAEs provide any more information than a single AAE.

At any rate, the paper would be much stronger if you show your readers that the model provides results that are consistent with all of the AERONET single-scatter parameters. That is, demonstrating that you can obtain the AERONET extinctions at multiple wavelengths (thereby demonstrating that you are using a model with the correct AE, too) and AAOD at multiple wavelengths (which also demonstrates that you are getting the correct SSA when combined with AOD) would provide a convincing argument that your model is linked to the measurements. A comparison of the asymmetry parameters that you obtain to the AERONET asymmetry parameters would further strengthen your case. Right now, the reader has no idea if your model can produce the measured AODs, AEs, or AAODs at any wavelength, so there is no link to the original measurements; your model only reconciles a small subset of the available parameters.

I don't think that this is asking too much... You've already used Mie theory to compute AAE and SSA -- why not use the same output to verify/constrain the AODs, AEs, AAODs, and ASYs?

I hope that you are able to model a majority the AERONET parameters at multiple wavelengths (AODs, etc.) in a reasonable way -- then this paper will be the very first AAE paper to be properly constrained by AERONET. That would be a very significant first, in my opinion, and I expect that others will follow.

On a lesser point, words like 'absorptivity' and 'emissivity' have specific meanings that we learn in our radiative transfer classes. Yes, Saleh redefined this term, but they should not have done this, in my opinion. I doubt that I will be the only reader who is confused with your labeling the imaginary refractive index as the absorptivitiy, especially since you did not define the equivalence of these terms in your paper (like Saleh did). It does not help that you point to Sun (2007) for Eq 3, which essentially equates your 'k' with their absorption coefficient. Additionally, since the Sun paper presents a powerlaw equation for the absorption coefficient which can be solved for the Absorption Angstrom Exponent, I wondered whether you are using your 'w' as the AAE. I am still not certain if you are doing this or not, but it would be flat out wrong if you are -- that is, a powerlaw based upon the imaginary refractive index does not yield an AAE.

Note that there is nothing wrong with using a powerlaw for the imaginary index, but you have to be clear about your terminology. The imaginary index is not equivalent to the absorption coefficient, and neither are equiavelent to absorptivity. Such ambiguous terminology undoubtedly loses customers.

Regarding Eq 3, which you cite as "...the relative contribution of BrC to the total absorption at 440 nm (dBrC),...", you wrote:

dBrC = (MAE_tot - MAE_bc) / MAE_tot .

I would have written this as:

dBrC = (MAE_tot * Mass_tot - MAE_bc * Mass_bc) / (MAE_tot * Mass_tot) .

I'll leave it at that.

I spent a lot of time reading this paper and writing these two reports, so I hope that you are able to use my comments constructively. I think that there is much room for improvement in this paper and that there is potential for a very solid piece of work, but someone else will have to decide whether the next draft meets this criteria. I don't intend to read another draft of this paper, as I have already contributed more than my fair share.