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Comment

Interactive comment on “Retrieval of aerosol optical depth over land surfaces from AVHRR data” by L. Mei et al.

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Atmospheric Measurement Techniques

Title: Retrieval of aerosol optical depth over land surfaces from AVHRR data

Authors: L. Mei, Y. Xue, A. A. Kokhanovsky, W. von Hoyningen-Huene, G. de Leeuw, and J. P. Burrows

General Comments:

This paper describes an initial effort to construct an aerosol retrieval algorithm using Advanced Very High Resolution Radiometer (AVHRR) data over land. The approach is essentially the MODIS “dark target” algorithm using an estimated surface reflectance at

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0.64 μm based on regressions from the 3.75 μm (AVHRR) band, the 2.1 μm (MODIS) band, and the 0.64 μm (AVHRR) band. After including a Rayleigh correction, a retrieval is performed using a look up table (LUT) adopted from the Bremen AErosol Retrieval (BAER) algorithm developed for MERIS. This approach is tested for a limited set of cases over northeastern China, including the Beijing, Xinlong, and Xianghe Aerosol Robotic Network (AERONET) sites.

In my opinion, publication of this paper is premature. Important parts of the algorithm are poorly explained, and there is extremely little validation presented here. As the authors themselves state, “due to the limited number of reference points available for this study area, we cannot properly evaluate the retrieval algorithm over this region.” The primary innovation of using the AVHRR 3.75 μm band with empirical relationships derived from MODIS itself has issues, only some of which are addressed by the authors. Without including more extensive validation and testing, I cannot recommend this paper for publication in Atmospheric Measurement Techniques.

Specific Comments

Below I have provided specific overall comments that may be helpful to the authors. Page and line numbers have been included where appropriate.

Page 2228, Line 3: The “consistency” of the AVHRR data record is somewhat limited by calibration drift, which the authors note later in the paper, as well as intercalibration issues.

Page 2228, Line 18: The correlation coefficient does not in itself provide enough evidence for adequate performance. Note that the R^2 value is 0.61, which suggests that a linear relationship between the two metrics explains 61% of the variability of the test variable. The other 39% is unexplained by this relationship.

Page 2228, Lines 23–24: Again, the correlation coefficient is insufficient. The authors do not describe the relative magnitude of the RMSE. Is 0.17 acceptable or not for an

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aerosol retrieval over land?

Page 2229, Line 5: I do not understand the meaning of the sentence beginning: “The results further depend. . .” Does this refer to the number of results, the quality of the results, or something else?

Page 2229, Line 25: The “popularity” of the MODIS “dark target” approach is mentioned before any reason is given for the need to determine the empirical relationships between radiances observed at different wavelengths. The key idea of the dark target approach is not described in this section. Put simply, it is that you can infer the “clear” reflectance of the surface at a short wavelength, where aerosols are significant, by using the top of atmosphere (TOA) reflectance of the same scene at a much longer wavelength, where the impact of aerosols is assumed to be insignificant. Essentially subtracting the inferred surface reflectance at the short wavelength from the TOA reflectance yields the path radiance, assumed to be due to aerosol alone.

Page 2230, Line 10: The “band-setting limitation,” introduced without description or reference here is, I believe, the central obstacle the authors are trying to overcome with their approach. This needs to be stated more clearly earlier in the paper.

Page 2230, Line 22: This entire paragraph is devoted to specific attempts by previous investigators to derive aerosol optical depth (AOD) over land from AVHRR data. The final sentence of the paper, however, states, “the developed algorithm is the first promising steps towards the retrieval of AOD from AVHRR over land.” This assertion is directly contradicted by this paragraph.

Page 2330, Line 26: The word “data” appears twice in this sentence.

Page 2230, Line 28: About their own retrieval authors write, “The approach assumes that surface reflectance of 0.64 μm can be obtained using an empirical relationship between the reflectances at that wavelength and at 3.75 μm , using the MODIS dark surface approach.” Just nine lines before, they criticize previous work on the basis

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that “the surface emissivity is variable, due to changing surface temperatures, which may cause an uncertainty in the relationship between the reflectance at 3.75 and 0.64 μm .” It is never explained how this problem is overcome through the application of the MODIS dark target approach, or through the use of the intermediary 2.1 μm channel.

A potentially critical issue that appears to be overlooked is the different spectral responses of the AVHRR and MODIS bands. Starting from the shortest wavelength, the nominal width of the AVHRR 0.64 μm band is 0.58 – 0.68 μm , while the nominal width of the MODIS 0.66 μm band is 0.62 – 0.67 μm . This means that the AVHRR band is twice as broad as the MODIS band (0.10 μm vs. 0.05 μm). The 3.75 μm AVHRR band has a nominal width from 3.55 – 3.93 μm , while the MODIS band is nominally 3.66 – 3.84 μm (0.38 μm vs. 0.18 μm). Empirical relationships derived using the MODIS observations, and implicitly the MODIS bandwidths, may not be appropriate when applied to AVHRR data.

Page 2231, Line 22: It was never clearly explained how the Raman-Pinty-Verstraete (RPV) bidirectional reflectance distribution model was employed in the algorithm. According to Fig. 3, only the visible reflectance is used, but perhaps I missed something. The RPV model should be used to estimate the surface albedo in Eq. (2) from the observed radiance, but it requires some additional assumptions.

Page 2232, Line 19: The sentence beginning “Because we assume that TOA reflectance is equal to surface reflectance...” is key to the approach taken here, but I fail to grasp why the surface reflectance at 0.64 μm is not inferred directly from the 3.75 μm data. The sentence states, “to avoid using [an] additional reflectance product,” but why is this a problem? The majority of the next portion of the paper is devoted to this issue, so the authors should explain why this is critical to their approach.

Page 2233, Line 5: The linear relationship is not given, it is assumed.

Page 2233, Line 20: It is not clear to me what a “good linear relationship” is. It should be assessed quantitatively. What’s the maximum error introduced by assuming a linear

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relationship, for example?

Page 2233, Equation (12): I am left to assume a linear least squares fit was applied to the data to “obtain” this result. What is the quality of this fit?

Page 2234, Line 8: The line should read, “...fast economic development. . .”

Page 2234, Line 21: The fact that the NOAA platforms observe the same Earth location twice a day is not relevant because one of these views is at night when aerosol retrievals using visible spectral bands cannot be performed.

Page 2235, Line 7: What does “quite similar” mean? I also do not understand why the authors compare their results to the MOD09 (8-day) reflectances and not to the estimated 0.66 μm MODIS reflectances determined using their own approach.

Page 2235, Line 9: As described above, the correlation coefficient does not tell the whole story. I see a great deal of spread in the data plotted in Fig. 5.

Page 2235, Line 13: What does “in good agreement” mean? The authors should be much more quantitative.

Page 2235, Line 15: Looking carefully at Figs. 4 and 6, I notice what looks like an anticorrelation between the AVHRR derived surface reflectance and the retrieved AOD. It would be interesting and informative to make a regression plot of these two quantities.

Page 2235, Line 18: I do not feel that Fig. 7 contributes significantly to the discussion. The authors already state in the text the content of the figure.

Page 2236, Line 1: It seems to me that a calibration error would affect the overall image (and overall retrieval), not a particular region. One could imagine complicated non-linear calibration effects that depend on the observed reflectance, but to first order the calibration should be linear.

Page 2236, Line 9: Given that the BAER LUT is known to perform so poorly in this region, why was it used at all? Alternatively, why was the analysis performed in this

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region instead of a location where the BAER LUT performs well?

Page 2236, Line 13: This is the first (and last) mention of cloud screening in the entire paper. Since it is apparently important, it should be discussed.

Page 2236, Line 24: I do not understand the meaning of the phrase "...we reduce the number of LUT in term of the polynomial by consideration of the single scattering approximation..." The use of the single scattering approximation in the retrieval algorithm is important and should have been discussed earlier.

Page 2238, Line 3: The statement "... for such a small area the surface properties are expected to be relatively constant" should be supported with a reference.

Page 2243, Figure 1: Does it make sense to have negative values on the y-axis? This plot should also include an error envelope and a mention of the number of points used in the regression.

Page 2246, Figure 4: The caption mentions that both the TOA reflectances and the surface reflectances are shown on the right. The TOA reflectances are on the left. Why are the AVHRR surface reflectances in the upper right panel "blurry" compared to the MODIS reflectances?

Page 2247, Figure 5: What is the number of points appearing in this figure. A one-to-one line and the regression line should also be included, at a minimum.

Page 2248, Figure 6: It is possible to use the MODIS data to determine the AOD at $0.66 \mu\text{m}$ using the Angstrom coefficient for the MODIS aerosol model, which should be a more accurate comparison than assuming the Angstrom coefficient is 1.

Page 2249, Figure 7: This figure conveys very little information, and I would suggest eliminating it.

Page 2250, Figure 8: A one-to-one line should be included at least, since it is very hard to see the underestimation described in the text. It might also be interesting to color

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code the points by AERONET site.

Page 2251, Figure 9: See previous comment on the Angstrom coefficient. Also, an overall regression analysis of the AVHRR data to the MODIS data for this case would be appropriate.

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