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

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Introduction

We thank referee #1 for his/her careful reading, comments and suggestions which we address in the following. The authors' answers are printed in italics:

> *Remark: The figure numbers in the referee comments and the page numbers in the authors' answers are corresponding to the original manuscript. If not stated otherwise, figure and equation numbers in the authors' answers are referring to*

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the revised, marked-up manuscript version (showing the changes made) which can be found attached to this answer.

General comments

- The manuscript provides a detailed characterization and uncertainty estimation of the hyperspectral line imager specMACS which is build for ground-based and airborne application. The measurement covers the solar spectral range with medium spectral resolution (2.5-12 nm) and therefore is intended to be applied for cloud and aerosol remote sensing. The authors describe several issues which have to be considered for spectral imaging sensors and investigated these by extended laboratory calibration. Theory and results are presented in high detail which proves that the instrument performance is well understood and sensor deficits (e.g., non-ideal behavior) can be corrected by postprocessing. Finally exemplary measurements highlighting the potential of the instrument for cloud remote sensing are shown. In future spectral imaging will become more popular to investigate atmospheric processes and different systems will be operated world wide. Comparing measurement of different systems requires knowledge about the sensor performance and common calibration procedure. In this regard, the manuscript provides an important contribution to current and future research and is worth to be published as it substantially helps to access instrument uncertainties of spectral imaging systems. However, in my opinion the manuscript lacks of two major issues which have to be reassessed in detail before publishing the manuscript. First the manuscript is quite long and difficult to read due to an unfortunate choice of structuring by the authors. Second the investigations are not finalized in a ways, that the uncertainty estimates are not transferred to a real measurement case even though measurement examples are given. Additionally

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I have some comments on the calibration methods which may change the interpretation of the results. Below, I compiled a list of comments which have to be considered in a revised version of the paper. There might be some contradictory statements resulting from my misinterpretation of the text when first reading. I am sure the authors will know how to weight in such cases and how to improve the text to avoid misinterpretations by other readers.

→ *Thank you very much for your time and effort in compiling this thorough and detailed review! Attached to this answer you will find a diff for the revised manuscript. As one mentioned issue affected the overall structure of the manuscript, we first restructured the text before doing the diff. By doing so, all suggested reductions/changes can be tracked more efficiently. Please also note our answers to referee #2.*

Major comments

- Length of the manuscript The manuscript is quite long, not well structured and therefore some times hard to read. I found two reasons. First, the objective of the manuscript is merged between 1) Describing the design (hard- and software) of the instrument and 2) the performance and calibration. I agree, that 2) can not completely be discussed without 1) but, considering the length of the manuscript I would prefer to focus on the performance and calibration and remove all passages which are not essential for the performance of the instrument in terms of measurement uncertainties. Some parts I suggest to remove, at least drastically reduce or move into an appendix are:
    - Software description (P9863, 10 - P9864, 19): Data acquisition does not improve the measurement performance in terms of radiometric, spectral or

spatial accuracy. Reduce to what is finally connected to the instrument performance (measurement frequency, exposure time, dark current).

- Detailed instrument concept (Sect. 2.1): Are there any aspects given in this section which are related to or needed to explain the calibration results what is the main subject of the manuscript? The section reads like a very detailed description of a common spectrometer concept. Are there any references which can be cited in order to reduce this section to a minimum?
- Auto Exposure (Sec. 3.2.1.): The Auto exposure is again not changing the calibration results and was certainly not applied during calibration. A short note, that in field measurements exposure is adjusted automatically to a set of integration times might be sufficient. All details of how the decision to change integration time is made can be removed or moved into an appendix.
- Scriptable measurements (Sec. 3.2.3.): See comment on software description.

→ *In our restructured version of the manuscript, following sections have been drastically reduced or moved into the appendix:*

- ⋆ *Removed: "Detailed instrument concept" and "Software description"*
- ⋆ *Moved into the appendix: "Auto Exposure" and "Dark current measurements"*

We left a reduced version of "Instrument automation", where the reader is referred to the detailed descriptions in the appendix. See the diff attached to this answer to see all reductions in detail.

This list might not be complete. I recommend the authors to have a closer look into the manuscript and decide what parts are really necessary and which not. The second reason increasing the length and reducing the readability of the manuscript is the separation of the calibration studies into two sections. Section 4 explaining the theory and Section 5 presenting the results. This is very unfortunate and additionally not consequently realized. With this structure it is hard to understand a theory of whatever effect if no measurements are shown. Also some non-ideal sensors behaviors have been anticipated in Section 4 before there is evidence for the reader. Therefore, I suggest to merge section 4 and 5. First explain the calibration procedure, then the results and finally discuss the results with help of the theory. This structure will make it easier to understand your findings. Readers do not have to move back and forth all the time. Several repetitions in the test can be removed. Also in general, I recommend the authors to go through the manuscript and check if any repetitions not providing any new information can be removed. Some specific suggestions where to shorten, what to remove, are given in the minor comments below.

→ *Thank you for this suggestion! We restructured the manuscript as you suggested: Each calibration theory section is now directly followed by the results and the discussion of results. All passages which were not essential for the instrument performance have been removed and we focused more on performance and measurement uncertainties. The diff of the revised manuscript attached to this answer was done after this restructuring. This was done to keep track of all additional changes and reductions (otherwise the diff would not really be helpful).*

- Overall uncertainty budget There is no overall uncertainty budget given. Only radiometric uncertainty in combination with dark signal, which is somehow part of the radiometric calibration, are discussed. In practice also the other effects (signal magnitude, noise, spectral uncertainty, polarization) will contribute to the overall uncertainty. The reader is somehow left alone to judge which of the described effects will be of importance during a real measurement. My first suggestion is to differentiate and note in the text, which effects contribute to the overall uncertainty and which effects could be corrected on basis of the calibration performed. This should be made clear to the reader in order to make sure which uncertainty finally remains. Additionally an estimate of how the overall uncertainty differs when single calibrations are not considered would be helpful. How large is the improvement of performance for each calibration? Some might not be worth the effort if to be repeated for specMACs or a similar system. In this regards, the uncertainty presented in Fig. 13b does not consider one point. In Fig. 13 the uncertainties are only discussed for a distinct measurement (laboratory measurements). However, when atmospheric signals are week, then the contribution of dark signal and noise uncertainties will increase. Especially over dark surfaces and in absorption bands (H2O). I suggest to show how uncertainties depend on the radiance or for real atmospheric measurement (see next comment). The uncertainties will certainly differ from the laboratory measurements using an integrating sphere.

→ *Thank you for this suggestion. I think we really missed out on this point, since the combination of all uncertainties into an error budget and a subsequent application to real-world measurements is of highest interest to the reader.*

For this reason, we added a new section "Overall radiometric uncertainty budget", where we show how the different uncertainties can be summed up to an overall uncertainty - this can be found in the diff below this text. Furthermore, we now give detailed equations/descriptions, how the dark signal uncertainty, nonlinearity uncertainty, uncertainty due to polarization, instrument noise and calibration uncertainty can be evaluated for real-world applications. The different uncertainties are then combined into the overall radiometric uncertainty budget.

We agree, that the radiance uncertainty certainly differs for real atmospheric measurements. See our next answer, which will cover this issue.

- Application to measurement data Section 6 shows some nice measurements indicating the potential of the instrument. However, no measurement uncertainties are given. Without error bars the value of the measurements for the intended applications is not clear. Uncertainties have to be added, especially when all the sections before were mend to estimate the instrument uncertainties. So why not demonstrating in Section 6 what is learned from the extensive characterization. Suggestion 1: Error bars in Fig. 19 for all wavelength and spectra. Maybe also relative uncertainties below. Discuss which wavelength ranges has which uncertainty. As I discussed above, the different radiance values will cause a different contribution of dark signal and noise to the overall error. This should be shown here. Suggestion 2: The same holds for the image presented in Fig. 20. Different spatial pixel in a scene of different illumination will have different uncertainties due to dark signal and noise. Fig. 20 presents an excellent example of an inhomogeneous cloud with radiance differing over magnitudes. Thus I expect uncertainties (relative and absolute) in the image to be different at different areas of the image. Showing this for two representative wavelengths of VNIR and SWIR will help to understand how the uncertainties will potentially migrate into cloud retrieval.

  → *This suggestion really helped us to improve the overall quality of our manuscript! Armed with the knowledge determined during the instrument characterization, we now combine all given uncertainties into an overall radiometric uncertainty. Using the equations in Sec. 3.1.6, the radiometric uncertainty can now be calculated "online", corresponding to instrument settings and signal levels.*

  This way, the first suggestion was implemented by providing error bars to Fig. 22 (former Fig. 19) for all wavelength and spectra. Additional, the radiometric error in different wavelength regions are discussed and compared to each other. In the same way, we implemented the second suggestion by providing "2D images" of the overall radiometric uncertainties corresponding *to the scene shown to Fig. 23. Please see the diff attached to this answer for the revised/new plots in the application section.*

In the conclusion section, these results/plots are now summarized in a new paragraph which reads:

> *The available error budget calculation now allows to estimate the significance of different radiometric uncertainties. For the VNIR, major contributions to the overall radiometric uncertainty of around 5 % are caused by the calibration uncertainty of R (error of $\approx 3\,\%$) and the polarization sensitivity for highly polarized light (error $\leq 5\%$ for fully polarized light). Without the nonlinearity correction, the radiometric signal would furthermore be strongly biased (-9 % at high signal levels). For the SWIR, major error contributions to the overall radiometric uncertainty of around 10 % are caused by the uncertainty of the absolute radiometric standard itself (error of 5 to 10 %, $\lambda > 1700\,\mathrm{nm}$) and the dark signal drift for low exposed regions (error of $20\,\%$ and more, depending on the frequency of dark frame measurements).*

- Approach to vary sensor signal for nonlinearity calibration The authors investigated the non-linearity of the radiometric calibration and the signal noise by changing the integration time while using a constant illumination. This is one approach but in my view not the right choice to investigate nonlinearity of the calibration. With constant illumination the number of photons arriving at the sensor does not change. Varying only the integration time does only increase the time of photon collection. This has one implication. As the authors wrote, the temporal mean of noise is zero. Increasing integration time therefore should reduce noise (Nphot at the same time. A more appropriate approach which is closer to reality is changing the illumination (radiance of integrating sphere) despite integration time. The measurements usually are done at certain integration time

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while the radiance changes due to clouds, etc.. Therefore the radiometric calibration mostly accounts for the radiance changes and not changes of integration time. The question is now, how linear is the response of the sensor to changes in radiance. This was not investigated here but is a known issue of other sensors.

→ *You are of course right, this is only one approach and might not be the best possible approach to characterize the nonlinearity of the sensor. We are well aware of this problem, but are also facing the difficulty to establish a light source of which the intensity can be precisely varied while still maintaining a perfectly stable spectral intensity distribution. Furthermore, this light source must also be bright enough to illuminate the sensor sufficiently well. Currently, we are not able to provide such a light source within the necessary precision to characterize the instrument in the suggested way. To keep track of this problem, we added a note in the conclusions section:*

> *Due to the difficulty of establishing a bright light source with spectrally stable and precisely linearly adjustable intensity, the radiometric nonlinearity has not been investigated directly in terms of incoming radiance alone. A deeper investigation of this behavior might show additional nonlinearity effects.*

However, this is not the only way to characterize a nonlinear behavior. Our characterization showed that the chosen nonlinearity parameters are identical for each pixel of a sensor, implying that the nonlinear behavior of all pixels is the same. Assuming that this is also true for the nonlinear behavior, there is another possibility to assess the nonlinearity in radiance. Under constant, isotropic illumination, different pixels of the sensor are illuminated with different radiances due to the spectrograph. Using this fact together with the assumption of identical nonlinear pixel behavior, any desired model can be tried out to fit the captured data: Distinct nonlinear behavior in integration time or radiance (with parameters constant over the sensor array) *using the different illumination at each pixel. If there really would be a nonlinearity in radiance alone (as opposed to total collected energy $\propto L \cdot t_{\mathsf{int}}$), we would expect a matching fit of a model considering this behavior. During our investigation, we only found one model which showed a good fit: a model nonlinear in the product of radiance and integration time as described in the manuscript. One should be reminded, that such a model is not nonlinear in integration time alone (which was also tried out but should no good fit).*

We extended section 3.1.2 ("Nonlinear radiometric response"), elaborating on this thought:

> *During this analysis, some alternate nonlinearity models have been considered to improve the confidence in the existing nonlinearity parameterization, which is assumed to be a function of total collected radiative energy ($\propto L \cdot t_{\mathsf{int}} \propto s_{\mathsf{n}} \cdot t_{\mathsf{int}}$). A simpler model, considering only a quadratic term in t_{int}, was not able to provide similarly good results as the model presented above. Some combinations of quadratic or higher order terms in the form of $s_{\mathsf{n}}^a \cdot t_{\mathsf{int}}^b$ have also been tried, assuming equal nonlinear response of all pixels of one sensor and exploiting the intensity variations between pixels as introduced by the spectrograph. As the assumption of equal nonlinear response for all pixels has been found to hold true for the finally chosen model and neither of the alternate models showed better results, they have also been discarded. This behavior suggests, but is no evidence, that the signal is actually a nonlinear function of the total collected radiative energy and neither in t_{int} nor L alone.*

Based on this argument and knowing that this is not a final evidence, we are still quite confident that other nonlinear effects will not be dominant. We subsequently added a statement to the quoted part of the conclusion:

> *(5) Due to the difficulty of establishing a bright light source with*

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> *spectrally stable and precisely linearly adjustable intensity, the radiometric nonlinearity has not been investigated directly in terms of incoming radiance alone. A deeper investigation of this behavior might show additional nonlinearity effects. There is some indication that these additional effects might not be dominant, as suggested in Sec. 3.1.2.*

Regarding the noise changing with integration time: during all investigations of the nonlinear behavior, we used averages of several hundred frames to suppress the noise to a negligible level. We do not think that the consideration of remaining changes in noise would yield major differences in the described nonlinear behavior.

Minor comments

- **P9855, 4:** Why two infrared sensors are given here? SpecMACS is measuring solar radiation. There exist also solar spectral satellite sensors.

  → *That is correct. We replaced the two sensors with two solar spectral satellite sensors:*

  Page P9855, Line 4ff

  > *Since then the exploitation of atmospheric and particle absorption has led to the development of spaceborne measurement platforms like the Moderate-resolution Imaging Spectroradiometer (MODIS) or the Earth Observing-1 Mission (EO-1) for spectral remote sensing of trace gas profiles and cloud properties.*

- **P9855, 24:** There is some literature which investigates the adding value of spectral measurements which might also helpful to motivate the use of hyperspectral imaging. Coddington, O., P. Pilewskie, and T. Vukicevic (2012), The Shannon information content of hyperspectral shortwave cloud albedo: Quantification and Practical Applications, J. Geophys. Res., 117, D04025, doi:10.1029/2011JD016771.

→ *Thank you for this hint - we included the reference into our manuscript.*

- **P9856, 11:** There is a break in the text when reading. After giving an outlook of the paper you jump back to describe existing instrumentation. A new subsection with title may help.

  → *We added a new subsection called "Conceptual embedding" since the following paragraph mentions the existing instrumentation as well as the intended application.*

- **P9856, 27:** Typo: "become"

  → *Thank you, corrected.*

- Introduction in general: I'm missing a review on existing hyperspectral imaging systems and their application in atmospheric sciences. How they compare to specMACS?

  → *A review on existing hyperspectral imaging systems was indeed missing. We added a new paragraph intro the introduction, giving examples of already existing instruments and their atmospheric applications.*

  Added paragraph at Page P9856, Line 1ff

  > *There already exist some imaging spectroscopy instruments for the ground-based or airborne remote sensing perspective. In the visible wavelength range, one of the earliest instruments was the Compact Airborne Spectrographic Imager (CASI, Babey and Anger*

(1989)) with 288 spectral channels (2.5 nm *resolution). Over the years, CASI measurements were used in various applications in atmospheric sciences. Naming only a few, Wendling et. al. (2002) investigated aerosol-radiance interactions, Mayer et al. (2004) determined water cloud droplet size distributions using the backscatter glory and Zinner and Mayer (2006) assessed retrieval biases due to inhomogeneity of stratocumulus clouds. Further cloud remote sensing applications were done with the AisaEAGLE instrument from SPECIM, which covers the spectral range between 400–970* nm *with a spectral resolution of 2.9* nm*. From the ground-based perspective, Schäfer et al. (2013) retrieved cirrus optical thickness and ice crystal shape, while Bierwirth et al. (2013) and Schäfer et al. (2013) used the instrument to retrieve optical thickness and effective radius of Arctic boundary-layer clouds from the airborne nadir perspective. The Airborne Visible/InfraRed Imaging Spectrometer (AVIRIS, Green et. al. (1998)) extended the measurement range into the near-infrared spectrum with 224 spectral channels (10* nm *resolution) between 400–2500* nm*. Gao et. al. (1993) already used it to detect cirrus clouds using the information in the near-infrared, while Thompson et. al. (2015) used the higher spectral resolution (5* nm*) with 600 spectral channels of AVIRIS-NG for the remote detection of methane. A further imaging spectroscopy instrument is the Airborne Prism EXperiment (APEX) imaging spectrometer (Itten et al., 2008; Schaepman et al., 2015) with 532 spectral channels and a spectral resolution between 0.9–12.3* nm*. Exploiting this high spectral resolution, Popp et al. (2012) used APEX for high-resolution remote sensing of NO2. With 1056 spectral channels in the 400–2500* nm *spectral region, the specMACS instrument continues the development of atmospheric radiation measurements to-*

wards imaging spectroscopy.

- **P9857, 27:** Can you give a reference confirming the accuracy range.

  → *In the revised manuscript, we remove this specific accuracy requirement, since it was unfounded as also noted by reviewer #2. However, we reformulated the spectral accuracy requirement as follows:*

  P9857, 27ff:

  > *Spectral accuracy requirements are not too strict for current microphysical cloud retrievals, as no sharp absorption line is evaluated. However, the solar spectrum itself exhibits many narrow absorption lines. For this reason, the spectral accuracy should be comparable or better than the spectral bandwith of the instrument. The radiometric accuracy can be compromised if resolved absorption lines are spectrally misaligned.*

- **P9858, 1:** You may add here, that the high spectral accuracy is mostly needed for VNIR.

  → *We included this comment in the mentioned sentence:*

  Spectrally high resolved measurement is mostly needed in the VNIR spectral range where many narrow absorption features are located, e.g., for photon path analysis using the optical depth of the oxygen A-band or for the detection of surface albedo influence on the basis of known spectral vegetation features.

- **P9858, 5:** In this discussion I'm missing how the uncertainties will migrate into the final retrieval results.

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→ *As no specific retrieval is shown later in the application section, the reader is referred to a publication discussion this influence on O2A retrievals in detail.*

P9858, 5 now reads:

> *As shown by Heidinger and Stephens (2000), the retrieval of the total column optical depth of the oxygen A-band is limited by the spectral resolution of the instrument.*

- **P9859, 11:** That's irritating. The instrument is build for ground-based operation but samples of airborne observations are shown. Why? Also later in section 6 no example of ground-based measurements is shown. When no ground-based measurements are presented I also see no need to describe the scanning mount.

→ *In the revised manuscript, the airborne example is left out in this paragraph as suggested by you in another item. Furthermore, we now included also samples of ground-based observations in Sec. 6. To harmonize this subsection with the airborne setup, we also moved the description of the scanning mount into the application section. The new application subsection reads:*

1 Ground-based and airborne applications

First deployments of the specMACS instrument were the ground-based measurement campaign HOPE in Melpitz, Germany in September 2013 and the aircraft campaign ACRIDICON CHUVA in the Amazon region around Manaus, Brazil in September 2014. (...)

1.1 Ground-based setup

(... description of the mount ...)

1.1.1 Ground-based measurements

An exemplary data set, measured during the ground-based campaign, is given in Fig. 19. The first panel (Fig. 19a) shows a true-color image that was rendered using spectral radiance data from the VNIR camera. Here, corresponding scattering angles towards the sun are shown as isolines. The next two panels show calibrated radiances for the same scene as they were measured with the VNIR spectrometer at $870\,\mathrm{nm}$ *(Fig. 19b) and with the SWIR spectrometer at* $2100\,\mathrm{nm}$ *(Fig. 19c). The more structured appearance of clouds at 2100* nm *can be attributed to shorter photon pathlengths due to a higher absorption by cloud droplets at this wavelength. Furthermore, the slightly lower radiance from cloud tops at 2100* nm *could be an indication for larger cloud droplets. This new perspective on clouds is an essential step towards the proposed microphysical retrievals from cloud sides (Zinner et al., 2008; Martins et al., 2011), since up to now, most imaging spectrograph instruments were designed for the nadir-looking perspective. (...)*

- **P9859, 12-18:** This section is not well placed. Until here the instrument and especially its spectrometer cameras with its specifications are not introduced. Without this information the exemplary data cube does not help because the reader may not understand where it comes from. I suggest to move or even remove Fig. 2.

  → *In the revised manuscript, we removed the former Fig. 2 altogether.*

- **P9859, 26:** Is the entrance slit similar for both camera systems?

  → *Yes, it is (*$\approx 30\,\mu\mathrm{m}$*).*

- **P9860, 1-26:** I'm not sure if this entire section is needed. Are there any aspects related to or needed to explain the calibration results which is the main subject of the manuscript? The section reads like a very detailed description of a common spectrometer concept. Are there any references which can be cited in order to reduce this section to a minimum?

  → *You are right - we restricted the section to the most important aspects. The reader is now referred to Aikio (2001) where the specific implementation of the used hyperspectral instrument can be found.*

- **P9861, 2:** What "PFD" stands for? Introduce abbreviation.

  → *"PFD" is the model name of the VNIR spectrometer given by SPECIM. As far as we know, the model name is no abbreviation.*

- **P9861, 16:** What "SWIR" stands for? VNIR was introduced.

  → *Correct, we added:*

  > *(which in the following is referred to as the shortwave infrared, SWIR)*

- **P9861, 22:** What is the dynamic range of both spectrometers? Can you provide num- bers.

  → *Good idea. The usable (noise-limited) dynamic ranges of the sensors (*$9.5\,\mathrm{bit}$ *for the VNIR,* 11–$11.6\,\mathrm{bit}$ *for the SWIR) have been added to the text. The digitized dynamic ranges are still mentioned in the sensor parameter summary tables. We also added the additional* $5.6\,\mathrm{bit}$ *which are potentially available (non-instantaneously) through the auto-exposure algorithm.*

- **P9861, 26:** Wording: "Just like in the case of" change to "Similar to"

  → *Thanks, we changed that.*

- **P9862, 3:** Why these important parameter are not discussed here in the text? In section 2.2 and 2.3 a lengthly explanation of all single components (inculing type names etc.) is given but the most important parameter describing the main characteristics of the system are not discussed. E.g. pixel number, nominal spectral resolution, FOV, dynamic range, sampling frequency, etc.

  → *For both sensors we added following description of the main characteristics:*
  Added at P9861, Line 13:

  > *It provides a resolution of* 1312×800 *pixels with a pixel distance on chip (pixel pitch) of* $8\,\mu\mathrm{m} \times 8\,\mu\mathrm{m}$ *and an active optical area of* $10.48\,\mathrm{mm} \times 8.64\,\mathrm{mm}$*. The field of view (FOV) along the spatial line is* $32.7\,^{\circ}$*, while the instantaneous field of view (IFOV) for a single pixel is* $1.37\,\mathrm{mrad}$ *across and* $2.00\,\mathrm{mrad}$ *along the spatial line. The entrance slit width of* $30\,\mu\mathrm{m}$ *limits the average spectral resolution to* $3.1\,\mathrm{nm}$ *with an average spectral sampling of* $0.8\,\mathrm{nm}$*. Further parameters can be found in Table 1.*

  Added at P9862, Line 3:

  > *The FOV along the spatial line is* $35.5\,^{\circ}$ *while the IFOV is* $3.79\,\mathrm{mrad}$ *across the spatial line and* $1.82\,\mathrm{mrad}$ *along the spatial line. The entrance slit width of* $30\,\mu\mathrm{m}$ *limits the average spectral resolution to* $10.3\,\mathrm{nm}$ *with an average spectral sampling of* $6.8\,\mathrm{nm}$*. Further parameters are listed in Table 1.*

- **P9862, 6-10:** The nature of stray light is somehow obvious. I suggest to remove this introduction of the section.

→ *Removed.*

- **P9862, 14:** Does the stray light affect both camera systems in same magnitude?

  → *No, the SWIR is affected more. A note has been added.*

- **P9862, 17:** "FOV" number was not given in the text jet. And the abberation was not explained.

  → *Now, FOV is already explained in the subsection "VNIR spectrometer"*

- **P9862, 19:** Is there any example image comparing the same scene with and without baffles illustration the efficiency of the baffles?

  → *The final stray light baffles have been constructed in a way which does not allow fast installation and removal. However, we added a plot which prominently illustrates the effect of the stray light protection. We used a prototype of the stray light protection, which can be placed and removed faster and therefore allows for direct comparision in the same scene. See Fig. 3 in the appended manuscript diff attached to this answer.*

- **P9863, 3:** What is the IFOV of both cameras?

  → *The IFOV of both cameras is now already given in Sec. 2.2 and 2.3*

- **P9863, 3-8:** I do not see the need to explain all this details especially the handling of the system. Restrict to the most important parameters such as the accuracy of the rotation stage. All other things which improve the convenience of the instrument operation are add-on which do not improve the scientific output of the system.

→ *Removed the mentioned lines 3-8 at P9863.*

- **P9864, Sec. 3.2:** Structure. There was no discussion about the dark signal of the system jet. The reader still does not know how good or bad dark signal of the system is and if there is a need to consider it at all. I would suggest to move this part to the end after all the components and problems of the system are described. The section is more connected to the application in field measurements based on the finding of the instrument characterization. Similarly this holds for the automatic exposure (nonlinearity of radiometric calibration) and dark frames (drift of dark signal).

  → *We moved the automatic exposure and dark frame section into the appendix. We left a shortened version of sections 'Instrument automation' and 'Scriptable measurements', since we think they belong and contribute to the "Design and (...) of specMACS" (title of the manuscript).*

- **P9865, 13:** "All CMOS pixel": That means spatial and spectral?

  → *Correct, changed the wording into:*

  > *... which is evaluated in real-time over all spatial and spectral pixels.*

- **P9865, 26 and 29:** Translate frames also into a time.

  → *Added a translation of frames into a time:*

  > *... 150 frames ($5\,\mathrm{sec}\,@\,30\,\mathrm{fps}$) ... 1800 frames ($1\,\mathrm{min}\,@\,30\,\mathrm{fps}$)*

- **P9866, 12:** Variation of dark signal has to be shown first.

  → *The section is now moved into the appendix and thus after the main section which shows the variation of dark signal.*

- **P9867, 10:** "auxiliary data" is misleading. Dark current for example is essentially for the radiometric calibration. I would suggest to include dark current into the radiometric calibration. That's basically where it is applied.

  → *This is indeed a confusing formulation. We now differentiate more clearly between the mostly stable sensor characterization and the faster varying (measured) dark signal, orientation data or sensor settings. It is true that dark current is applied during radiometric calibration. However, we want to emphasize that we do not characterize the dark signal in the laboratory but rather measure it in close succession with the illuminated signal. Therefore, we keep it separated from the sensor characteristics.*

- **P9867, 9-12:** Discrimination between characteristics and auxiliary data is misleading. What do you mean here? Above it was stated that three characteristics are required. Now auxiliary data is needed as well.

  → *This should be fixed together with the previous comment.*

- **P9867, 21:** "IMF" abveration had already been introduced.

  → *Thanks, removed.*

- **P9867, 22:** "DLR" abveration had already been introduced.

  → *Correct, removed.*

- **P9868, 6:** "Bad pixel" are not part of sec. 4.1.

  → *Thanks, we removed the reference to "bad pixels" in this sentence.*

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- **P9868, Sec. 4.1.1:** Why starting with noise? The reader still does not know about the characteristics of the dark signal. I would suggest to start with dark signal, then radiometric calibration and finally noise.

  → *This seems to be a reasonable recommendation! During the larger reorganisation of the overall structure, we moved the noise subsection at the end of the characterization section.*

- **P9869, 4:** Ndc should depend on temperature, right?

  → *Yes, this is correct. However, this should only be relevant for the SWIR spectrometer, where the largest part of the dark signal is the dark current signal level (comparable with the photoelectric signal for long integration times). For the VNIR, the read-out noise level seems to be larger than the dark current signal (there is almost no dependence of the dark signal level on integration time) and thus we see no strong impact of Ndc variations due to temperature variations.*

- **P9869, 7:** I would suggest to discuss the theory after showing the results of your calibration. Just add a theoretical curve into your plot. Then it is easier to understand for the reader.

  → *In the revised manuscript, a short introduction about the noise origin is given, followed by the description of the noise analysis and its results. After showing the results, the theory about the Photon Transfer Curve (PTC) introduces the discussion of the noise analysis results at the end of this section. Since the square fit in our plots is the theoretical curve given by the PTC, this suggestion is already covered.*

- **P9869, 20:** How stable is you integrating sphere? Due to noise in the current the radiance output may already have some noise level.

→ *We added the following additional sentence at P9873 after L2:*

> *As determined by Baumgartner (2013), the output stability of the LIS is better than* $\sigma = 0.02\,\%$ *for a duration of 330 seconds.*

- **P9869, 26-27:** Explain how these parameter (which?) are applied for the correction? This again suggests to change the order of your structure. First calibration, then noise.

  → *During the suggested restructuring of the manuscript, this part got deleted. The description of the application of the characterized parameters (radiometric response* R*, nonlinearity* γ*, integration time offset* t_{ofs}*) has been improved and is now preceding the noise section (which originally included P9869, 26-27).*

- **P9870, 20:** The dependence of the dark signal on temperature should be discussed.

  → *After the restructuring of the manuscript, this paragraph is now directly followed by the discussion of the dependence of the dark signal on temperature.*

- **P9870, 22:** Shouldn't both cameras act different? The SWIR is temperature stabilized, the VNIR not.

  → *That is correct. The sensor temperatures themselves should clearly be very different. However, we used the temperature sensor inside the VNIR casing as a proxy to characterize the ambient temperature condition.*

- **P9871, 3:** For the proposed ground-based measurements higher temperature changes are expected. How to deal with it?

→ *That is a good question, since we are still in the process of finding out the best dark frame measurement strategy for ground-based measurements. Since the dark signal behavior for large temperature swings has not been thoroughly investigated up to now, we included the following item in our outlook section:*

> *(...) The dark signal behavior for very large temperature swings has not been thoroughly investigated. Frequent dark frame measurements and the avoidance of direct sunlight onto the instrument are therefore essential during outside ground-based measurements. (...)*

- **P9871, 4:** What is DN?

  → *We now explain the abbreviation one subsection earlier:*

  P9867, Line 25:

  > *(...) Each pixel outputs the measured signal as a digital number (DN).*

- **P9871, 13:** Why in the equation a * is used and in Eq. 8 and 9 not. I'm not sure if here one redundant quantity is introduced. How Eq. 7 and 10 can be connected? I don't see it at the first view. You may add R here somehow in Eq. 7 instead of the $\propto$. And when it is correct write $S_0^* \neq S_0$.

  → *The * has been used to differentiate between the (less correct) idealized signal as measured by a linear sensor and the (more correct) signal including nonlinearity. However this has nothing to do with the * in the section before and therefore might be quite missleading. To remove this confusion, S_0^* has been changed to $\tilde{S}_0$ and a note has been added differentiating both symbols. As also suggested, R has been added to the equation which further* *clarifies the next point and the relation between the above mentioned Eq. 7 and 10.*

- **P9871, 14:** You may add $s_\mathrm{n} = s_\mathrm{n}(L)$.

  → *Good point. This actually turns out to be* $s_\mathrm{n} = RL$ *and has been added to the updated version. We further refined that* s_n *should be independent from camera settings (which can be quite variable) but not from the camera parameters (as stated in the previous version and which might include the sensor response* R*).*

- **P9871, 15 and 21:** You draw conclusions from measurements which are not shown. Merging Sec. 4 and 5 may help to avoid this.

  → *That is correct. After the reorganisation of the manuscript structure we added an additional plot which directly shows the deviation from the linear model*

  P9871, Line 17:

  > *(...) Figure 7 and 8 show the found deviations of the VNIR and SWIR from the idealized linear model (Eq. 7). Here, the photoelectric signal* S_0 *of the same stabilized light source (LIS) should become invariant after normalization with the set integration time* t_set*. The fit of the original VNIR signal* s_n *(grey line, Fig. 7) seems to show a photo response non-linearity, which is visible at higher signal levels by lower DN for* $t_\mathrm{set} = 12.0\,\mathrm{ms}$ *compared with* $t_\mathrm{set} = 2\,\mathrm{ms}$*. By contrast, the fit of the original SWIR signal* s_n *(grey line, Fig. 8) is almost linear but seems to be insufficiently normalized when using the set integration time* t_set*. (...)*

- **P9875, 1:** Are bad pixels randomly distributed or e.g., for one wavelength 265 spatial pixel are bad.

  → *Indeed and yes, they are randomly distributed. A note has been added.*

- **P9875, 13:** Can you first show measurements. Otherwise the reader does not know how the LSF and SRF looks.

  → *We now show Fig. 16 (former Fig. 8) at the beginning of the subsection "Response function" and added following introduction:*

  P9875, 13:

  > *Fig. 16 shows a measured line spread function of the VNIR spectrometer and a spectral response function of the SWIR spectrometer.*

- **P9875, Eq. 14:** How Δx is practically derived? Is the fit systematically changed to force the ratio to be 0.7610?

  → *The fit is left untouched in the derivation of Δx. Instead, the parameter Δx is found by optimizing the symmetric integration limits $\Delta x/2$ to satisfy the ratio 0.7610 in Eq. 24 (former Eq. 14). Hereby, the integration limits are centered around the median x_c of the response function. Therefore, the process is twofold: first, a third order B-spline fit F is found and used to obtain the median x_c. To obtain the FWHM, the area under F (with symmetric limits $\Delta x/2$ around x_c) is optimized to be equal to the FWHM area under a Gaussian function.*

  P9875, Line 15ff now reads:

  > *For this reason, the process to retrieve the center and the resolution respectively bandwidth of the response functions is twofold: First, a*

> *third order B-spline F is fitted to the measurements to determined the center of a response function as the median x_c of F. Then, the resolution Δx is centered around x_c and determined by the area under the normalized spline fit F, which is equal to the area (0.7610) under a Gaussian function $G(x)$ between its full width half maximum* FWHM*. This way a measure of the response function width is provided in analogy to the full width half maximum of a Gaussian shaped function. Consequently, the resolution is derived by optimizing the symmetric integration limits $\Delta x/2$ to satisfy Eq. 24:*

- **P9876, 1:** How the data was measured?

  → *The corresponding sentence is no longer in the revised manuscript. The paragraph now ends with*

  P9876, Line 1ff now reads:

  > *The basic idea to transfer the FWHM concept to asymmetric response functions is also illustrated by the inset in Fig. 16a. Using this technique the angular resolution $\Delta\theta$ and the spectral bandwidth $\Delta\lambda$ are determined.*

  The actual measurements to determine the LSFs and SRFs are then described in the following two subsections (4.2.4 and 4.3.5) in detail.

- **P9876, 1:** Always try first to introduce figures and then explain, discuss the plot.

  → *Section 4.2.3 now introduces the response function section with Fig. 16 (former Fig. 8) and the following sentence: P9876, Line 1ff now reads:*

  > *Fig. 16 shows a measured line spread function of the VNIR spectrometer and a spectral response function of the SWIR spectrometer.*

- **P9876, 4-7:** One of many repetitions. Try to shorten. "already defined" signals that it's already done. No need to repeat.

  → *P9876, Line 4-7 now are shorter and read:*

  > *Every pixel of the sensor arrays has its own set of LSFs, which are described by the viewing angle* θ_C *and the angular resolution* $\Delta\theta$*.*

- **P9877, 15-16:** Again repetition.

  → *The corresponding sentence was removed in the revised manuscript.*

- **P9877, 25:** A sketch or image may help to understand the setup.

  → *The spectral calibration setup was identical with Gege et al. (2009). Therefore the interested reader is now referred to this publication where the setup is described in detail:*

  > *A detailed sketch of the calibration setup can be found in Gege et al. (2009) in Fig. 7.*

- **P9878, 13-18:** Wasn't that already stated in Section 4.2.3?

  → *Correct. In the revised version we removed this paragraph.*

- **P9870, Figure 9:** Aren't top and bottom panel the same? Just once logarithmic and once linear scale? Why you have to show both? The parameters main parameters and characteristics can be read from both.

  → *This is correct - the underlying data is the same, once logarithmic and once linear scale. However, we want to show the different noise characteristic for low as well as for high signal levels. In our opinion, the logarithmic scale* *better illustrates the read-out noise, while the linear scale better shows the nonlinear behavior of the VNIR noise.*

- **P9880, 17:** You refer to results of an upcoming section. This is not a good way and makes the reader to think about jump to Sec. 5. Shifting the noise analysis might help to avoid that.

  → *After the restructuring of the revised manuscript, the noise analysis now follows the former Sec. 5*

- **P9880, 20:** A short comment why the maximum dynamic range is not reached would help. Dark current signal for sure?

  → *Yes and true, it has been changed to:*

  P9880, Line 20:

  > *Between 0–12 000* DN, *which is only limited by the subtracted dark signal, . . .*

- **P9881, Sec. 5.1.2:** I was missing a discussion on the importance of the dark signal compared to dynamic range and the radiometric calibration. Is it possible to translate dark noise into radiance. The question is not how the dark signal behaves in general. As you do sequently measure it in field. the question is how good the dark signal is measured. Uncertainties in dark current migrate into the radiances. And this depends also on the ratio between signal and dark-signal-uncertainty. This will for example change for different spectral regions.

  → *We intentionally decided against a translation of dark noise into radiance, as this is very dependent on sensor settings including integration time. We do not use a fixed or typical integration time but an automatic exposure system. Accordingly, an exemplary noise equivalent radiance would not be* *very representative. In our opinion, the explicit consideration of dark signal uncertainty is more insightful as it considers the actual measurement setup (integration time and typical illumination condition). Moreover, the question of how good a dark signal is measured might not capture the entire problem, since the dark signal uncertainty is actually dominated by the dark signal drift and not the dark signal noise. For this reason, the "online" dark signal uncertainty is now included into the overall radiometric uncertainty as shown in several figures and in the short discussion of the error contributions in the conclusion section. We also added a description of how we consider the dark signal drift together with the dark signal noise in the overall error calculation. This should provide the reader with a tool to estimate the noise influence depending on the illumination condition in his application.*

- **P9881, 8:** I'm surprised that it is not the other way around: SWIR is cooled to have a stable sensor temperature. Why dark-current should react on temperature? That can only happen when the cooling system does not cool sufficiently. VNIR is not cooled. So the sensor should somehow react to temperature changes. That's what was written in 4.1.2 "originates from thermally generated electrons and holes within the semiconductor". Can you explain this behavior of the VNIR sensor?

  → *As already mentioned in the original manuscript (P9881, L25), we found out, that the SWIR cooling system does not cool sufficiently to make the dark-current independent from temperature. The analysis of the VNIR dark signal in Fig. 6 shows a very small but measurable dependence on temperature.*

- **P9881, 23:** How do you explain the independence of the dark signal to tint? Is there an internal dark current correction already applied in the camera?

  → *The internal dark current correction of the VNIR was already disabled by the* *vendor SPECIM. We also inquired this with the manufacturer of the sensor unit, who ruled out the existence of further correction techniques. As already mentioned in the preceding answer, there is a very small dependence on temperature visible. Apparently, the read-out noise level is much higher than the dark current signal level for this sensor.*

- **P9881, Figure 10:** What about the temperature range expected for ground-based operation? This is not covered here.

  → *That is correct. As already mentioned in a preceding answer we included an item for this in our outlook paragraph in the outlook section.*

- **P9882, 5:** First introduce the figure, then discuss.

  → *Now, its the other way round.*

- **P9882, 6-8:** That's obvious. I suggest to remove the sentence.

  → *We agree - the sentence is removed in the revised manuscript.*

- **P9882, 15:** There is no figure showing the radiometric response and the fit? Would be helpful for the reader to see the non-linearity.

  → *Thanks for this suggestion. As already mentioned in a preceding answer, we now show and discuss figures with the original radiometric responses, the fits and the corrected signals in the revised manuscript (Figure 7 and 8).*

- **P9884, 27:** Discuss what polarized radiation can be expected in atmospheric applications and how large the error will be in different cases. E.g. remote sensing of clouds? Aerosol from ground-base measurements where Rayleigh scattering of the sky may contribute polarized radiation?

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→ *You are correct - this is indeed a very interesting discussion. In our revised manuscript we now have derived an equation (Eq. 20) which can be used to estimate the radiometric error associated with an assumed light polarization* p*. This equation is now also used during the overall radiometric error budget in the ACRIDICON example.*

At the mentioned line, we added following paragraph:

P9884, Line 27:

> *In the field, radiation is never fully polarized. The polarization of sunlight reflected by water clouds is well below 5 % for most viewing geometries. It only reaches values of up to 15 % in the rainbow region of optically very thin clouds (Hansen, 1971). In contrast, Rayleigh scattering can be strongly polarized, depending on the scattering angle. If strongly polarized light must be assumed, the calibrated radiance has to be handled with care and provided with corresponding uncertainty estimates following Eq. 20. For sensor regions with a small polarization sensitivity* P*, the radiometric error scales linearly with the light polarization* p.

- **P9886, 5-7:** Repetition.

  → *We removed these lines in the revised manuscript.*

- **P9888, 5-6:** That's an obvious procedure. Has not to be mentioned.

  → *We removed this sentence in the revised manuscript.*

- **P9888, 7-11:** This information is not needed.

  → *We removed these lines in the revised manuscript.*

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- **P9888, 24:** Give the value of refractive index.

  → *We have added the range of the real refractive index from the data sheet.*

- **P9889, 4:** Why not calculating absorption? You have the refractive index and can use Lambert-Beers law.

  → *The complex refractive index is not given in the datasheet, however the manufacturer sent us a better specification of the window transmission which has been added to the plot.*

- **P9891, point 3.:** I would conclude different. Wasn't stated, that radiometric uncertainty is about 3% in the best wavelength? If the radiometric calibration changed about 10% indicated by the difference in manufacturer and own calibration this suggest that the calibration has to be repeated over time. Otherwise you can not claim to have an accuracy of 3%. It would be rather 10% due to temporal changes.

  → *You made a valid point here! We changed our conclusion for this point by including your recommendation.*
  P9891, point 3:

  > *The radiometric response R given from the manufacturer does not differ by more than 10 % from R found in this work. Although R seems to be quite stable, the calibration should be repeated over time since the radiometric uncertainty is about 3% in the best wavelength region.*

- **P9892, 5:** The overall accuracy is not but should be given in the conclusions.

  → *This was already covered by the answer to the major issue about accuracy requirements.*

- **P9892, 16-19:** Repetition.

  → *We removed this sentence in the revised manuscript.*

- **P9909, Figure 4:** Give a scale or dimensions.

  → *We added following clarification:*

  > *The baffles have a length of 160* mm *and a diagonal of 125* mm.

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Please also note the supplement to this comment:
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Interactive comment on Atmos. Meas. Tech. Discuss., 8, 9853, 2015.