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Atmospheric Measurement Techniques Discussions

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Interactive comment

Interactive comment on "A highly miniaturized satellite payload based on a spatial heterodyne spectrometer for atmospheric temperature measurements in the mesosphere and lower thermosphere" *by* Martin Kaufmann et al.

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Reply to the reviewers' comments: Reviewer #1

General

We thank the reviewer for carefully reading the manuscript and his/ her constructive and helpful comments and suggestions. They helped us to improve the paper in several aspects. Before we consider them point by point, we like to make the following general remarks:

- The section about the instrument characterization was criticized by the reviewers and we restructured it.
- Further analysis of the spectral power estimation using subspace methods revealed some problems when dealing with interferograms with finite spectral resolution. For the figures shown in the first version of the manscript, infinite spectral resolution was assumed, which is not realistic. To handle finite spectral resultion with subspace methods, the number of spectral components has to be increased, but to develop concepts for further analysis of this kind of data is ongoing work not in the shape to be presented here. Therefore, all analyses and performance assessment in the second version of the manscript is based on conventional Fourier transformations.
- We corrected the value for the etendue of the instrument, which referred to the full circular aperture, but we use an inner rectangular for the later analysis, only. This reduces the orginal value of 0.014 cm^2 sr by about 1/3.
- some numbers (like the image size or spectral resolution) differed slightly across the document and were harmonized

Point by point response

1.*: Abstract: "with a solid angle of 0.65 degrees" The solid angle unit is "steradian", not degr*

Reply: agreed, we changed solid angle to acceptance angle

__2.__: Caption of Figure 2: "'Barth' indicates the number of molecules created by the recombination of atomic oxygen." Judging from the axis label and the rest of the description, I assume that 'Barth' indicates the Volume Emission Rate of the airglow component that is resulting from the Barth mechanism, not a number of molecules. Same comment applies to the A-band and B-band description in this caption.

Reply: agreed and corrected.

__3.__: Figure 2: This figure could be improved by (a) omitting the number "2" on the left of the y-axis, (b) adding a "K" after the temperature labels (200, 210) on the top right, and (c) omitting the integral sign and 1 3 0 1 on the top right. It is not clear to me what the latter means.

Reply: agreed, this was a pdf problem in the final document, corrected

__4.__: Page 6, line 2+: "The zero frequency of the fringe pattern is at the Littrow wavelength and the spectral peaks of the neighboring wavelengths are spread or heterodyned around this central wavelength." This sentence is not quite clear to me. What is meant with "spectral peaks"? Instead, it might be worth pointing out that the heterodyning effect results in the fact that high spectral resolution can be obtained because small wavenumber changes result in fringes with discernable spatial frequency, which can be observed with available imaging detectors.

Reply: agreed, we changed the wording as suggested.

__5.__: Page 6, line 5+: For completeness, the authors might consider adding a reference to the first satellite borne SHS instrument: Englert, C. R., M. H. Stevens, D. E. Siskind, J. M. Harlander, and F. L. Roesler (2010), Spatial Heterodyne Imager for Mesospheric Radicals on STPSat-1, J. Geophys. Res., 115, D20306, doi:10.1029/2010JD014398.

Reply: added

6.: *Page 6, line 13: This equation is incorrect. The right side is missing the grating groove density (see Harlander et al., ApJ, 1992, Equation (1))*

Reply: agreed, the equation was corrected.

7.: *Page 7, line 1: It might be worth adding that "position x" is in the direction parallelto the dispersion plane.*

Reply: agreed, 'parallel to the dispersion plane' was added.

8.: *Page 7, line 9: It is not quite clear to me why the authors say that the spectral resolution is limited by the detector resolution (pixels per length?). I do agree that the Nyquist theorem limits the bandpass, as the authors state.*

Reply: agreed. Our statement was imprecise. We meant, that the choice of the grating groove number in combination with the detector pixel number determines (and limits) the spectral resolution and the bandpass. Not to confuse the reader, we changed the wording to 'The bandpass of an SHS is limited by the detector resolution by the Nyquist theorem'.

9.: *Page 7, line 17: For completeness the authors might consider adding to this sentence: ". . . by Hilliard and Shepherd (1966) with a Michelson interferometer, and first introduced for SHS by Roesler and Harlander (1990)." The reference is: "Roesler and Harlander, Spatial heterodyne spectroscopy: interferometric performance at any wavelength without scanning, Proc. SPIE 1318, 1990, doi: 10.1117/12.22119."*

Reply: agreed, reference was added

10.: *Table 1: giving the clear aperture in PI times radius squared is a little confusing. I recommend listing the aperture diameter or radius.*

Reply: agreed, diameter is now given

11.: *The authors might consider giving the field of view dimensions in both directions, so that a reader can verify that the etendue is the product of the field of view solid angle and the aperture area.*

Reply: Since we have a circular aperture, the etendue is simply given by the solid angle of the spherical cap and the aperture area. We prefer to keep the numerical value instead of giving a formula.

12.: *Figure 4: The top axis suggests that the detector is 0.8 cm wide, but using a pixel pitch of 5.04 microns and 840 pixels per row only results in a width of about 0.4 cm. Please check.*

Reply: agreed, the figure was updated in two aspects: The localization plane scale was corrected and nighttime data is shown.

13.: *Figure 4: The caption states: "Note: I will update the figure later on". Please provide the correct figure (in case this is not the correct one), and please include what local time this simulation was made for, since day and night profiles are significantly different.*

Reply: agreed, we added 'nighttime' in the figure caption and updated the data shown

14.: *Page 9, line 14+: Depending on the field of view orientation with respect to the satellite velocity direction, the scene is scanned through the field of view, which, for a 60 second exposure, can be significant. It might be worth pointing that out.*

Reply: agreed, we added 'or smeared out during the exposure of the image'

15.: *Page 9, line 18: Please add that the 66mm are the diameter, since some dimensions are given as radius and some as diameter throughout the paper.*

Reply: done

__16.__: Figure 5: The first sentence of the captions claims that the figure includes the filter, the second sentence says that it is not shown. Please clarify.

Reply: agreed, the figure was updated and shows the filter now

__17.__: Page 11, line 11: It might be worth mentioning the Modulation Transfer Function of the detector optics here, since it will potentially influence the modulation of the higher frequency fringes.

Reply: Agreed, see also comment of referee 2 on the temperature dependence of the MTF. From our point of view, the MTF is a good *qualitative* indicator for the optical performance of the system, but it cannot be used to *quantify* the visibility reduction due to aberrations, out-of-focus configuration, etc.. To comment on the temperature dependence of the entire optical setup, we give some remarks on the MTF and not on the interferogram contrast, because the simulation is very time consuming and not available for the publication timeframe of this work. We added the following text in the manuscript: 'The SHS has a fairly well athermal design, but the foci and the modulation transfer function (MTF) of the entire optical system depend more on temperature. For low spatial frequencies, this effect is small, but for the highest spatial frequencies seen by the instrument, the MTF reduces from about 85% at 20^{o}C to about 70% at 0^{o}C. Further simulations and comparison with measurements are in preparation.'

__18.__: Page 11, line 16: The authors state: ". . . 80 photons/s at every spectral point within the interferogram." I think what they mean is ". . . 80 photons/s at every pixel recording the interferogram." The interferogram is in "spatial" space, not in "spectral" space.

Reply: agreed, corrected

__19.__: Page 12, line 9: The authors state: "Although the dark current at a detector temperature of 20C is a factor of 2.4 larger than the expected atmospheric signal in the nightglow maximum, it does not deteriorate the data processing significantly." This is *not clear to me. An additional signal that is 2.4 times larger than the shot noise limited, targeted signal, will increase the noise by a factor of sqrt(3.4), or 80 percent for every interferogram point. I would not call that insignificant.*

Reply: agreed, our wording was not correct and referred to the signal of a single emission line. We give the expected photon flux per pixel earlier in the chapter (40 ph/s, not given explicitely in the previous version) and changed the statement related to the significance of the dark current in the following way: 'At 20°C, the dark current is at least a factor of 5 lower than the atmospheric signal in the emission layer maximum and therefore not a dominant source of random noise at these altitudes. This becomes more critical at other altitudes and for higher detector temperatures.'

20.*: Page 13, line 14+: It is not clear to me why the first step is required, since all non-uniformities are covered by performing the second step, including the detector non-uniformities.*

Reply: agreed, we removed the first step from the text. Nevertheless we performed this step, mainly to select a 'good' detector from a batch of detectors.

21.*: Page 13, lines 24: Do the authors mean: "Due to the highly compact design of the *front* optics and the use of spherical lenses only. . .", since assessing the effects of the interferometer and detector optics are discussed in the following paragraph?*

Reply: We expect to see image distortions mainly from the detector optics, although we also want to quantify distortions introduced by the entire system, which requires a test image to be positioned in front of the front optics. The interferograms can be used to quantify image distortions of the camera optics, but this gives information in the interferogram-dimension, only. The corresponding distortions are likely the same in the other dimension as well, but the test image is a good way to verify this assumption. We re-ordered the entire chapter on 'Instrument Characterization' and hope that this point becomes clearer now.

22.: *Page 14, line 9: If I understand this method correctly, it aims to determine the same fringe phases for each rising and falling fringe edge by finding a constant intensity level. This works, if the fringes have a constant offset (non-modulated part), flatfielding has been performed and a correction for modulation efficiency has been performed, prior to finding these edges. If this is what was done here, please include these caveats.*

Reply: We will use an adaptive edge detection algorithm, which will circumvent the points you mentioned. This was misleading in the first version of this manucscript and we added 'adaptive edge detection' to make this point clear. We also like to mention in this context, that we changed the experimental setup to perform these measurements from an integrating sphere to a homogeneized laser beam. The reason for this modification is the difficulty to project the light of the sphere (with its curved walls) into infinity. We therefore will use microlens arrays to homogeneize the laser light. We changed the text in the following way: To characterize and quantify the modulated part of the intensity, an optical setup with a tunable laser is used. First, the laser light is homogeneized using microlens arrays and imaged onto a rotating diffusor. The laser spot on the diffusor is set to infinity by a large lens, such that the full aperture of the instrument is uniformly illuminated by plane waves with a divergence of at least $\pm 0.65^o$. The laser frequency and power are continuously monitored during the measurement. The laser power and the flux are calibrated before the measurements are taken.'

23.: *Page 14, line 18+: I suggest reworking the following for clarity from: "total power in each wave" to "total power for each spectral element".*

Reply: agreed, changed accordingly

24.: *Page 14, line 20: "known good enough" should be "known well enough"*

Reply: changed

25.: *Page 14, line 21: Using a Hilbert transformation to determine the envelope of the modulated part of the interferogram is fundamentally the same idea as the methods* *described in Englert et al. 2004 & 2006, where the corresponding complex/imaginary interferogram is generated from the real interferogram. It might be worth pointing that out. In addition, it is not quite clear to me why the authors do not use the fringe phase that can easily be determined using the Hilbert transform to determine the phase distortion. Since it does not rely on the above caveats, it appears to be a more resilient method than using the constant intensity level to find constant phase positions, as described immediately above this section.*

Reply: we added the reference and agree on this comment. The interference pattern is also used to verify and to correct the image distortion orthogonal to the interferogram direction in-orbit, if needed.

***26.**: Page 14, line 21: The reference Liu et al. 25 (2017) is not yet published as of the submission of this manuscript and could not be accessed by the reviewer. Please, at least, include the final citation.*

Reply: This manuscript is still in preparation. Due to some problems with the interpretation of that data, we omitted this part

***27.**: Section 7: Please mention that this method explicitly requires a-priori information. It would be beneficial if you could comment on whether this is similar to fitting the line strengths of the known lines to the spectrum obtained with an FFT. The FFT does not destroy information, so all additional information has to be from a-priori knowledge.*

Reply: see above, we removed this part

***28.**: If the authors have a 3D design image of the instrument design, it would benefit the paper to include it, rather than just stating that there is a design that fits into 3.5 liters. (optional)*

Reply: A design image is now included in the conclusion section

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Discussion paper

29.: *From Figure 4, I assume that the instrument will observe the limb between tangent point altitudes of 60km and 120km. Can you please comment on the case in which the airglow extends above 120km during the day? Presumably, the temperature retrieval for at least the highest altitudes will be affected.*

Reply: This is true and a general retrieval 'problem'. In existing retrievals, the regularization parameters of a constrained retrieval setup are chosen in such a way, that the information obtained from the upper most measurement altitude(s) is spread over a broad altitude regime, resulting in a very broad vertical resolution of the retrieved quantities. The corresponding temperatures are not very useful for further analyses, but a smooth transition into some a priori data is assured by this method. We feel that this information is difficult to place in this manuscript and prefer to give a more detailed discussion on the retrieval in a separate manuscript

30.: *Please comment on any thermal effects that are likely to be encountered on orbit, including thermoelastic distortion of the optics, which can affect the focus of the fringes and therefore the modulation transfer function, depending on fringe frequency (larger effect on high frequency fringes), which might have a significant effect on the relative line strength determination.*

Reply: Agreed. We calculated the MTF and added the following text: 'The SHS has a fairly well athermal design, but the foci and the modulation transfer function (MTF) of the entire optical system depend more on temperature. For low spatial frequencies, this effect is small, but for the highest spatial frequencies seen by the instrument, the MTF reduces from about 85% at 20^{o}C to about 70% at 0^{o}C. Further simulations and comparison with measurements are in preparation.'

31.: *It is not clear to me how a 3.5 liter instrument will fit into a 3U CubeSat. Are the authors thinking of further miniaturization?*

Reply: The optical instrument itself fits into about 1.5 litres. Deiml et al. [2014] made a concept study of an extendable baffle to fit the entire instrument into a 3-unit Cube-Sat. We added the following text in the manuscript: 'The utilization of an extendable baffle and some minor design modifications allows to fly the instrument on a three–unit CubeSat.' One design modifications is to decrease the length of the detector optics by a few millimetres by using some aspheres instead of spherical lenses. We made a corresponding design, but it was not persued for budget reasons. For a CubeSat mission we favour a 6-unit spacecraft to relax the compactness of the entire instrument, to avoid the risk of an extendable baffle, to allow for more power, and a few other reasons.

M. Deiml, M. Kaufmann, P. Knieling, F. Olschewski, P. Toumpas, M. Langer, M. Ern, R. Koppmann, and M. Riese, "Dissect: development of a small satellite for climate research," Proceedings of the 65th International Astronautical Congress, Toronto, Canada, no. IAC-14,B5,1,10,x22911, 2014.