

Interactive comment on “Spatial distribution of dust’s optical properties over the Sahara and Asia inferred from Moderate Resolution Imaging Spectroradiometer” by M. Yoshida et al.

Anonymous Referee #1

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Comment on “Spatial distribution of dust’s optical properties over the Sahara and Asia inferred from Moderate Resolution Imaging Spectroradiometer” submitted for possible publication to ACP by M. Yoshida et al.

This paper uses satellite observations in the visible wavelength to infer optical properties of the aerosol. The estimate is based on the spatial variability of the measurement and its linear relationship with the same in clear conditions. The retrieved optical parameters are compared to surface remote sensing measurements, and the spatial distributions are interpreted.

There are several very good points in this paper. It is the first time, to the reviewer

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knowledge, that the method originally proposed by Y Kaufman has been used on such a large scale. The authors must be praised for putting so much efforts in the data analysis. The paper is also rather complete as it includes the practical implementation of the method, the data processing and analysis, the validation of the results, and an interpretation of the spatial and temporal distributions of the products. The paper does offer original data and products which could be of interest for the community. On the other hand, the paper also shows several weak points that prevent its publication in the present state. (1) First of all, the wording is poor which makes the reading difficult. I strongly suggest an extensive re-writing of the paper. As two of the authors are native English speakers, this sounds like an acceptable request (?). (2) The method description is very unclear (see detailed comments). It never states what are the assumptions, and I had to read the paper several time to finally understand what is done for the data analysis. For instance, it is never stated explicitly that the method assumes that the aerosol optical properties (optical depth, single scattering albedo) are constant at the $1 \times 1^\circ$ scale. (3) The data interpretation is unconvincing. The authors find a strong correlation the single scattering albedo and both the surface reflectance and the optical depth. They choose for an interpretation which is a physical link and do not even mention a possible spurious correlation which appears more probable to this reviewer.

Overall, I think this paper should be published after significant revision to (i) make it clearer and (ii) be more prudent in the data interpretation.

Detailed comments Abstract could be more quantitative, providing values of the retrieved single scattering albedo, and accuracy of the retrieval P31108_L14: “are suitably minor”. This statement does not agree with the paper result that show rather weak correlations and significant difference with the ground truth. P31108_L18: “w0 is determined mainly by [...] and/or the optical depth of airborne dust in Sahara”. This is certainly and impossible statement. W0 is a microphysical properties of the aerosol whereas the optical depth is a property of the layer. P31113_L7-12: Sentence to be re-written Section 2.1. Method description. It should be made clear that the big as-

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sumption of the method is that the aerosol optical properties do not change at the $1 \times 1^\circ$ resolution. In addition, there are regions that are very homogeneous spatially. For these regions, the proposed method cannot work or may provide very unreliable results. One expects the aerosol signal to be much more homogeneous than the surface structure. Is that always the case ? P31114_L22: I am surprised that the authors use a non spherical aerosol model together with the microphysical parameters from an AERONET inversion that assumes a spherical aerosol model. I see a strong inconsistency that may have a significant impact on the aerosol phase function. P31116_L1. The method assumes that the observation is “clear” when the OMI index is less than 2, and aerosol-loaded when it is larger than 3. How have these thresholds been chosen and to what optical depth do they correspond ? P31116_L11. Even if the view zenith angle is the same, the sun angle is likely different so that the observation geometry varies P31116_L15. It is absolutely not clear what means “statistically significant” here. Does it mean that the sign of the retrieved slope is known, or that the slope is known within x % accuracy, or what ??? Equation (2) does not help. It is said that SE is the square sum of errors but what are the errors ? P31116_L21. “We assume the level of significance of rejection to be 5%”. I guess this is not an assumption but rather a criteria for rejection. So the datapoint is rejected when F is lower than 0.05, or lower than 0.95 ??? Please explain. P31117_L14. Section 2.5 I fully disagree with this section. The equations are valid only in the case of small optical thicknesses ≈ 0.2 and smaller. The authors use cases with large optical depths, often larger than 2. For such cases, the equations are not valid. They do not bring anything to the paper and I therefore suggest to remove section 2.5 entirely. P31121_L5: “These variations correspond to the spread of the scatter plot. . .”. This is an assumption. There may be other causes for the scatter, including measurement noise, undetected clouds, or directional effects. P31122_L13: You show temporal composite but do not explain how these have been achieved. Is it a simple average of aod and w0, or a weighted average (I would do an average of w0 weighted by the aod if I were you, as the uncertainty is probably smaller for large aod) P31122_L17: Not clear why the uncertainty in the aerosol optical

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properties are larger in the southern region. In this region, there is a seasonal cycle of the surface reflectance. This may be a factor. P31122_L21: “the total uncertainties are concluded to be suitably minor”. One can only say that I requirements have been defined and uncertainties quantified, and the latter be smaller than the former. Please be quantitative to claim such statement. P31122_L8: “...most likely because the former is in the form of point observation, while the...” The method developed in this paper relies on the hypothesis that aerosol optical properties are constant over $1 \times 1^\circ$. It is bizarre to claim spatial heterogeneity as the reason for poor agreement with validation data. P31122_L21: “...is more consistent”. Than what ? How does one knows that ? P31123_L23-27: I fully disagree with this statement. A tendency to reject small aod may result in a high bias on temporal averages, but your validation is made on individual retrievals. Then, even if your method selects only high aod cases, it has no reason to result in a high bias in the validation procedure. P31125_L14. Section 3.3 This concerns the interpretation of the spatial structure of the results. The validation has shown that there is a rather wide spread between the retrievals and the ground truth. Beside, there are few validation sites. Then, if the method shows a bias that depends on the ground reflectance, it may not be seen by the validation procedure and generate spurious structures. P31126_L11. “because the spread in Asia is larger”. Are you saying that the correlation is lower because the non-correlation is larger ? P31126_L15-19: These lines discuss previous estimates of the single scattering albedo. This is also done in the introduction, and the two are not fully consistant. Please harmonize (or rather remove one of the two instances) P31127_L3-9: The author hypothesize that there is a true relationship between the optical depth and the single scattering albedo. I feel that a bias in the method is much more likely. The authors have the possibility to check their hypothesis against sunphotometer products. Do they find any w0-AOD in the Aeronet data ? If so, this is a most interesting result. If not, the authors should conclude that their method generates such spurious correlation. P31127_L10: Section 3.4 In this section, the author discuss the spatial distribution of the dust optical properties. In particular, they interpret the correlation between the surface reflectance and

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the dust absorption. They fail to mention that many regions are NOT dust sources, and that the dust may be transported over considerable distances. One should therefore be much more careful than they are in the result interpretation P31129_L17 Summary: In the summary, there are several strong statements that are presented in the paper as possible hypothesis. The validation procedure shows a large bias on the AOD, and a significant spread on w_0 so that, in the reviewer opinion, the spatial distribution should be interpreted with more caution.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 31107, 2012.

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