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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A case study on biomass burning aerosols: effects on solar UV irradiance, retrieval of aerosol single scattering albedo" *by* A. Bagheri et al.

Anonymous Referee #2

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The comment of Bagheri et al. to review 2 repeats the questions of the reviewer, but the an-swers are short. Only the answers for the minor suggestions are given in detail, but even here I do not agree with the one of the answers. Minor comment 2: in Diaz (2000) the height effect is mentioned to be between 2 and 4 % and not 25% as argued by Bagheri.

The idea of the paper by Bagheri et al., as it is mentioned in the title, was to study effects of biomass burning on UV irradiance and to determine its aerosol single scattering albedo. For the first part, the authors show the ratios of background and biomass burning aerosol conditions for direct and global irradiance (Fig. 2), measured with different

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instruments. Here all aerosol effects are included, i.e. effects of increasing aerosol amount (AOD) and of changing aerosol properties (SSA, scattering function), but also the differences in ozone amount between the two dates of measurement. The results, mentioned as most interesting observations by Bagheri at the end of page 17995, are typical for increasing aerosol load and thus well known (see Rev.1) and of course in agreement with the literature.

To use the DDR method to determine spectral values of SSA for an aerosol condition, be-sides the spectral AOD and albedo the diffuse irradiance must be known. (I do not agree with the statement that “the most significant information about retrieved SSA´s are not their abso-lute values but rather in their relative changes”. To use the results for other places and condi-tions, absolute values are of interest.) The diffuse irradiance, resp. its ratio, has been deter-mined by modeling, under the as-sumption of aerosol properties. Here for the background conditions has been used an aerosol type “urban”. This was (answer of Bagheri to general comment 7) because “the model produced a better match to measured irradiance with the urban aerosols”. However, with the decision for this aerosol type, already the SSA for the background aerosol is decided. If urban aerosol is used for the background conditions, the SSA must be low, as to be seen in Fig. 3. This approach explains the (astonishing ?) fact that the SSA for Trondheim aerosol (which should be a sea shore aerosol) has the same SSA than biomass burning aerosol. To clarify the conditions: What was the wind direction at the clear day? The DDR method should be done for individual wavelengths, to get spectral values of SSA. An Angstrom alpha may be an input parameter of the model, but is not of relevance for the DDR method. In contrary, the use of alpha may lead to erroneous results. The published ra-tio of the direct irra-diances (Fig. 2) shows remarkable differences between model and meas-urement for wavelength above 400 nm. This probably is an effect of the alpha assumption, which has been made for the modeling. A second (minor) point is that the properties AOD, height of aerosol layer and visibility are not independent. Thus it is astonishing to use them together for the model.

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Thus again: The first step should be to determine spectral values of AOD, taking into account ozone, Rayleigh scattering etc, which is no problem using the direct irradiances. These data should be shown for the clear and the turbid day, in absolute values, to see the different tur-bidity, and with respect to alpha, to see something about aerosol size distribution. Then the direct irradiance together with the global irradiance should be used to determine the diffuse irradiances for the two aerosol conditions. If this is not possible, due to different instrument sensitivity or calibration, do the job via the ratios. But then clearly explain what information can be taken from the ratios. As mentioned above, SSA is of interest, not the relative change of SSA between two days.

The revised version of the paper may clear many things up. But I felt a necessity to write this second review, because I got the feeling that in the first review I could not explain my re-marks in a way that they could be understood by the authors.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 17987, 2008.

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