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Atmospheric Measurement Techniques Discussions

Interactive comment on "The Airborne Multiangle SpectroPolarimetric Imager (AirMSPI): a new tool for aerosol and cloud remote sensing" *by* D. J. Diner et al.

Anonymous Referee #3

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This paper is well written, but despite its length suffers from a lack of relevant detail with which claims and assertions can be evaluated. More quantitative statements are required as described below to make this paper of use to a reader interested in this potentially interesting instrument. There is little detail provided on instrumental accuracy, either polarimetric, or radiometric, with the only statement on the accuracy of the polarization measurements made by this instrument being that "Preliminary results for AirMSPI show similar residuals." Given that the method used for polarimetric analysis is a time varying modulation it would be desirable to know how the measurement technique behaves as a function of polarizer orientation (i.e. modulation depth) "as a polarizer is rotated in front of the camera." Some clear statement of accuracy is also

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Interactive Discussion
Discussion Paper

highly desirable so that when looking at figures such as 11) and 12) and seeing statements such as “Agreement is very good.” the reader can make their own assessment of how good the agreement is. On page 5 at the beginning of section 2.3 it is irritating to have the FWHM bandpasses separated from the band centers, which are given on p.2. Repeating this information is desirable. In a paper of this kind it would be good to show the solar spectrum with trace gas absorption and the MSPI bandpasses on the top so that any issues with modeling of absorbing species are apparent. Failure to do so raises concerns about the widths of the 445, 470 and 660 nm bands all of which have full widths at half maximum of roughly 40 nm and are in the vicinity of various oxygen (O2 and O2-O2) and water vapor absorption features. On page 5 referencing an abstract to justify the absence of polarization measurements in the UV seems a little underhanded. Moreover it does not do justice to the details of the trades associated with the fairly rapid transition from a domain where aerosols can have substantial impacts on polarization and polarized reflectance to the deep UV where the polarized reflectance will indeed be saturated by the molecular contribution. Since this transition occurs over the 350-450 nm spectral range I would regard the statement that “polarization channels in the UV would not offer significant benefits for aerosol retrievals” as tendentious and unjustified. My suggestion would be to either delete this comment, or provide more detail/quantification regarding what level of molecular scattering is regarded as large enough to eliminate “significant benefits for aerosol retrievals”. While it may well be true that “extinction ratio determines the magnitude of the modulation pattern of the PEMs, and is readily accounted for in instrument calibration” to not provide the actual extinction ratios for the three polarized bands is not acceptable. These extinction ratios will have an effect on the detectability of polarization, precisely because the magnitude of the modulation is dependent on them. If MSPI data is ever to be used by anyone other than the author this is the type of information that must be made available. On page 6 the statement is made that “When all samples are combined over a frame the effective quantization is 16 bits.” Please elaborate. For a 16-bit ADC one would usually expect to get an effective number of bits of around 14 for a careful design that has an adequate dark offset and sufficient dynamic headroom. I would therefore be interest to know exactly how 23 9-bit measurements give "nearly 16 bits". As above the author should bear in mind that this paper may well be used by those interested in looking at MSPI data and so comments that are germane to instrument performance should be carefully considered and well justified. The "validator" sounds interesting, but if it is made of sheet polarizers illuminated through a plastic diffuser I would be interested to know what magnitude of polarization is actually achieved and how stable it is. Usually stimulators of this kind are prone to stray light with consequent reductions in the observed polarization. The description provided provokes the readers interest without adequate information to assess what level of performance the "validator" provides and consequently raises concerns about whether the claimed fraction of 1 mrad stability is realistic. I would suggest including more recent references to the use of polarization observations over land and ocean at the beginning of Section 3.2 viz., O. Dubovik, M. Herman, A. Holdak, T. Lapyonok, D. Tanré, J.L. Deuzé, F. Ducos, A. Sinyuk and A. Lopatin, "Statistically optimized inversion algorithm for enhanced retrieval of aerosol properties from spectral multi-angle polarimetric satellite observations," Atmos. Meas. Tech. 4, 975–1018 (2011). O. Hasekamp, P. Litvinov and A. Butz, "Aerosol properties over the ocean from Parasol multiangle photopolarimetric measurements," J. Geophys. Res. 116, D14204 (2011). J. Chowdhary, B. Cairns, F. Waquet, K. Knobelspiesse, M. Ottaviani, J. Redemann, L. Travis, and M. Mishchenko, "Sensitivity of multiangle, multispectral polarimetric remote sensing over open oceans to water- leaving radiance: Analyses of RSP data acquired during the MILAGRO campaign," Remote Sens. Environ. 118, 284–308 (2012). otherwise the reader may think the authors are unaware of the limitations of some of the subsequent analysis. On p.12 it is stated that Mixture 8 has a fine mode with median size of 0.03 μm and standard deviation of lognormal radius distribution of 0.5. By my calculations that corresponds to an effective radius of 0.056 μm, which seems awfully small. While the size may be correct I would suggest that, since the size distributions and the parameters that define them are not written out in this paper, the effective radius of each mode also be stated to eliminate any ambiguity. I would also note that observationally non-absorbing aerosols are almost non-existent and the difference between AERONET and MISR would be consistent with typical aerosol single scattering albedos of 0.95-0.98. On p.13 it is noted that a “depolarizing bidirectional reflectance distribution function” is used. The authors should probably note that this only works for the 470 nm polarized observations because they are in the principal plane where the ocean body contributions to the observed polarization are quite small cf. Chowdhary references. On p.14 it is stated that “Agreement is very good.” It is necessary in scientific papers to provide a measure of how good. In particular differences between model and measurements in DoLP of up to 0.1 at high view angles are not consistent with that statement. Another concern is that the standard deviation of the observed DoLP appears to be comparable to the claimed accuracy after average over 100 x 100 m target areas. This suggests very low SNR. Is this the case? Also, if averaging the correct measure of uncertainty for the mean is the standard error not the standard deviation. Please correct these omissions and/or errors. It is then speculated in the same paragraph that “the coarse mode in the MISR lookup table is too large.” What is the basis for this assertion? Is it the AERONET observations, additional modeling, or the fact that there is a bow. If it is simply the presence of a bow, then it is at least as likely that shape is an issue as size and such comment should be made. The comparison of wind speed used in the model with SSM/I is unconvincing for two reasons. 1) There is no reason to expect a monthly mean wind speed to agree with an instantaneous wind speed on a given day. 2) Although the shape of the sunglint is almost always well modeled by a Gaussian distribution modified by Hermite polynomials for wind direction the link between the distribution of surface slopes and the wind speed provided by Cox and Munk has large uncertainties. I would suggest eliminating the “reasonable value” wind speed discussion, or if not include uncertainties in the wind speed inferred from the ocean BRDF model and the standard deviation of monthly mean wind speed from SSM/I. On p.15 the phrase “Based on AERONET results” is used. Is this a climatology, or from the almucantar retrieval technique. If it is from the almucantar retrieval technique please

AMTD
6, C550–C555, 2013

Interactive Comment

reference one of the Dubovik papers that provides uncertainties on the products. On p.16 Diner et al. is referenced which is fine, but since the land surface model appears to be the usual ocean model with a pre-factor and fixed refractive index of 1.5 it would be appropriate to reference one of the papers where that ocean model was introduced. Otherwise someone might think that the 2012 Diner et al. paper is the origin of this usage. Since the DoLP is independent of radiometric calibration it would be nice to see comparisons between model and measurements for DoLP for the observations over land. Also, it is stated that "The negligible values of al at 355 nm indicate that the surface has negligible effect on the top of the atmosphere measurements either because the surface is intrinsically dark or because the atmosphere is so hazy at this wavelength." In point of fact the work of Torres et al. and the various papers published documenting UV albedo from GOME observations indicate that the lack of sensitivity to the surface is because the surface is intrinsically dark AND the atmosphere is "hazy"/opaque. The subsequent use of the phrase "sensible data" warrants re-wording and appropriate reporting of actual quantitative measurements of goodness of fit. On p.18 it is suggested that cloud base can be estimated from cloud "reflections". If clouds were pyramidal this would clearly by true. However, since the cloud reflections are generated primarily by diffuse radiation that is then specularly reflected off the ocean the actual height of cloud material that is being estimated by looking at the distance of the "reflection" from the cloud is probably more closely related to the height of maximum projected area. I would also suggest that the high polarization is related to the use of the 46° camera, which means that the reflected cloud light has experienced a reflection off the ocean close to the Brewster angle. A more nuanced description of what is being observed would be helpful and it is not clear that any value is added by the reference to Lin et al. On p.19 cloud top is "assumed to be at 1 km altitude". Why? Does this come from stereo, the magnitude of Rayleigh at 470 nm in side scattering angles, some other method? Please explain. In point of fact as shown in the original Bréon and Goloub paper and in more detail in Alexandrov et al. no removal of Rayleigh is necessary in the estimate of cloud droplet size from observations of cloud bows. In fact, as shown in Alexandrov et al., when 3D effects/shadowing are present the removal of Rayleigh using a model of the kind given in eq. (1) would be deleterious to the use of polarized reflectance. Again on p. 19 when the phrase "optically thick" is used a value should be given, since a cloud is thick for polarized reflectance purposes at an optical depth of $\sim$3. While there is nothing wrong with the use of Daimon and Masamura refractive indices in this case, in general the Harvey, Gallagher and Sengers paper referenced by Daimon and Masamura is to be preferred at colder temperatures and certainly for anything associated with supercooled water. In the comparison between Daimon and Masumura and the Harvey et al. formulation I was also concerned that the difference in refractive index between the two in the near infrared was directly correlated with the rapid increase in liquid water absorption. While I know that Harvey et al. take absorption into account in their estimate it was not clear to me that Masamura and Daimon did and I would therefore be cautious in using their results beyond 700 nm. On p.21 in the conclusions "we have presented quantitative interpretations" requires that quantitative measures be reported for all comparisons. If you do not report quantitative measures when comparing model and measurements this phrase in the conclusions would have to be changed to "we have presented qualitative interpretations". Minor Comments: I am not sure that the parenthetic remark that (MISR by design is polarization insensitive) is germane to the description of a new instrument and its results. Similarly on page 3 saying what you are not doing (diattenuation balancing), is not relevant to the reader. It is preferable to describe what you are doing.

Interactive comment on Atmos. Meas. Tech. Discuss., 6, 1717, 2013.