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Interactive comment on "Sensitivity of aerosol retrieval to geometrical configuration of ground-based sun/sky-radiometer observations" *by* B. Torres et al.

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Review for Atmospheric Chemistry and Physics Discussions

Title: Sensitivity of aerosol retrieval to geometrical configuration of ground-based sun/sky-radiometer observations

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General Comments: This paper attempts to characterize differences in retrieval results of aerosol optical properties from two different types of sky radiance scans, the almu-

cantar (constant elevation angle equal to solar zenith) and the principal plane (constant azimuth angle equal to solar azimuth). The Dubovik and King (2000) inversion algorithm (with improvements made by Dubovik et al. (2006)) are applied both to simulations and AERONET measurement data. This is a reasonable strategy, however it appears that there were some significant issues that were not considered properly in both the simulations and the data analyses. First, for the simulation analyses, the authors utilized inversion results of size distributions and refractive indices from the Dubovik et al. (2002) paper that used an older version of the algorithm (particle non-sphericity was not accounted for) and with an erroneous Lambertian surface reflectance assumed (typical of green vegetation regardless of site location). Additionally the input data used in the Dubovik et al. (2002) paper were of poorer quality since interference filter stability was relatively poor prior to 1997 (resulting in larger calibration errors) and also since data quality screening was rudimentary compared to current screening. These issues and perhaps others may have led at least partially to relatively poor matching of input AOD versus output AOD in the simulations (simulated output values $\sim$10% higher than input at higher AOD levels in Table 1). Additionally, the lack of consistency in AOD levels for different sites in the simulations hinders an accurate assessment of differences between aerosol types since at higher AOD levels the influence of differences in surface reflectance and particle shape are reduced (i.e. the high AOD case for the GSFC site was 0.5 at 440 nm while for the Mongu site it was 0.8 at 440 nm). Numerous other issues with the simulation analyses are detailed in the 'Specific Comments' section below.

In the analysis of simulations against real AERONET data there are also issues that were either not considered or likely bias the analysis. To begin it is puzzling that the GSFC site analyzed in the prior simulations section was exchanged with the Beijing site while the other two sites remained the same. The AOD levels of the Beijing data analyzed were much higher than at GSFC and for the other sites (see Table 4), resulting in much lower uncertainties at Beijing in retrieved optical parameters and less solar zenith angle dependence due to the much larger aerosol signal at that site. It is also noted that identifying the Solar Village data as desert dust aerosol is not always accurate as this site more commonly exhibits mixtures of fine (sub-micron radius) and coarse mode (super micron radius) particles. The authors did not realize that the two subsets of data for the Solar Village site (bottom of Table 4) have significantly different Angstrom Exponent values (more than a factor of two difference) and therefore they are comparing mixed aerosol versus dust-dominated cases in the Desert Dust I and Desert Dust II subsets respectively. Even more importantly, for all sites studied, the data selection criteria (given on page 6879, lines 8-17) results in the chosen dates have relatively stable and homogeneous AOD (std. dev./mean < 0.1; criteria 2), and therefore having conditions that are not necessarily typical and that minimize the potential problems that Principal Plane scans have with inhomogeneous atmospheric conditions and cloud screening. Additional issues and comments regarding the real data analysis section are given below in the 'Specific Comments'.

As a result of the problems noted above, I cannot agree with some of the discussion and conclusions (sections 4 and 5) presented by the authors. In the Discussion section (pages 6886-6887) the authors say: "…the complete elimination of principle plane retrievals from the provided aerosol product leads to lose of valuable aerosol information, especially taking into account that both our sensitivity tests and real data analysis conducted in this study generally show high consistency between principal plane and almucantar retrievals." I argue that the analysis done is this paper does not accurately account for issues of aerosol inhomogeneity and cloud screening of the principal plane data that would result in both biases and higher random noise in principal plane retrievals relative to almucantar retrievals. Also the authors did not account for the fact that spectral AOD data (also primary input data to the retrievals) is less accurate at smaller solar zenith angles and thus may result in a significant error source for principal plane retrievals made when SZA is small (some sites/seasons at mid-day). Further details regarding issues with the Discussion and Conclusion sections are given below in Specific Comments.

The AERONET project has always placed data accuracy and the quality of products first and foremost in decisions regarding new data base versions and new products. It is for these reasons of retrieval product quality that the decision was made to provide retrievals using only the almucantar scan geometry. The AERONET team at GSFC has long been aware of the issues raised in this paper (and additional issues that were not mentioned in this manuscript) that affect the quality of principal plane retrievals. These issues are continually being analyzed and explored by the GSFC AERONET Team with the goals of providing the most consistent and accurate database possible from AERONET observations.

Specific Comments:

Page 6854, lines 3-5: The AERONET acronym should be all CAPS (not Aeronet, as you have used throughout the paper). Also, the way this sentence is written you imply that PFR-GAW instruments measure the same quantities as AERONET and SKYNET, however the PFR only measure spectral AOD (not sky radiances) and therefore do not retrieve other parameters such as refractive indices.

Page 6855, lines 3-6: Please also reference Holben et al. (2006; SPIE) as this reference also provides more detail on the minimum number of observations required for four different scattering angle range bins. This was an important new quality control check introduced in 2006 for the Version 2 almucantar retrieval products in order to improve retrieval robustness.

Page 6855, lines 16-17: It should be noted that operationally the maximum scattering angle in the principal plane scan is less than stated here since the observations cannot be made all the way to the horizon due to horizon obstructions and also due to inexact optical airmass computations (refraction effects, etc.). It is for the reason of difficulty of airmass computations at large airmass that the Level 2 AOD product from AERONET is limited to a maximum airmass of 5. At larger airmass values the aerosol vertical profile information is required for accurate computations.

Page 6857: Note that the Solar Village climatology from Dubovik et al. (2002) is more representative of mixed aerosols (fine & coarse mode size mixture) than pure 'desert dust aerosol" that you state here, with average Angstrom exponent (440-870 nm) ∼0.6. See Kim et al. (2011; ACP) and Eck et al. (2010) for more information on the selection of desert dust dominated cases and the dynamics of fine-coarse mode mixtures. Also note that the data base sample size was relatively small in 2000 (Dubovik et al. (2002) only used data through the year 2000), and that the current data sample size is much larger for all sites analyzed and also more accurate due to consideration of particle shape, more accurate surface reflectance and better input data screening for anomalies.

Page 6857, lines 13-16: How do you justify selecting different AOD levels for the GSFC and Mongu sites in your simulation analyses? Both are fine mode dominated aerosol types and the use of significantly different AOD levels confounds the comparisons you make. At higher AOD levels the aerosol signal is greater and the uncertainty level of the retrieved parameters decreases, therefore it seems that your comparison begins with some built-in bias.

Page 6857, lines 21-24: "It should be noted that the obtained spectral aerosol optical depth does not exactly match the input values provided as reference. This can be explained by the fact that the used aerosol parameters represent the climatological regressions." This is not really a satisfactory explanation for AOD differences of ∼10% at the higher AOD levels as shown in Table 1. Did you use the same particle shape and surface reflectance in your simulations as were used in Dubovik et al. (2002)? Note that surface reflectance in Dubovik et al. (2002) was held constant for all sites (no solar zenith angle dependence or geographical variation. All sites were assumed to have Lambertian albedo of 0.03, 0.06, 0.20, and 0.20 for 440, 675, 870 and 1020 nm respectively. Additionally the constraints on the tails of the size distribution (lowest and highest radii limits) were not as strong in the retrievals shown in Dubovik et al. (2002) as are utilized in the current (since Nov 2006) retrieval algorithm. A lack of

consistency in the retrieval algorithms plus assumptions (constraints) and input data here is a problem.

Page 6864, lines 4-5: You say there is no reliable information on aerosol vertical profiles. It might be more accurate to say there is little information, as CALIPSO does provide information in at least a climatological sense (if not more) on aerosol vertical profiles.

Page 6868, section 2.4: You need to also mention that geographically and temporally varying (16 day averages throughout the annual cycle) surface albedos were utilized in Version 2 retrievals. These spectral surface albedos are mid-day black sky albedos from Moody et al. (2005), and are based on atmospherically corrected MODIS data averaged over a 5km radius of each AERONET site (see Eck et al., 2008; section 2.3). The BRDF model and parameters (for each ecosystem type) are used to compute the spectral reflectances at solar zenith angles throughout the day.

Page 6869, lines 3-10: Can you quantify the errors in spectral albedo that results from this variation in BRDF model parameters that you assume? Otherwise it is difficult to assess whether you have analyzed a realistic range of spectral surface albedo uncertainty.

Page 6869, lines 11-13: For your analysis of the effect of surface reflectance errors on the retrievals you only show results for the largest aerosol AOD for each site. However this minimizes the effects of spectral surface albedo effects and BRDF in the retrievals since errors due to surface reflectance are much larger for low to mid levels of AOD. Therefore your analysis of this error source tends to minimize the potential errors and does not give the range of more typically encountered values due to this perturbation.

Page 6870, lines 5-7: The authors state: "The analysis of the results leaded to two main conclusion. First, the introduction of the random errors in the surface reflectance assumption did not results to any retrieval bias." This is misleading, since in reality the assumed surface reflectance can have a bias and this bias will result in biases in the retrieved optical properties. See Sinyuk et al. (2007) and Eck et al. (2008) for examples.

Page 6871, lines 16-18: "In summary, the observed differences in the optical parameters are relatively small considering the magnitude of the errors that we have introduced in the surface reflectance assumption." I argue that the uncertainty in the retrieved parameters due to surface reflectance uncertainty has not been adequately characterized in this paper since: 1) high AOD cases only were studied, which minimizes the surface effects, and 2) it is not possible to tell whether you have used realistic errors in surface reflectance in this study.

Page 6872, lines 17-21: I am puzzled as to why you only analyze the retrieval errors that result from the maximum pointing error (0.4 degrees) and not the more typical value of less than 0.1 degrees. As a result it is not possible from this section to know what the effects of a typical pointing error has on the retrievals.

Page 6873, lines 5-8: It should be noted in the text here that in actuality the left and right sides of the almucantar data scan are averaged for AERONET processing. This averaging is not just a hypothetical possibility, as suggested in this sentence, but in reality this is operational and has always been applied in AERONET data processing (based on a decision made over a decade ago as a result of much analysis). This results in numerous benefits versus the principal plane scan as you have stated previously.

Page 6877, lines 1-4 & lines 17-19: You state that the real part (and imaginary part in lines 17-19) is more stable for the biomass burning aerosol type than for the urban aerosol, but you need to mention that the AOD levels are not the same for these simulations (the high AOD cases have GSFC at 0.5 and Mongu at 0.8 at 440 nm). Therefore these comparisons are biased from the start since several sources of uncertainty diminish as AOD increases.

Page 6879, lines 5-6: Why did you change from GSFC to Beijing for the urban aerosol category while the other two sites (Mongu and Solar Village) remained the same? You

need to justify and fully explain why you did not maintain continuity with the same urban aerosol site used in the analysis done in the previous (simulations) section.

Page 6879, lines 8-10: "...not asked for in retrievals..." is awkward wording and confusing, so please rewrite this sentence. Even though you did not use Level 2 retrievals (since you want to analyze data for SZA<50 degrees) you should still apply data quality checks such as sky error <5%, ensuring good fit between measured sky radiances and those computed from the retrieved aerosol parameters. Additionally the symmetry check should be applied to the measured almucantar radiances to screen for clouds and other non-homogeneities. Was there any attempt to screen the principal plane scans for clouds? Did you only use cases where AOD>0.4 at 440 nm and if you did not then justify why lower AOD cases were analyzed.

Page 6879, lines 18-22: The analysis of real data is for a relatively small sample size, only 6 to 12 days per site/subsite data set and therefore possibly not very representative of all conditions, especially for partial cloudiness and aerosol inhomogeneity since you have only selected days with stable AOD and therefore less cloudy and more homogeneous conditions.

Page 6880, lines 3-4: You say all the data from Beijing is from fall and winter, yet Table 4 shows data from August (2 days; summer) and one in March (meteorological spring) also, so there is an inconsistency here. Also you need to mention in this section that the very high AOD levels at Beijing results in significant reduction in retrieval errors since the larger aerosol signal dominates all of the sources of uncertainty. Therefore comparison of Beijing to the other sites begins with a built-in bias towards better results from Beijing.

Page 6880, line 4-6: You state that an Angstrom Exponent (alpha) threshold of higher than 1.7 was used for Beijing, however this is not a typical Angstrom exponent value for this site even for months when there are no desert dust transport events. The alpha values in Beijing are typically significantly lower than 1.7 even for pollution events since the fine mode (sub-micron radius) particle size is relatively large (high AOD results in greater coagulation rates) and there is a larger coarse mode background value in Beijing than other urban sites, partly due to fly ash from coal combustion (see Eck et al. (2010) and references therein).

Page 6880, lines 10-19: You need to show the mean Angstrom exponent in Table 4 for each day (in addition to the mean AOD), especially for the Solar Village data that you split into two populations. The Desert dust I (Solar Village) population has mean Angstrom values ranging from 0.42 to 1.1 except for one day with 0.20, therefore this population should be described as "Mixed fine/coarse" rather than "Desert dust". On the other hand, the Desert dust II population has Angstrom exponent ranging from 0.11 to 0.24, except one day that alpha of 0.47, therefore this population has true desert dust aerosol (see Kim et al. (2011) for dust selection criteria using AERONET data). Therefore differences between retrievals from these two data populations for this site can in part be attributed to differences in aerosol type and you need to mention this in the text. Additionally, data quality issues are apparent in some of the days in the Desert dust II population. For example, on some dates in the Desert dust II population, there are spectral crossover of 440 and 500 nm AOD which indicates AOD calibration uncertainty resulting in larger AOD errors at small solar zenith angles (SZA). See Hamonou et al. (1999; JGR) equation 1 showing that the error in AOD due to calibration is proportional to 1/m (m=optical airmass; m$\sim$1/cos(SZA)). Therefore it is likely that retrieval errors (biases) for both scans are larger at smaller solar zenith angles. It is necessary to include some discussion of the uncertainty in AOD as a function of SZA in this section, and in other sections of this paper. Other errors including sky radiance inconsistencies at 6 degrees azimuth angle (indicating sky radiance calibration issues; these will be quality-control screened in a future AERONET data base version) are evident in some of the days in the Desert dust II data but not in the Desert dust I data. Therefore input data quality also varies between the two Desert dust (Solar Village site) data populations.

Page 6881, lines 27-28: Please explain why Beijing was chosen in this section in place of GSFC, since this confounds the ability of the reader to assess the differences in both simulations and real data, as can be done for both Mongu and Solar Village sites.

Page 6882, lines 1-2: I disagree with your suggestion that the mean spectral single scattering albedo (SSA) values for Beijing are similar to other urban sites shown in Dubovik et al. (2002). See Figure 19 in Eck et al. (2005) that shows less wavelength dependence of SSA at Beijing than the other strongly absorbing urban sites. Additionally the fine mode particle radius is significantly larger at Beijing (at the high AOD levels you studied) than for GSFC, Mexico City, Maldives and Cretiel (urban sites shown in Dubovik et al. (2002)).

Page 6882, lines 15-16: It should be noted that the 0.03 uncertainty in SSA for AERONET almucantar retrievals (Dubovik et al., 2000) is largely due to sky radiance calibration uncertainty, and the 0.03 value is for AOD of $\sim$0.4 at 440 nm, while for the Beijing data analyzed in this paper the SSA uncertainty is much lower due to much higher AOD of the cases studied.

Page 6883, lines 12-14: It seems that you are confusing uncertainty in almucantar retrievals due to combined effects of calibration, pointing error, surface albedo, etc. with the consistency between almucantar and principal plane retrievals. Therefore it seems that there is no 'improvement' to talk about.

Page 6883, lines 20-28: You say that the discrepancies between Desert dust I and Desert Dust II cannot be attributed to differences in the aerosol measured, but a quick check of the data for the dates you analyzed show that there are significant differences in Angstrom exponent and therefore it is to be expected that there should be differences in aerosol properties for these two Solar Village data subsets. You compared size distribution parameters in this section but did not look at any measure of the relative contribution of fine versus coarse mode in AOD (fine mode fraction (FMF) or Angstrom exponent). Eck et al. (2010) and Eck et al. (2008) have shown significant variation in

SSA and size distribution as a function of FMF and/or Angstrom exponent.

Page 6885, lines 11-14: It should be mentioned here that the better agreement between almucantar and principal plane retrievals for larger solar zenith angles is also due in part to more accurate AOD input data to the retrievals at larger SZAs.

Page 6886, lines 4-6: Averaging of the left and right branches of the almucantar scan not only results in more 'stable' measurements but also a more representative spatial (angular) distribution of sky radiances due to aerosol inhomogeneity. This should be added to your discussion.

Page 6886, Line 25: Please note that SZA also exceeds 50 degrees at mid-day in winter in subtropical to mid-latitudes, not just at high latitudes as you suggest.

Page 6886 line 28 through Page 6887 Line 7: You suggest here that valuable information is being lost by not providing the principal plane retrievals as a data product within AERONET. However the only advantage that principal plane retrievals have over almucantar retrievals is a larger solar zenith angle range (allowing for mid-day retrievals in the tropics and in summer in mid-latitudes), due to sky radiance measurements made over a greater range of scattering angles. However your reasoning does not consider a balanced approach since there are numerous disadvantages that the principal plane scan has relative to the almucantar scan. Lack of rigorous cloud screening and lack of ability to account for aerosol inhomogeneity are two significant factors that hinder the robustness of the principal plane scan retrievals. Your 'real' data analysis presented in this paper does not assess the effects of these issues since you deliberately selected only dates where the AOD was very stable thereby excluding many cases where the principal plane would be disadvantaged. You studied additional disadvantages of the principal plane such as greater sensitivity to pointing errors and aerosol vertical profile than almucanatrs, but yet did not consider in the analysis presented that spectral AOD (a key algorithm input parameter) is less accurate at smaller solar zenith angles. In summary you need to at least modify your statements to account for the fact that the

principal plane retrievals will often be of lower quality (larger uncertainty), and especially so at lower solar zenith angles.

Page 6887 lines 8-11: It is quite inappropriate for you to be mentioning the new hybrid scan concept in this paper since the idea originated at GSFC by the AERONET group after much analysis, and there are no GSFC co-authors on this manuscript. Additionally, even then it is premature to mention in this paper since this is still in the early development phase.

Page 6887, lines 13-27: The opening statement in the conclusions that considers only the ideal case is a poor way to begin discussion on principal plane versus almucantar scan results. Additionally a failure to mention the advantages the almucantar has when the there is partial cloud cover is a serious omission here in the Conclusions section. Another significant omission in the Conclusions and the entire paper is the lack of consideration of AOD accuracy and how it varies as a function of solar zenith angle.

Page 6888, lines 16-19: You did not realize that there are significant differences in Angstrom exponent and FMF between the Desert I and Desert II subsets signifying different aerosol properties, therefore this conclusion is invalid and that part of the paper needs to be re-analyzed or rewritten to take consideration of these issues.

Page 6888, lines 26-28: It is somewhat surprising that you consider this a new conclusion (need to limit almucantars to SZA>50 degrees), as it was well known for over a decade.

Minor Comments/Corrections:

Page 6853, lines 3-5: In line 4 please replace 'error' with difference' to be consistent.

Page 6860: Figure 3 is quite small and difficult to read in standard size format (I had to enlarge to at least 200% on my monitor screen). This applies to many other figures in the paper as well.

Page 6865, lines 14-16: The 'right' and 'left' in the text here are inconsistent with your labels in Figure 5.

Page 6865, lines 23-25: This seems like almost a repeat of the sentence in lines 14-16, maybe it can be rewritten to be less repetitious?

Page 6867, line 16: typo: “find mode” should be “fine mode”

Page 6867, line 22: “aerosol vertical distribution centered at the surface” should be something like “aerosol vertical distribution with maximum at the surface” or a clearer description of the vertical profile. Same comment applies to the caption of Table 2.

Page 6868, line 14: typo: “for operational” is repeated in this sentence.

Page 6868, line 20: The word ‘aleatory’ is very infrequently used. I suggest using something like ‘unpredictable’ or ‘random’ unless you want to force readers to check a dictionary.

Page 6868, line 22: You should mention that the current AERONET version is Version 2, and also reference Dubovik et al. (2006) since that algorithm includes non-spherical particle scattering effects.

Page 6870, lines 3-4: “. . .the improvement in the principal plane is considerably more notorious.” The word notorious is a very poor word choice here. Same comment for Page 6871, line 4.

Page 6870, line 16: The radius value of 0.02 micron appears to be an error since this is smaller than the minimum radius given in the retrievals.

Page 6872, line 4: Can a web address be given where a pdf of Torres et al. (2012) is posted? Otherwise this work is inaccessible to most readers.

Page 6872, line 9: typo – “Sun bean” should be “Sun beam”

Page 6872, lines 14-15: Please give a brief summary of the methodology used in Torres et al. (2012) that was used to assess the pointing error.

Page 6873, line 12 & Fig. 9: Note that the caption of Figure 9 (last sentence) has an error in description of the y-axes of the plots. These plots are all single scattering albedo plots, not refractive indices.

Page 6878, line 2: “. . .considering the instrument field of view as punctual.” The word ‘punctual’ does not make sense in this context.

Page 6878, lines 3-5: Please give a reference for the FOV of the old and new versions of the Cimel.

Page 6879, lines 25-26: Please give the Angstrom exponent threshold values that you used in data selection.

Page 6881, line 21: The placement of horizontal lines in Table 5 is confusing, splitting the data for some sites but not others.

Page 6882, lines 14 & 24: I think you probably mean SZA>50 here (both sentences) instead of <50 degrees.

Page 6888, line 8: “The low sensitivity in the extremes provokes as well that the values in the extreme radii of the size distribution. . .” Poor English here, resulting in a very confusing sentence. Please reword this sentence.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 6851, 2013.