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

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We thank the reviewers for their comments and suggestions. Detailed responses are provided below.

Referee #1

1 I am looking forward to seeing the retrieved results from AirMSPI and their validation with AERONET. The retrieval algorithm will be an crucial tool for using full advantages of AirMSPI.

We agree. This work is in progress.

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2 Nevertheless, the manuscript corresponds to the scientific and technical quality requirements of AMT. I think it can be accepted for publication in AMT in the present state.

Thank you for your comments.

Referee #3

1 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.”

This paper is intended to serve as an introduction to the AirMSPI instrument, to describe its design and evolution from its precursors (LabMSPI, GroundMSPI), and show examples of the instrument’s data, in some cases using models to demonstrate that the measurements follow expected patterns. Detailed descriptions of calibration procedures and results require entire papers by themselves; here, we are trying to strike a balance between introducing the readers to this new instrument vs. overwhelming them with details best dealt with elsewhere. We agree with the reviewer that publication of such details is essential, and intend to do so in subsequent papers. In several instances, we feel that the level of detail the reviewer is requesting would derail the readability of this paper. In the lead author’s experience as MISR PI, it is not possible for a single publication to provide every bit of desirable information. We will include further details in response to the reviewer, and in so doing make what we think is reasonable judgment regarding the scope of this paper.

2 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

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of polarizer orientation (i.e. modulation depth) “as a polarizer is rotated in front of the camera.”

This subject and the associated methodology was discussed in greater detail in Diner et al. (2010). A note to this effect will be added to the manuscript. The relevant sentence will also be clarified to indicate that the polarizer rotation is stepwise so as to not give the incorrect impression that the external polarizer is moving while the modulation is occurring.

3 Some clear statement of accuracy is also 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.

Quantitative measures of agreement between the data and models will be added.

4 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.

Section 2.3 will be rewritten to include this information.

5 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.

As noted by the reviewer, the paper is already quite long. In Section 2.3 we refer to current efforts to characterize the system spectral performance in detail and the preparation of a separate publication incorporating the results. Rather than further extend the length of this paper, we feel the reviewer’s suggestion is best accommodated in the separate paper on spectral performance.

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6 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”.

Further elaboration is beyond the scope of this paper so the sentence and reference will be deleted from the rewritten Section 2.3.

7 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.

The text already notes that extinction ratios vary between 40 and 100 among the different polarimetric channels. What matters most is the worst case performance, and a discussion in Diner et al. (2010) will be referenced, noting that in the worst case the loss of SNR due to attenuation of the modulation pattern leads to <0.0009 random uncertainty in DOLP.

8 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

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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 interested 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.

We thank the reviewer for calling attention to this sentence, which indeed contained a calculational error. The digitization scheme used by AirMSPI generally keeps quantization noise <50% of shot noise. The text will be corrected.

9 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.

The validator is not designed for absolute polarimetric calibration, but rather to provide a sufficiently polarized input to the AirMSPI camera such that the shape of the modulation waveform can be used to derive the average PEM retardance. The degree of linear polarization of the validator varies across the camera field of view, and has average in-flight values of 0.79, 0.88, and 0.16 at 470 nm, 660 nm, and 865 nm, respectively. This discussion will be added to the paper.

10 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.

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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.

The Dubovik et al. (2011) reference is already included at the beginning of Section 3.3, and Hasekamp et al. (2011) and Chowdhary et al. (2012) are already included at the beginning of Section 3.2. We don’t understand what change the reviewer is requesting.

11 On p.12 it is stated that Mixture 8 has a fine mode with median size of 0.03 microns and standard deviation of lognormal radius distribution of 0.5. By my calculations that corresponds to an effective radius of 0.056 microns, 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.

The size distributions are stated to be monomodal and bimodal lognormal distributions, and the parameters have been provided. However, we do not object to including effective radius and these values will be added to the text.

12 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.

As stated in the text, the AERONET site is 170 km from the AirMSPI scene. The AERONET AOD is included to show that the MISR retrieval is reasonable, but given the spatial separation one should not overinterpret small differences between the MISR and AERONET values.

13 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.

Our goal throughout this paper is to show how the AirMSPI observations stack up against simple models. Use of a coupled aerosol-hydrosol model, as in Chowdhary et al. (2006), Appl. Opt. 45 and Zhai et al (2010), JQSRT 111, is beyond the scope or purpose of this paper. Nonetheless, these references will be added to show that more sophisticated modeling is possible.

14 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.

Quantitative measures of agreement will be added to the paper.

15 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?

No. The reviewer seems to have inferred that we first averaged up the data to 100 m x 100 m resolution, and then computed standard deviations on the reduced resolution imagery. This is not the case. The observed standard deviations were calculated at full spatial resolution within the 100 m x 100 m target areas. Since the result combines measurement uncertainty with actual spatial variability, the measurement uncertainty is at least as good or better than the values shown. We will change “over the 100 m x 100 m patch” to “within the 100 m x 100 m patch” to clarify this point.

16 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.

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The metric is used to describe variability within the patch, which as noted above can arise from both measurement uncertainty as well as actual spatial variability. Hence, the standard deviation is the correct quantity.

17 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.

It is the fact that the model predicts a bow that is not seen in the AirMSPI data. The text will be modified to reflect this possibility that shape is also an issue.

18 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.

The reviewer is correct that no exact correspondence between a monthly mean and instantaneous value should be expected. Our point was that the derived wind speed is a reasonably expected value drawn from a statistical population having the quoted mean. Nonetheless, we will respond to point 1) by replacing the monthly mean value with the SSM/I value observed within a few hours and at the same location and date as the AirMSPI overpass. This value turns out to be 8 m/sec. With regard to point 2), Bréon and Henriot (2006), JGR vol. 111, used scatterometer and POLDER data to conclude that “the Cox and Munk model of the surface slopes pdf, developed more

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than half a century ago from a few airborne photographs of the Sun glint, allows an amazingly accurate fit of the distributions observed from space over all oceans. The model fits the slope probability over two orders of magnitudes. In particular, the mean squared slopes, both upwind and crosswind, show a near-perfect agreement between the CM modeling and the satellite data.” 19 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 reference one of the Dubovik papers that provides uncertainties on the products.

We will clarify that this is based on almucantar results and add a reference to Dubovik et al. (2000), JGR 105, 9791-9806.

20 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.

We will add a reference to Priest and Meier (2002), Opt. Eng. 41, 988-993 for the polarized facet model, and reference Waquet et al. (2009) (already in the paper) for the use of refractive index of 1.5.

21 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.

These data will be added to the plots.

22 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

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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 sentence will be rewritten to capture the reviewer’s point.

23 The subsequent use of the phrase “sensible data” warrants re-wording and appropriate reporting of actual quantitative measurements of goodness of fit.

The text will be modified and quantitative metrics added where appropriate.

24 On p.18 it is suggested that cloud base can be estimated from cloud “reflections”. If clouds were pyramidal this would clearly be 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.

The discussion will be qualified to indicate the assumptions about the cloud morphology.

25 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.

Mention of the Brewster angle will be included. The reference to Lin et al. is included to show that the derived value falls within an expected range.

26 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.

1 km is a typical height for marine StCu clouds, and is used to provide an estimate of

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Rayleigh scattering above the clouds, as was done by Bréon and Goloub (1998). As noted in the next point, the results are insensitive to this assumption.

27 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.

As discussed in the Alexandrov et al. (2012) paper, both those authors and Bréon and Goloub (1998) include a term to account for Rayleigh scattering in their parameterizations, but in their original work, Bréon and Goloub correct for Rayleigh scattering assuming a cloud top height of 1 km. Careful reading of the Alexandrov et al. (2012) paper found no mention of the assertion that 3D effects are deleterious to the removal of Rayleigh. Nonetheless, we analyzed the data using both the Bréon and Goloub equation, including Rayleigh removal, as well using the Alexandrov et al. equation without Rayleigh removal. The derived droplet sizes are unchanged. Additional text will be added to the paper to note this.

28 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 ~ 3 .

The use of the term optically thick is in the same sense as used by Bréon and Goloub, such that $\exp(-\tau/\mu)$ is negligible. All that is meant is that this term has been neglected, following Bréon and Goloub.

29 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 Masamura 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

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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.

The reviewer seems to prefer the formulation of Harvey et al. (1998) to the actual measurements of the refractive index reported by Daimon and Masamura (2007). As noted in our paper, the temperature of the cloud top observed by AirMSPI is likely to be around 21°C, so the measured data from Daimon and Masamura for 19°C was taken to be representative for this case, which is well above the freezing point of water. As noted in Daimon and Masamura, the fit derived by Harvey et al. was done for the visible portion of the spectrum and the uncertainty in the formula is 1×10^{-3} in the infrared. Even so, agreement between the Daimon and Masamura measurements and the Harvey et al. formula is within a few $\times 10^{-6}$ from 400 to 900 nm, which includes the MSPI polarized wavelengths.

30 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”.

Quantitative measures of agreement will be added to the paper.

31 Minor Comments: I am not sure that the parenthetical remark that (MISR by design is polarization insensitive) is germane to the description of a new instrument and its results.

A future spaceborne MSPI instrument conceptually resembles MISR in the inclusion of multiple view directions. We are simply pointing out a distinction between the two instruments.

32 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.

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This paper is third in a sequence describing first, the LabMSPI monochromatic imaging polarimeter; second, the GroundMSPI multispectral imaging polarimeter; and third, AirMSPI, the airborne version. We felt it useful to document lessons learned as the technology development progressed. The comment about diattenuation balancing is one of those lessons.

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

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