

Interactive comment on “Spectral absorption properties of atmospheric aerosols” by R. W. Bergstrom et al.

Anonymous Referee #2

Received and published: 12 September 2007

I have reviewed this paper as well has read over the comments posted from other reviewers. As a whole I find this a very worthy manuscript which defiantly should be published. It is very concise and presents good ideas clearly. Scientifically I can find no flaws. The SSFR measurements of absorption spectral absorption is one of the few true measurements of absorption that can be used on aircraft.

My primary comment follows on a previous reviewer's that this is not entirely new work, but is rather a fusion of review and new data. This needs to be brought out more clearly in the introduction. To be sure, the idea of an absorption angstrom exponent; is by no means new. The new part in this paper is simply the diction. Going back over 20 years there have been discussions of the wavelength dependence of absorption. As I am sure the authors here are keenly aware, discussions

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always revolved around whether a particular species has greybody, λ^{-1} or λ^{-2} , which is the point of this paper. Typically, it is taken as λ^{-1} , which is precisely what they show from the Ron Brown. For biomass burning, there has been reports ranging from λ^{-1} to λ^{-2} . From the abstract, they are laying claim to a fairly well known concept. So what that is new is really being said?

To answer this question, here I think the author's have framed the issue better than has been done before. In fact, I think this paper would be a must read for new students to the field. I also believe that they provide the scientific community an infusion of much needed hyper spectral data that does much to prove their point. Between the two, they provide a λ^{-1} universality that has not yet been provided. But the paper needs to provide this context. In the current form that is not clear. Indeed, based on their data, absorption follows a power law better than the original angstrom law does for extinction (e.g., see Reid et al., 1999; Eck et al., 2001 which clearly demonstrates the wavelength dependence of the power law for scattering and optical depth). They should state this as the main point of the paper, drawing on a number of additional data sources or previous research in their introduction. A bit more of a discussion should then be included (instead of a few comments in the conclusion) which clearly state the benefits this constraint offers to the scientific community for parameterizations and such.

Anyway, this is simply my point of view. I hate it when reviewers try and force their editorial points of view on authors and I will not do so now. But I think this is worth thinking about.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 10669, 2007.

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