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Atmospheric Measurement Techniques Discussions

Interactive comment on "Design and characterization of specMACS, a multipurpose hyperspectral cloud and sky imager" *by* F. Ewald et al.

Anonymous Referee #2

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GENERAL COMMENTS – MAJOR STRENGTHS AND ISSUES

Strengths

* Spatially resolved spectral measurements with 3D vertical profiling of clouds and aerosols will help understand the physics of mixing at small scales. This paper describes an instrument intended to acquire such measurements, details its ground-based calibrations, and provides preliminary results from airborne measurements.

* Although they are very idealized, the instrument characterizations performed are thorough with well explained results in both the text and the figures. The authors have

presented figures (particularly Figs. 8-16 and 18-20) clearly showing results of FPA characterizations, instrument throughput and sensitivities, and initial science data. §5 describes these results well and §6 nicely describes preliminary cloud measurements.

* Use of national calibration facilities for end-to-end instrument characterizations allows standardization and inter-comparisons with similar future instruments.

Issues

* This paper should focus more on results of the specMACS rather than general background of FPAs, radiometry, and characterizations. For example, §4.1 and §4.2 describe photon and noise sources, polarization effects, non-linearities, and bad pixels. Nearly all of §4.1 is a tutorial on characterizing FPAs. That is already done in other publications and needn't be reproduced here. What is presented in §4 is generic and presents no new or specific information regarding the specMACS itself. For this reason alone, ***this section should be eliminated,*** referencing other papers for general FPA and characterization background. The only remaining items should be those specific to how the specMACS was actually characterized, and they should be moved into §5. As currently written, this generic background section (§4) detracts from the actual specMACS characterization results. §5 and parts of §4.2 are where the paper gets to be non-generic.

- ***My suggestion would be to eliminate §4 altogether and add only the necessary specMACS specific items to §5, where the specMACS results are presented.***

* §1.1 fails to clearly show requirements flow and derive radiometric accuracy requirements (vs. goals).

- Absolute accuracy requirement is not well derived. "We aim for an absolute radiometric calibration uncertainty of 5% or below" seems a bit arbitrary from the preceding text, especially when followed by a 10% radiance error.

- "...a spectral bandwidth and accuracy of 10–50 nm is supposed to be sufficient" is

not very definitive. What is this based on? What are specMACS' driving spectral requirements?

- It is ambiguous whether the accuracy requirements are 5, 10, or 20%.

* Does 2 μm in aerosol radius correspond to a 20% radius error (for what type of aerosol?), and does this in turn correspond to 20% in radiance uncertainty? Confusingly presented.

* Is 20% at 2100 nm truly an absolute radiometric accuracy and not just a relative accuracy across the spectrum? This seems more a relative rather than absolute radiometric accuracy requirement.

* ***Uncertainties are not addressed.*** No attempt is made to demonstrate that they sum to less than the required accuracy (which is not clear itself). Thus, the paper's claims that the requirements are sufficient for the desired atmospheric measurements are not justified.

* §4.1 and §4.2 describe idealized effects only. Non-ideal effects are largely neglected. Examples include:

- thermal background from the instrument itself;

- polarization leakage through the wire grid calibration polarizer (which varies with wavelength);

- the use of air conditioning to hold ambient air temperatures constant (HVAC systems are bang-bang closed loop feedback systems with thermal variations that must be accounted for);

- the difference between nominal and actual FPA integration times (which was nice to see identified in §4.1.3 – although how this difference varies with integration time was not described).

* Several sections of the instrument itself provide far more details than needed. For

example, ***each of §3.2.x is overly detailed and should be shortened*** to a sentence or two and consolidated into a single §3.2. These details are not particularly relevant to the overall instrument capability for achieving and demonstrating radiometric accuracy. The corresponding figures (5, 6, and 7) should similarly be eliminated as they provide no new information to the paper.

* Conclusions: ***The Conclusion fails to summarize the intended and achieved requirements or present justifying uncertainties to show those requirements were met.*** Result #3 (R being stable over time) is not demonstrated via any long-term data. This result is also unclear on the radiometric accuracy level obtained and needed (10%, 5%?).

LESS MAJOR COMMENTS

* §5 is where this paper gets to be relevant for specMACS. Figs. 8-16 nicely show results.

* The beginning of several sections includes repeated background that was presented in prior sections, making the paper disjoint. These redundancies in descriptions and even acronym definitions should be removed by the authors to better focus on the paper's intended results. Examples include:

- The first paragraph of §4.2.4. (The remaining four paragraphs in that section, however, are a good example of the specMACS-specific characterization details that should remain in the paper, albeit moved to §5.)

- "Either k [DN] varies with signal level S0, which would cause a photon response non-linearity (PRNL) or a charge sharing is occurring between pixels which would violate the Poisson assumption" is redundant with §3. Suggest removing from §3 when shortening/eliminating that section and including such statements only here in §5.

* Some of the calibrations are lacking in thoroughness. These are not sufficient to reject the paper, which describes what the authors actually did and which is still relevant for

the instrument's characterization, but overlooked calibration items such as those listed below should be acknowledged as lacking in the paper.

- §4.1.2: Both FPAs are sensitive to thermal background, so will never have S0=0 even with incident light blocked. That thermal background similarly scales with integration time, providing an additional component affecting Eqn. 5.

- Thermal characterizations seem to have been done inadvertently (via flight results) rather than controlled intentionally as part of the planned calibrations (§4.1.2).

- Diffraction contributions to angular resolution are never mentioned (§5.2.1).

- §4.1.1: FPN can change with temperature and intensity. These variations do not seem to be taken into account in characterizations.

- §4.1.3: Non-linearities are only characterized by changing integration time with constant signal level. This folds in variations in integration time and does not allow for true FPA non-linearities due to varying incident intensities. Varying intensity levels can give different non-linearity results than obtained by only varying integration time.

- Calibrations would be more convincing than mere stray light modeling (especially since that modeling was not done prior to initial detection of stray light problem, which, although seemingly obvious, apparently came as a surprise to the authors).

* §4.1.1: Poisson distribution presumes the dark noise during each readout is large. Is it?

* Read noise in §5.1.1 is conflicting. The text gives 5.0 and 4.5 DN for the VNIR and SWIR, whereas the y-intercept from Eqn. 15 and 16 would instead indicate 5.07 and 4.77 DN respectively. (These latter, from Eqn. 3, include dark current noise, but the text claims the read noise is from this y-intercept alone. And no explanation is given for how the read noise and the dark current noise are differentiated at the low signal levels here.)

* §5.1.4: This is a response of the entire system, not just the cameras (FPAs) since it includes the transmissivity of the optical system. Text suggests it is only the response of the FPAs.

* §5.1.5: The maximum error would be P, not P/2, assuming the (unknown) input light is completely polarized in a direction of minimum or maximum instrument sensitivity.

- Conclusions section: Result #9 (polarization sensitivity) should be 10% and not 5%.

* No discussion is given of long-term stability of instrument sensitivity or cleanliness environment in which the instrument is stored to reduce optical surface contaminants and maintain calibration accuracy.

* Usage of "spectral channel" by number in text (§5) and figures (Figs. 12, 14, and 15) is meaningless to a non-specMACS reader. Convert to wavelength units.

* Should delete Figs. 1, 5, 6, 7, and 17, which do not show relevant characterizations or results.

TYPOGRAPHICAL AND GRAMMATICAL SUGGESTIONS

* There are many typographical and grammatical mistakes throughout the document. I started listing these but eventually decided that with the major reductions/eliminations I suggest of §3 and §4 that the authors should improve the paper's grammar, spelling, and punctuation with that rewrite. Such corrections are more their responsibility than mine, and I assume the authors or the editor will find and correct these prior to final publication. I do include several such corrections below.

* Past, present, and even future tenses are often mixed even in single sentences. Needs consistency.

* Overuse – and in other places the lack of use – of appropriate commas is persistent in paper.

* Some acronyms, such as LIS, are defined repeatedly; and some are then not even

used.

* The word “data” is plural but is incorrectly used throughout this paper with a singular verb.

* “Can not” should be replaced with “cannot” in most instances.

* Naming of figures is inconsistent (“Fig.” vs. “Figure”).

* A “line camera” (Abstract and later in text) implies a single-row array rather than a 2-D spatial/spectral FPA.

* What are “core sensors” in Introduction? Suggest removing.

* §2.1: λB is undefined.

* §3: What is “MiB”?

* Eqn. 3: σdark is not defined.

* §4.2.6: What does “problem dependent” mean?

* Figure 10: Are values in DN, as caption states, or DN range in percent, as plots show? (The latter seems correct, but caption conflicts with plots and with §5.1.2.)

* Some examples of needed typographical and grammar corrections are listed below with their current (rather than corrected) incorrect wording underlined (see PDF supplement):

- Abstract: “Equipped with a high spectral and spatial resolution, the instrument...”

- Abstract: “...the spatial and spectral performance was assessed.”

- Lots of “e.g.”s break up flow in Introduction

- Introduction: “...improvements of our understanding ... is pursued.”

- Introduction: “...measurements becomes possible.”

- Introduction: “High spectral resolution measurements are needed” would be better than “Spectrally high resolved measurement is needed...”

- §2: “A scanning setup allows to capture...”

- §3: “...the system must be able to run fully autonomous...”

- §5.1.4: There are lots of unnecessary “case of” pairings that should be removed.

- §5.1.5: What does “...the polarization dependent signal loss becomes maximal for the particular pixel” mean?

- §5.2.1: “...LSF for the VNIR sensor is show in Fig. 8a”

- §5.2.1: “For both spectrometer the strongest keystone distortion occur...”

- Conclusions: “...allowed to measure...”

- Conclusions: “...allow to estimate...”

- Conclusions: “...straylight...”

- Conclusions: “...achived...”

- Conclusions: “...requierments...”

- Conclusions: “...exemplarily...”

These are just some examples, but in their rewrite the authors should clean up many other such typographical and grammar errors.

Please also note the supplement to this comment:
http://www.atmos-meas-tech-discuss.net/8/C3459/2015/amtd-8-C3459-2015-supplement.pdf

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