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Atmospheric Measurement Techniques Discussions

Interactive comment on "The Aerosol Limb Imager: acousto-optic imaging of limb scattered sunlight for stratospheric aerosol profiling" *by* B. J. Elash et al.

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The paper describes a new remote sensing instrument based on AOTF technology which has obviously been the subject of intense research and development. The originality of the work cannot be questionned and the content of the paper forms a complete report on a nice experimental achievement. The manuscript is well structured and tackles every aspects from the justification of the instrument to the discussion of the results of an ambitious test campaign.

As an AOTF enthusiast relying on some experience with AOTF-based hyperspectral imaging instruments, I would like to contribute to the quality of this paper by raising

some comments and providing some suggestions. I could notice from the referee's review that all my typo remarks had already been addressed, hence I will only discuss the more important points.

1) I am not sure that the introduction section should be so long. To me, regarding the scope of the paper, the not-so-concise discussion on previous limb missions having measured stratospheric aerosols is not helping in appreciating the work done here. Shortening this part could free some space for some missing information in the calibration section or for the error analysis.

2) p. 13290, l. 23-24: the width of the spectral transmission function of an AOTF is something which is frozen at the manufacturing step, i.e. when the crystal cutting angles are frozen. In that sense, there is no such thing like typical bandwidths, as one can design an AOTF with a 5-10 times narrower or broader bandpass.

3) p. 13291, l. 24: The acoustic wave in this kind of device is not a standing wave. In TeO_2 AOTF, it is a shear wave mostly absorbed at the opposite end of the crystal.

4) AOTF design. There is an extensive discussion on the selection of the most appropriate optical design which is well argued. However, I wonder why the rear facet of the AOTF was cut such as depicted in fig.2 (by the way, replace "standing RF" by "acoustic" in this figure). From the moment it is decided to work with the e-light as input, a better configuration could have been found where the diffracted beam remains parallel to the incident axis and a larger angular separation is achieved with the 0-order. This is important because with a half-FOV of 3°, and taking into account your drawing and the fact that the diffracted beam leaves the crystal with an angle of 2.7°, there should be a significant overlapping of the 0th and 1st orders... Could you better justify this design choice in the text?

5) I think the section 2.2 should contain a proper mathematical description of the radiometric model of the instrument, including the spectral transmission function, the polarization sensitivity and other effects such as PRNU. This would certainly help in

understanting the impact of the calibration uncertainties when discussing the error budget.

6) p. 13299, l. 14-15: I would not say that 1.2nm is much less than the AOTF spectral resolution. You indeed performed a characterization of the spectral transmission function of the AOTF with a not-exactly monochromatic light source. In the end you got a result (fig.6a) which is the convolution of the incident light spectrum and the AOTF response. The typical sidelobes are not completely resolved, but this is not really an issue for your calibration as the results seem perfectly in line with standard AOTF performance. I would recommend next time to work with sharp emission lines or laser lines at some selected wavelengths, and rely on the physics of acousto-optic interaction to extrapolate the AOTF response function between the calibration points.

7) Section 3.1: Why didn't you use a physical model to fit the experimental data with the AOTF tuning curve? This would provide a better understanding of the overall instrument. Also, as the F(lambda) relationship is dependent on the crystal temperature, it would be usefull to compare the temperature in the lab when the calibration of the instrument was done with the temperature of the crystal during the flight. Again, a physical model of the AOTF would help in extrapolating the calibration to other working temperatures. The reported 0.1% error in the fit can yield an uncertainty as high as 1nm. A 10°C shift of temperature would also yield a 1nm drift. Is this still tolerable for your measurements? More details on the precision of the wavelength selection would be appreciated.

8) Section 3.2: It is mentioned that a diffraction efficiency of 54-64% is observed, but nothing is said concerning the power applied to the transducer of the crystal. Also, these values appear quite low compared to typical DE for TeO_2 easily reaching 90%. Moreover the method described neglects the attenuation by the crystal itself, and it is not clear if the incident light was initially polarized. I would recommend to re-write this section such that one can better understand how these values could be obtained.

9) Stray light: Cycling between the ON/OFF state of the AOTF is probably a unique feature of the AOTF which is well emphasized in the text. However, knowing how complex straylight characterization can be, I wonder why all these efforts were made as in the end, the problem is mostly solved by the ON/OFF approach. The only effect which is not solved by the ON/OFF method is the straylight generated after the AOTF. Is there something that can be said on this based on the characterization that was performed?

10) Relative flat fielding: It is not clear which setup was used to create the radiometric flat field, and if the complete FOV was illuminated. From what can be read, I understand that only sub-sections of the detector were illuminated, so it is not clear how the response of pixels looking at the bottom of the scene can be related to the response of the pixels looking above... This is important as you perform a spatial normalization in the processing algorithm. A mathematical radiometric model would help understanding what has been done. I would suggest to re-write this section in order to explain how the setup looked like, with which accuracy for the flatness of the radiometric field, and how does it impact the final product.

11) Conclusion: Taking into account the impressive amount of work that has been done in this work, I would have expected some more discussion in the concluding section. From what can be read in this section, further improvements of the instrument would only consist in reaching absolute radiometric calibration, and a better flat fielding. I am not convinced that this will significantly reduce the error bars (50% at 1 sigma). Actually the shortness of the conclusion reflects the absence of a detailed error budget. This is probably my main concern with the manuscript: due to the absence of a mathematical model of the instrument, it is not possible to understand the amplitude of the different errors, and the results presented in fig.12 cannot really be interpreted.

Interactive comment on Atmos. Meas. Tech. Discuss., 8, 13285, 2015.