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Interactive comment on "A simple parameterization of the short-wave aerosol optical properties for surface direct and diffuse irradiances assessment in a numerical weather model" *by* J. A. Ruiz-Arias and J. Dudhia

S. Kinne

stefan.kinne@zmaw.de

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Review of paper:

A simple parameterization of the shortwave aerosol optical properties for surface direct and diffuse irradiances assessment in a numerical weather model by Ruiz Arias and Duhia

Positives - Interesting topic

Concerns - secondary detail (rel. hum dep, TSI weighing, vertical distribution) is di-

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verting attention - only two aerosol types are considered (one of them [urban] is unrealistically strong absorbing) - assumed SSA and ASY values in tables A3 to A6 seem incorrect (based on info given) - tests involving significant aerosol loads (those matter) are missing - little relevance for global applications

General comments

The paper evaluates a simple method to address aerosol impacts on downward solar broadband radiative fluxes at the surface and efforts to separate contributions attributed direct and diffuse radiation. The idea here is to use information on mid-visible aerosol optical depth which - if available and accurate – (along with data on atmospheric water vapor and ozone) should define the direct solar broadband flux component at clear-sky conditions (desired in solar energy applications). There is a reliance on that these AOD data are available and accurate. This may be difficult as satellite AOD are not always available and often inaccurate or as climatological values may not apply for any particular investigated condition. Most efforts are spent on defining solar sub-spectral properties of the RRTM radiative transfer model, as not only the mid-visible region but also other solar spectral regions contribute to solar broadband fluxes. The authors hope to cover aerosol impact (assuming AOD550 plus water and ozone column data are available) with one single (aged accumulation-mode) bi-modal size-distribution and with two different aerosol absorption choices. This is way too simple. There is no consideration for coarse aerosol (e.g. dust) and the assumed more absorbing composition is unrealistically strong. Also the validation is not convincing. The validation from direct comparisons focuses on very low AOD cases, so that potentially larger deviations (which should be expected at important larger AOD values) are avoided. Aerosol is highly variable in size and composition. The compositional differences are much larger than the addressed variability in relative humidity (different aerosol types respond also differently on rel.hum). So the extracted added diffuse solar flux attributed to aerosol, which was the purpose of this paper, is highly questionable. I see very little benefit to the community from the proposed approach.

minor comments

tables and figures (I started here, because they need to be self-explanatory)

Table 1 apparently ïĄň values refer to the central wavelength of spectral solar sub-bands with respect to the wavelength (and solar energetically bands 13 and 14 are irrelevant)

Table 2 Relative humidity can swell aerosol size and reduce the Angstrom parameters. This is apparently considered here, although I would assume that rural aerosol sizes are often larger than sizes related to urban pollution (opposite as to what is shown here). Another concern is that the Angstrom parameter is mainly modulated by the dominant aerosol type, being lower than 0.5 for dust and larger than 1.5 for heavy pollution and wildfires. So the covered Angstrom range of the table is limiting to a specific aerosol type. It is also not clear from the table what the difference between ïĄą1 and ïĄą2 is? Apparently, it accounts for the spectral dependence of the Angstrom parameters given with different value for wavelengths below 550nm (ïĄą1) and above 550nm (ïĄą2). Again, this is detail for a specific aerosol type (and size). Given the actual diversity in aerosol type and size distribution, these assumptions are very limiting in applications.

Table A1 – A6 In the table apparently extinction ratios (with respect to the reference wavelength at 550nm), SSA and ASY values for all 14 RRTM solar spectral bands for the two considered aerosol types are offered. These two aerosol types hardly cover the spread of aerosol properties (e.g. check with optical and microphysical AERONET statistics). I am puzzled about the assumed mid-visible absorption of ‘urban’ aerosol. The SSA is extremely low (SSA at 550nm: 0.64 at 50%rh and 0.78 at 80%rh – check with AERONET).

Figure 1 Scaling factors are presented for the rural aerosol type for all 14 solar spectral sub-bands as function of the relative humidity. I wonder, why only rural and not urban aerosol factor are presented? I do not understand the difference between ‘weighted’

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and ‘unweighted’ results, which are quite different. Generally I wonder about the scaling factors, which range from values below 1 and well above 1. (I guess these are just the extinction factor with respect to the 550nm reference wavelengths as the values for band 10 (533nm) are close to 1). I assume that the factor change at higher relative humidity is due to size-increase (water uptake). How are changes in atmospheric water vapor accounted for (which affect the extinction in near-IR bands? Or is this ignored?). Given that water vapor in the atmospheric column should have a strong impact it seem more logical to me to include aside from AOD,550nm also atmospheric water as other variable (with relative humidity impacts on AOD only as secondary effect . . . also since some aerosol types resist water uptake, like dust or fresh BC).

Figure 2 It is difficult to see the details (possibly ignore band 14 for a better spread). Again the urban aerosol absorption potential (1-SSA) is very large (unrealistic for typical conditions).

Figure 3 Results are investigated apparently for six sites. A 5% error seems unacceptably large (e.g. 40W/m2 at 800W/m2 solar insolation). I think that the broadband solar flux observational (measurement) error is only at 5 to 10W/m2. From the comparisons I conclude that the generally good agreement for solar downward fluxes at all-sky conditions is based on the prescribed AOD and larger deviations for diffuse radiation indicate problems with the assumed aerosol absorption.

Figure 4 Only AOD,550nm up to values of 0.2 are investigated (there are often many times larger AOD values)? What are the very low SSAvalues in the frequency distribution? Are they related to very low AOD values . . . in which case they are meaningless. Make a frequency distribution for AAOD (=AOD*[1-SSA]) rather than SSA. Again the reference data seem WRF simulations. Measurements (e.g. AERONET) would be much more useful, especially if there are many modeling issues to potentially contaminate (unless WRF skill has been demonstrated against AERONET statistics). If the total flux error is at 5% then I expect the diffuse percentage error to be much larger than the direct percentage error at cloud free/low aerosol conditions.

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Figure 5 This figure was included to demonstrate progress over the Duhia model . . . but its underlying assumptions (strength and limitations) of that model need to be addressed in order to judge on progress.

Text

Chapter 1 (Introduction) Just to get the terms correct. . . GHI = DNI +DIF . . .if there is no terrain issue which you neglect. Correct? I wonder about the usefulness of so called 'simple methods' (Dudhia 1989) – no link found? What do you mean with "run with near instantaneous observations" . . . what observations, how included?

Chapter 2 With respect to aerosol induced changes in GHI by the DNI and DIF changes are od opposite sign but do not offset each other completely as with more aerosol . . . less GHI. I would say that AERONET offers all relevant AOP data (AOD, size-distr and ref.indices at 4 solar wavelengths so that AOD, SSA and ASY could be (MIE) easy determined) . . . apparently these data are used in the evaluation of the described approach

Chapter 3 I am wondering about the (two) aerosol models that are used. These models are very old, while in contrast there is now much more info on aerosol properties from AERONET sun-photometer statistics (collected over more than 10 years at many sites globally) available. The aerosol models (rural and urban) have an effective radius of about 0.067um. Thus, all aerosol particles are concentrated in a relatively small size accumulation mode. In that context also the Angstrom parameters given in Table 2 are inconsistent with that size-distribution. The adoption of 20% soot for the 'urban' aerosol is unrealistic, as such high BC content (on top of already absorbing aerosol) is rather rare. The result is very low SSA values. And such low SSA values are not (AERONET) observed, unless for extremely rare events, if at all. (Why relying on such an old reference data, as we have learned so much more from AERONET statistics?)

Chapter 3.1 With only one realistic choice for absorption and only one choice for the aerosol size-distribution (Shettle and Fenn), all the additional efforts on accounting for (e.g. Angstrom) spectral dependence, on considering impacts of a changed relative humidity and on the weighing via the solar constant (TSI) within solar spectral sub-bands (the largest rel. errors probably occur in extreme [uv, ir] bands, where solar insolation is relatively small so that the overall the impact should be close to negligible) seem almost irrelevant. To correct: Shettle and Fenn provide information on the (bimodal) size-distribution and on spectral refractive indices from which SSA and ASY can be derived (with MIE simulations) - they do not provide SSA and ASY value. When applying the rural bimodal size-distribution (999.* 0.03/1.42 and 0.125*0.5/1.5) and refractive indices (at 550nm 0%rh: 1.59 / -.66e-2) as given in Shettle and Fenn, I get a SSA,550nm of only 0.87 and an ASY,550nm of 0.52. Both values are lower than those given in Tables A3 and A5. This makes me wonder on what undisclosed assumptions the values in Tables A1 to A2 are based on.

Chapter 3.2 Again, weighing the TSI weighing [SSA (with AOD and TSI) and ASY (with SSA, AOD and TSI)] is only of secondary importance. Getting the appropriate size-distributions is much more important.

Chapter 3.3 Also the vertical distribution is only a secondary importance. It would be though interesting if the approach compares well to CALIPSO space-lidar statistics on aerosol vertical distributions.

Chapter 4.1 What AERONET data are actually used (only AOD550 and water column data?). If only these data were used was there a sanity check? Were predicted sub-spectral AOD data of the WRF model compared to the sub-spectral AOD (440,500, 670, 870, 1020nm) AERONET data? Has WRF modified the originally supplied AOD550nm to be spatially consistent with other site data and if so, by how much? Or are simply off-line radiative transfer simulations applied? Please clarify.

Chapter 4.2 The test case combines poor spectral definitions for AOD, SSA and ASY at non 550nm wavelengths due an inappropriate assumption for the size-distribution (failure to be AERONET consistent) For the Table Mountain site (TBL) it is argued that

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the strong absorption aerosol type is relevant. But is not Table Mountain a very remote site, where urban pollution should not be an issue, unless there was a wildfire in which case I would expect elevated AOD? For the case of low SSA match it also would be of interest what the associated AOD is. In case of a very low AOD (very noisy)... who then cares?

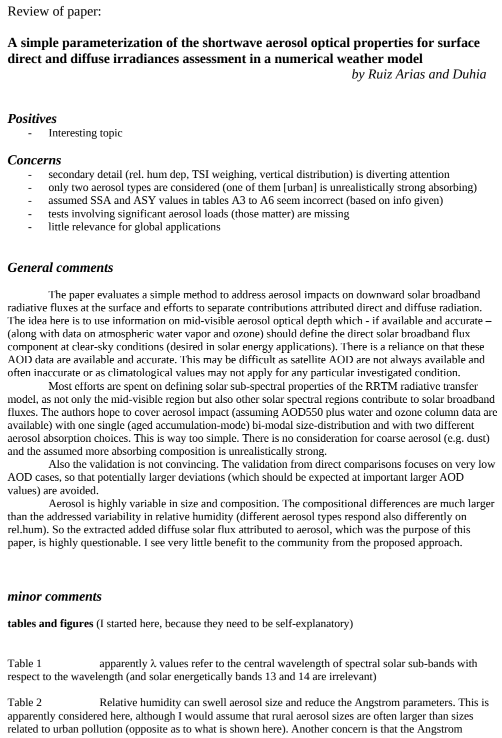
Chapter 5 Please briefly describe the Duhia SW scheme (how many spectral bands?) and assumed simplifications.

Chapter 5.1 The validated AOD range only addresses AOD values up to 0.2 (the average value of 0.06 corresponds to clean oceanic background!). Thus a real test for strong aerosol loading is avoided . . . so at minimum the 'validation' is incomplete.

Chapter 5.2 I do not understand this paragraph. If seasonal varying AOD data are used as input certainly this is better than using an annual average value. It is unclear tough why the Duhia model underestimates solar fluxes in winter, as the largest AOD values over the continental US are usually observed during spring and summer.

Chapter 6 I wonder about the (dis-) agreement of sub-spectral AOD and SSA of AERONET to those of your spectral assumptions for the test-cases. I agree that that the AOD550nm value, if available and accurate is a good first order estimate to constrain impacts on solar fluxes at the surface (and you should have stopped here). I am not sure if the method can be unilaterally applied globally, since the rural mode only addresses the fine (or accumulation) mode aerosol size and many (potentially available) AOD maps from satellite data are often inaccurate and biased (due to a-priori assumptions for aerosol absorption and size and for surface reflectance). I completely agree with the last sentence ... what do YOU do with this approach in cases of dust?

Interactive comment on Geosci. Model Dev. Discuss., 7, 593, 2014.



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