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Atmospheric Measurement Techniques Discussions

Interactive comment on "The Collection 6 MODIS aerosol products over land and ocean" *by* R. C. Levy et al.

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General Comments:

This paper presents an overview of the MODIS Collection 6 (C6) Dark Target (DT) aerosol products. In particular, it describes a number of the changes that users can expect to see relative to the widely used MODIS Collection 5 (C5) aerosol products. The authors provide a readable introduction to the product content and the theory behind the retrieval approaches. In addition, some comparisons are made that show how the C6 and C5 products compare to one another, both globally and with regard to commonly used validation datasets. New merged datasets, a higher resolution (3 km) product, and the methodology used to construct the Level 3 (gridded) products are also discussed in some detail.

In a lot of ways the paper reads like a "conversational" Algorithm Theoretical Basis Document (ATBD), which is a good thing, in my opinion. ATDB's tend to be written and read only by the algorithm teams themselves, but they often contain information that is important for the larger scientific community. For this reason alone, I feel this is a significant paper, appropriate for publication in Atmospheric Measurement Techniques.

My overall opinion of the paper is less harsh than that of the other referee (Jeff Reid), who had some frank criticisms of the paper in terms of the extent of the data analysis. However, as a member of an algorithm development team for a different satellite instrument, I am perhaps more sympathetic to the situation of the authors. Although the paper does not present any extensive validation of the new algorithm, it does present a number of examples of the changes observed by the algorithm team as modifications were made in the retrieval approach, and it discusses the background and motivation for these modifications. This provides valuable insight into the strengths and weakness of the MODIS DT aerosol retrieval algorithms from the perspective of the developers themselves. That said, I do agree, however, that critical analysis of the performance of the MODIS C6 DT aerosol retrieval appears to be lacking in a number of areas. In my view, the paper would be greatly strengthened by including a more careful consideration of some of the results already shown, as well as expansion of the analysis along the lines suggested by Jeff Reid.

Specific Comments

This is a rather long paper, so I will provide my comments broken down by section.

Introduction

With regard to the "spatial resolution and repeatability," I was surprised not to see a reference to at least Levy et al. (2009), if not Shi et al. (2011), and the work of others who have looked at the spatial statistics of MODIS aerosol retrievals in great detail.

2.2 Basic concepts of the MODIS aerosol retrieval algorithm

Perhaps I'm missing something, but it's not clear to me how, only knowing the total AOD and fine mode fraction, the AOD can be derived at other wavelengths. One would need to know something about the optical properties of both the fine and coarse mode aerosols, at least. In that case, it would be possible to derive the AOD at other wavelengths and infer the Ångström Exponent (AE). This is an important point because certain research groups like to construct a MODIS-derived Aerosol Index (AI) by multiplying the AOD by the AE (e.g., Lebsock et al., 2008). Unlike the equivalently named AI from TOMS/OMI, the intention here is to derive a parameter that is related to the aerosol number concentration, which in turn provides an estimate of the cloud condensation nuclei (CCN), in the manner of Nakajima et al. (2001). However, what Nakajima et al. (2001) did was retrieve the AOD in two AVHRR wavelengths separately, allowing in some sense the "direct" derivation of the AE, independent of an aerosol model. The terminology is also somewhat confusing when the authors compare the DT land and water approaches. The authors state that an aerosol "model" is retrieved over land, while an aerosol "mode" is retrieved over ocean (p. 169). It's not clear what distinction is being made. In both cases, a look up table (LUT) is employed and a specific aerosol "model" is retrieved. Even over water the MODIS retrieval provides information on the AOD at 0.55 μm and the model (fine mode and coarse mode) that yields the minimum error relative to the observations – AE is not an independently derived parameter because using an aerosol model in a LUT prescribes the spectral slope of the AOD. If the intention is to highlight that the LUT itself is prescribed both regionally and seasonally over land, while a single LUT is used over the global oceans, then this point needs to be made more clearly.

With regard to the retrieval of fine mode fraction (FMF) over water, I was surprised there is no discussion of (or even a reference to) the results reported by Anderson et al. (2005), who found that MODIS AE provides better agreement with suborbital measurements than MODIS FMF, at least in the ACE-Asia region for the Collection 004 algorithm. I do not believe these results were addressed in the study by Kleidman et al. (2005), which is referenced here. Again, this issue is important be-

cause members of the research community wish to use the MODIS aerosol products as a means for assessing the effect of aerosols on clouds (e.g., Lebsock et al., 2008), and the anthropogenic aerosol fraction (e.g., Yu et al., 2009). While the MODIS algorithm team cannot possibly be expected to anticipate all the possible uses of their product by the community, I believe that it is important for the algorithm team to help the community use their products properly, and a paper like this serves as an appropriate forum for such discussions. In light of this, I applaud the decision to change the SDS "Cloud_Condensation_Nuclei" to "PSML003_Ocean," as a step in the right direction (although from Table 3 it is difficult to determine whether the SDS "Cloud_Condensation_Nuclei_Ocean" remains in the product or not). The removal the intensive properties from the Level 3 products will also provide a useful safeguard on some potential "inappropriate" uses of the MODIS data products in the future.

2.3 Evaluation of the C5 MODIS aerosol products

The expected error (EE) for the FMF is given as $\pm$0.20 over ocean with a reference to Kleidman et al. (2005). Looking at that paper, it is unclear where this value comes from as comparisons are made against both the AERONET "Dubovik" and "O'Neill" inversions, with differing results. It is also unclear to me what the implication of a $\pm$0.20 FMF fraction sensitivity is. One interpretation is that the MODIS retrieval provides effectively three ranges of FMF over ocean (0.0–0.4, 0.3–0.7, and 0.6–1.0, for example, minimizing the overlap). It would be helpful to have a discussion of this issue from the algorithm team.

I realize that I may be expressing a minority view, but I still take issue with the retrieval and reporting of negative AODs over land in the MODIS product. I am aware that other consumers of the product view the inclusion of negative AODs as a significant advance that provides more "statistically robust" results. I have two issues with this approach. First, this violates the physics of the retrieval itself, implying that less radiance is viewed by the instrument than for a perfectly clean (i.e., aerosol free, or Rayleigh-only) atmosphere. Algorithmically, it is surprising that this can ever occur given that some fraction of the pixels (20% at the low end over land) are already discarded as "unsuitable" for retrievals. The exclusion should handle the types of situations (e.g., cloud shadows) that could lead to extremely small radiances. Second, careful examination of the statistics shows a significant increase in the sampling exactly at the -0.05 AOD cutoff employed. This can be seen in Figure 5, but is somewhat clearer in Figure 5b in Kahn et al. (2009), with finer bin resolution. This is not consistent with the idea that there's a tail that is tapering off quickly at low (negative) AOD, as would be the case for a Gaussian distribution centered somewhere around 0. It might be useful to plot the MODIS DT-land AOD data from -0.05 to +0.05 in equal increments of 0.01 to better see this behavior. The unequal bin widths used in Figure 5 in this paper make interpretation difficult.

3.1 Changes common to both land and ocean algorithms

The title of this section might be modified to include the term "radiative transfer," since that is the primary focus of the changes discussed. The detail provided in this section is exemplary and this is precisely what makes this paper so valuable to the community.

The authors note that the angular/stream resolution of both radiative transfer codes was increased, but it would be helpful to note what the final choice was for comparison and future reference.

Given the likelihood of slight differences in the fabrication of the detectors, I was surprised to see a single set of MODIS central wavelengths (CW) provided in Table A1, rather than a set of CW for Terra and a separate set for Aqua. It is encouraging to see the improved agreement between the numbers shown in Table A1 for C6 and those found in Table 2a in Kahn et al. (2007), which were calculated for Terra.

The "King factor" the authors provide (0.0279) is actually the Rayleigh "depolarization factor" (Young, 1980). As discussed by Young (1982), there is already enough confusion in the terminology, so it is better to follow standard practice here. In fact, this depolarization factor is weakly dependent on wavelength, so the value adopted by the

MODIS aerosol team is really applicable only to a wavelength of 0.600 μm. At 0.450 μm it is 0.02883 and at 2.0 μm it is 0.02702. These results are taken from Table 2 in Tomasi et al. (2005).

The authors report that the radiative transfer code changes, taken in sum, "resulted in an overall global mean AOD increase of approximately 0.01 over land, and a decrease by 0.005 over ocean" (p. 175). Are these results for both Terra and Aqua? Do these results reflect an annual mean value, or a comparison of a single month of data? Moreover, these numbers provide one example of what I believe is a lack of critical evaluation of the results. Taking the mean values of C5 AOD from Tables 2 and 4 in this paper, the change due to the radiative transfer alone results in a nearly 6% increase in the AOD over land and a 4% decrease in the AOD over ocean. The EE (for C5) is presented as $\pm$(0.05 + 15%) over land and $\pm$(0.03 + 5%) over ocean (pp. 171-172). How does one relate a change in the global mean AOD to the EE? In other words, is a $\sim$5% change in the global AOD "acceptable" given the reported uncertainty envelope?

3.2.6 Comparison with C5 products and AERONET

Again, when discussing the results in this section, the authors neglect any critical evaluation. Expressing the change in the global mean land AOD (for Aqua) from C5 to C6 as a percentage, the values are +3% for January and +17% for July. As discussed above, this change to the global mean AOD seems large given the quoted EE. While it is not unexpected that the global AOD would increase given the removal of the additional CO_2 correction from C5, apart from some changes to the radiative transfer, the only other apparent significant changes from C5 to C6 are the cloud masking logic (including pixel call back) and a bug fix in the Quality Assurance (QA). So what are the possible reasons for a change in the global AOD over land of this magnitude? I was surprised not to see any regression plots of C6 vs. C5 that might shed some light on this. The histogram in Figure 5 suggests that C6 has a significantly greater fraction of higher AOD pixels compared to C5. How did the MODIS team verify this was not due simply to poorer cloud screening leading to a greater amount of cloud contamination, for example? Comparison with AERONET (e.g., Figure 7) would not show this because the AERONET data themselves are cloud screened. In the difference plots in Figure 6 it is notable that the majority of the land areas (with sampling) have a pinkish hue, indicating an overall increase in AOD that could very well be indicative of a cloud screening issue. However, the commentary on this section focuses instead on places with extremely high AOD, which the authors associate with heavy smoke. The results for Siberia and Canada are consistent with other smoke climatology databases (e.g., http://www-misr.jpl.nasa.gov/getData/accessData/MisrMinxPlumes/), but not every heavy smoke region (e.g., the Amazon) shows a large increase in AOD in July, and the region around Beijing is unlikely to be impacted very often by heavy smoke.

3.3.1 LUT and wind speed dependence

It might be worth noting that MISR also includes a wind speed dependence in the aerosol LUT over water. The reference would have to be Kahn et al. (2007) or the MISR aerosol ATBD.

The description of the model used to provide the winds is incomplete. I believe the model is the National Centers for Environmental Prediction (NCEP) Global Data Assimilation System (GDAS), which is provided at 1° horizontal resolution as a MODIS ancillary product. However, I think that technically this is just an "analysis" result, as opposed to a "reanalysis," but that may be an issue of semantics. Careful checking of the documentation shows that no "2-meter wind speed" parameter exists. I would guess that the MODIS team is using the "U-component of wind at 10 m AGL" and the "V-component of the wind at 10 m AGL" to derive a wind speed at 10 m AGL (not 2 m AGL as specified a number of times in the text). There are parameters provided at 2 m AGL (humidity and temperature), but not wind.

3.3.2 Cloud masking, sediment masking, and pixel selection

Although there are different standards, it might be good to stick with the band desig-

nations utilized by the MODIS calibration team (e.g., Wu et al., 2012). Here bands longward of 1.0 μm are designated shortwave infrared (SWIR), rather than near infrared (NIR), as is done in this section.

3.3.3 Quality assurance

In my experience, the criterion used for determining a "Rayleigh-like" atmosphere is too loose. At first glance, the approach seems reasonable given that the Rayleigh optical depth at 0.856 μm is 0.01623 (Table A1). 10% of this value is just 0.001623. Since for low AOD the radiance is essentially linearly related to the AOD itself, the criterion appears to be putting a lower limit on the MODIS AOD retrieval of approximately 0.002. However, since the Rayleigh optical depth at 0.553 μm is 0.09480, the 10% criterion really implies a lower limit to the MODIS AOD retrieval of about 0.01. Moreover, vicarious calibration by the MODIS team suggests that the calibration is good to within about 2 – 3% (Wu et al., 2012), so it seems that the algorithm may not be taking full advantage of the instrument sensitivity.

3.3.5 Comparison with C5 products and AERONET

The change of -0.025 in the global mean AOD over oceans from C5 to C6 is huge! This is a 16.7% decrease in January and an 18.2% decrease in July. In fact, it is difficult to believe the magnitude of this change given the EE over ocean in C5 is reported as $\pm$(0.03 + 5%). The authors contend that this change is "a result of the new protocol of assigning QAC for low signal cases" (p. 193). Trying to piece together the causes for this change from Figure 11 alone is challenging. The sampling increased by 5.76 $\times$ 106, which is a single tickmark in the upper (linear) portion of the histogram plot. In fact, this is about the magnitude of the difference in sampling for the modal (peak) value in the 0.050 AOD bin. Although the increase in the relative sampling of AOD = 0.000 is enormous (a factor of 40x), the absolute sampling from the AOD = 0.000 bin is only about 2 $\times$ 106. It is clear from the figure that there are increases in every bin for AOD < 0.050 and decreases in every bin for AOD > 0.050, which is not consistent with the explanation that including more AOD = 0.000 (with high QAC) in calculating the mean value is the driver for this large change in the global mean AOD over ocean. It would be easier to parse out the underlying causes if a second histogram was generated using just matched C5/C6 data, which would make it clearer how the values in the bins changed in C6 relative to the C5. A regression plot of the ocean retrievals from the two collections would also likely be extremely informative.

From Figure 12 it is clear that the inclusion of the wind speed has had a significant (likely positive) impact on the MODIS AOD retrievals, particularly noticeable in the storm track regions, and the monsoon region of the Indian Ocean in July. However, I also note an increase in the AOD in the warm pool region, in particular, associated with a decrease in the AE, which could indicate increased cloud contamination.

Finally, in this section, the authors claim that the 2% reduction in the AERONET collocation in the AE comparison "is due to not reporting size parameters when the AOD signal is too small to retrieve size robustly" (p. 195). Except in this specific section, I did not find any mention of a change in the threshold from C5 to C6 where size parameters are reported, which I assume is based on the discussion in Section 3.3.3. If a change was indeed made, then it would be appropriate to include it in Section 3.3.3, rather than having it appear in this section.

3.5 New cloud-diagnostic products

Relative to the discussion at the end of this short section, it would be useful to include some references to the work that has been done on cloud adjacency effects, etc.

4 New MODIS 3 km product (MxD04_3K)

The authors state that, "the new MxD04_3KM file will provide only a subset of the SDSs offered by the standard MxD04_L2 file" (p. 203). It would be useful to summarize what SDSs will be made available.

8 Conclusions

Once the paper concludes, the big unanswered question is when the MODIS algorithm team expects C6 to be publically released, for both Terra and Aqua.

Appendix A

It would be helpful to have a reference to the solar spectral database used to perform the weighting of the response functions. Clearly, small differences in this database can lead to differences in the spectral response functions.

From the discussion here it appears that a single Rayleigh optical depth is calculated and applied globally based on a calculation performed for 45°N latitude. Is this correct, or is Rayleigh included in the radiative transfer?

The discussion of the derivation of the ozone correction in this section is difficult to follow. While the formula for the calculation of the water vapor transmission is provided (Equation A5), no similar formula is provided for ozone. Also, it would be somewhat more useful to provide the ozone correction factors, which are multiplied by the column ozone abundance to yield ozone optical depths (see Kahn et al., 2007, Table 2a).

References

The following references do not appear anywhere (that I could find) in the paper: Anderson et al. (2003), Kinne et al. (2003), Kishcha et al. (2007), Schafer et al. (2008), and Takemura et al. (2002). A citation for Chu et al. (1998) from page 167 is missing.

Tables

Table A2: The correction factors for carbon dioxide appear to be missing.

Table A4 (p. 239): I believe the parameter should be "precipitable water" not "perceptible water," this could be a spell checking error.

Table A5 (p. 241): Again, "precipitable water" not "perceptible water."

Figures

Figure 5: There's a typo in the figure caption. It should be "0.55 μm." As discussed above, it would be helpful to have both a histogram of matched data and the histogram plotted with equal sized bins. It is interesting to note that this histogram has the opposite behavior seen in Figure 11, the histogram for the ocean retrievals. In the land histogram the sampling in all the bins less than the modal (peak) value decreases, while the sampling in all the bins greater than the modal value increases. The biggest driver in the reduction of sampling appears to be in the first (-0.050) bin.

Figure 7: There's a minor inconsistency in the figure caption and the axis labels. The caption gives the wavelength in μm, while the axis labels use nm. It would be useful to have additional plots showing the C5/C6 values regressed against one another, and the AERONET regression for the matched dataset (i.e., times and places where both C5 and C6 retrievals are available).

Figure 8: The NCEP wind speed is provided at 1° resolution, so contouring the data in the way it is done in this figure makes it difficult to see how the values are changing, and the contour labels are hard to read. It would make more sense to provide an additional panel showing the NCEP winds by themselves.

Figure 9: If it were true that "the largest differences are near the glint mask," why is the glint exclusion region not "outlined" in the difference plot? Instead, differences appear near the latitudinal extremes of only a few of the glint exclusion regions. It appears that there may be a relationship with the AOD as well, so a map of the percent change in AOD might also be informative.

Figure 10: As the point of this figure is to demonstrate improved screening of high cloud, it would be more compelling if there were a panel showing the presence of high cloud (e.g., from the MYD35 product).

Figure 11: There's a typo in the figure caption. It should be "0.55 μm." See comments related to Figure 5 and the earlier discussion of Section 3.3.5.

Figure 13: There's a minor inconsistency in the figure caption and the axis labels. The caption gives the wavelength in μm, while the axis labels use nm. See comments related to Figure 7.

Figure 14: This figure would be more compelling if it included at least an RGB image for visual comparison.

Figure 17: There's no color scale for the figure (it looks like it is cut off). The color scale itself is poorly chosen, as it is difficult to see the changes against the black background. Perhaps a white or gray background, consistent with some of the previous figures, would be appropriate.

Technical Corrections

The technical corrections are presented in terms of page and line number in the manuscript as much as possible. Note that there are a number of places where comma usage for the last item in a list is done inconsistently. Also, punctuation with regard to quotation marks appears to be done incorrectly in most cases. In the interest of space, these are not noted here.

Page 160, Line 1: I'm pretty sure it's the "Moderate Resolution Imaging Spectroradiometer."

Page 160, Line 2: The word "incredible," used to describe the dataset, is somewhat subjective and could perhaps be replaced with "extensive."

Page 160, Line 12: The abbreviation "DT" is not introduced, but it can be inferred from the context.

Page 161, Line 2: The abbreviation "DB," which appears in the "merged DT/DB product," is not introduced.

Page 161, Lines 2-3: "In addition to changes to the aerosol retrieval, C6 will include . . ."

Page 161, Line 11: The reference should be "Koren and Feingold, 2011"

Page 162, Line 8: Should it be a "constant effort," rather than a "consistent effort"?

Page 163: Line 1: "C6 will represent a continuous, consistent data record. . ."

Page 163, Line 4: ". . .retrievals in urban and suburban. . ."

Page 163, Lines 24-26: The list should be separated by commas.

Page 164, Line 2: ". . . or updated in the C6 aerosol retrieval. . ."

Page 165, Line 8: ". . . in the case of the aerosol retrieval. . ."

Page 165, Line14: I think the sentence should read: "MxD04 retrievals require input L1B files. . ."

Page 166, Lines 16-17: "non-cloudy"

Page 166, Line 27: The Kaufman et al., 1997 reference needs an "a" or "b" to specify which one.

Page 167, Line 6: The expression "more central visible wavelengths" is ambiguous. I believe the intention is to say "longer, visible wavelengths. . ."

Page 167, Line 13: Again, the Kaufman et al., 1997 reference needs an "a" or "b."

Page 168, Line 2: I appreciate the sentiment, but I question the ability of MODIS to resolve the presence of "puddles."

Page 168: Line 5: There doesn't need to be a comma in "Martins et al. (2002). . ."

Page 168, Line 7: What is the meaning of the term "arbitrarily" in this sentence? The implication is that there is no particular reason behind the choice of the thresholds over land and ocean.

Page 168, Line 14: ". . .represent conditions under which an aerosol retrieval is possible. . ." Is the word "possible" the appropriate choice in this context, or is the in-

tention rather to choose conditions under which an aerosol retrieval using the MODIS DT retrieval algorithm is likely to succeed?

Page 169, Line 1: Don't need a comma after "This means. . ."

Page 169, Line 26: "This means that, for both DT algorithms, the primary. . ."

Page 170, Line 27: ". . . extend the range of retrievals. . ."

Page 171, Line 1: I think the meaning of this sentence is "Although minor changes were made under the same Collection number."

Page 171: Line 19: The acronym for "AERONET" was already spelled out on the previous page.

Page 171, Line 23: Is it important to mention that the SPs come from "moving" ships?

Page 172, Line 11: No comma after "Yet. . ."

Page 172, Line 24: Are "local computing machines" equivalent to "local computers"? What does "local" refer to in this context? My assumption is that it is NASA's Goddard Space Flight Center (GSFC).

Page 174, Lines 15-16: The sentence should either read: "Subtle differences between 'layers' and 'levels' when computing transmission functions were confused" or "The subtle difference between 'layers' and 'levels' when computing transmission functions was confused."

Page 174, Lines 21-22: "In fact" is used twice in this sentence.

Page 176, Line 30: "These studies show that, on a global basis, the MODIS. . ."

Page 180, Line 25: As written, this sentence does not make sense. I believe the issue is that the first "conservative" refers to "cloud conservative," and the second refers to "clear sky conservative."

Page 183, Line 15: ". . . the QAC reduced to 0."

Page 183, Line 18: The word “cases” appears twice in this sentence. The second instance could likely be replaced with “observations.”

Page 183, Line 19: “Figure 4. . .” (typo)

Page 183, Line 21: It might be more correct to say, “. . . were potentially cirrus contaminated pixels that would have been tagged. . .”

Page 183, Line 22: “. . . AOD stay farther from. . .”

Page 186, Line 20: “. . . a larger relative error for low AOD cases.”

Page 188, Line 20: There should be a reference (Cox and Munk, 1954) to the statement that increasing wind speed diffuses the glitter pattern.

Page 189, Line 20: Should probably read, “. . . (Levy et al., 2009), but we describe them here as well.”

Page 190, Line 22: The sentence should probably read: “To undo the extra cirrus contamination. . .”

Page 193, Line 6: As discussed in the Specific Comments, the NCEP data packaged in the GDAS format for MODIS may more properly be termed an “analysis” as opposed to a “re-analysis.”

Page 194, Line 12: “. . . the large decreases ($\sim$0.04 or more) are in the midlatitudes of both. . .”

Page 194, Line 17: “. . . from 0.55 and 0.86 μm” (typo in units)

Page 196, Line 26: “This information is carried along in an array. . .” (no comma)

Page 198, Line 7: “. . . transition regions with comparatively low vegetation cover, but which are sufficiently. . .”

Page 199, Line 7: “(although by definition these products should be at least as good. . .”

Page 199, Line 12: The standard NASA definition of Level 2 (L2) products is “swath” as opposed to Level 3 “gridded” products. Using “along orbit” to describe the L2 products here is confusing.

Page 199, Line 20: Instead of “Greenwich Mean Time,” should probably just use “UTC”.

Page 200, Line 3: “Analogous to the selection process. . .”

Page 202, Lines 2-3: This sentence reads awkwardly. To keep the overall intention, it might be rephrased as, “Since before the launch of Terra, the MODIS aerosol algorithms were designed to retrieval aerosol information at 10 km resolution (at nadir).”

Page 202, Line 6: “. . . while producing manageable volumes. . .”

Page 202, Line 28: The punctuation around the “(Remer et al. 2012)” reference is confusing.

Page 203, Line 22: “Note that, initially for C6, the 3 km. . .”

Page 204, Line 8: “. . . requires information as to whether the ‘ocean’ or ‘land’ fork. . .” (missing “the”)

Page 204, Line 16: “This means that in any area. . .”

Page 205, Line 8: Should probably read, “. . . in MODIS-Terra’s blue channel calibration to lead to artificial trends. . .”

Page 206, Line 25: I’m not sure if a CDR is a “quality” or a “quantity” or something else.

Page 206, Line 26: There’s a space missing before the open quotes.

Page 207, Line 17: “. . . at least as good an aerosol product as the MODIS. . .”

Page 207, Lines 21-22: “. . . than that provided by MODIS.”

Page 208, Line 6: “. . . DT-retrieval algorithms themselves, but. . .” (extra comma)

Page 210, Line 9: The abbreviation “ROD” was introduced two sentences previously.

Page 211, Equation (A4): Missing a closing parenthesis.

Page 212, Lines 12-13: "... coefficients for a valid NCEP value..."

Page 212, Lines 20-21: I think what is meant by this sentence is "... and applied the values from table 9.5 of Jacobson (2005)."

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