Atmospheric Measurement Techniques Discussions

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Comment on amt-2021-89

Anonymous Referee #2

Referee comment on "Boundary layer water vapour statistics from high-spatial-resolution spaceborne imaging spectroscopy" by Mark T. Richardson et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2021-89-RC2, 2021

The manuscript presents new retrieval statistics for planetary boundary layer (PBL) water vapor from high-spatial resolution spaceborne imaging spectroscopy. The authors focus on a sensitivity analysis based on a coupled forward and inverse modeling in the frame of the Earth Surface Mineral Dust Source Investigation (EMIT) mission. They analyze uncertainties introduced by instrument errors, surface type, and varying solar zenith angle (SZA), and assess the overall potential of upcoming spaceborne high-resolution VSWIR instruments.

The study is really interesting, novel and well written. Especially the presentation of the statistical evaluation is of high quality. If proven robust also for real data, the concept will be of great interest for the atmospheric science community. However, I have a few comments, which follow below.

General comments

- The introduction could benefit from a clearer structure. While the two closing paragraphs including the four research questions are distinct and coherent, the remaining part could be more explicitly separated into literature research describing previous work and theoretical concepts on the one hand, and presenting the novelty and methodologies applied in this study on the other. For instance, lines 7-11 could be moved to the final paragraphs of the introduction combined with a little rephrasing.
- The discussion part of Section 3 could be improved by a more detailed comparison with retrieval results from already existing instruments such as MERIS. You are listing several instruments for TCWV retrievals in the introduction and the reader could get a

better impression of the retrieval performance from synthetic EMIT data in case some reference values from other datasets are given.

- Overall, the manuscript would strongly benefit from an application of the presented methodology to "real" data. You basically agree with this on page 12, lines 26-28, with the statement "Limitations include the use of the same radiative transfer code for forward and inverse simulations…". For instance, the Italian PRISMA instrument already delivers high-spatial resolution imaging spectroscopy data and could be used for PBL TCWV retrievals in the same manner as EMIT. On the other hand, if this study is intended to serve as a pure sensitivity analysis, this should be clearly mentioned in the introduction.

Specific comments

Page 1, lines 24-27: Could you provide a little bit more context why the knowledge about vertical moisture structure of the atmosphere is crucial for weather and climate applications, and why thermodynamic information is a targeted observable recommended by NASA's Decadal Survey?

Page 2, line 14: "However, similar capacity is anticipated…". I would go beyond and replace "similar" with "improved" since SBG and CHIME will most likely be offering even higher spatial-resolution than EMIT.

Page 3, lines 12-14: The reader could get the impression that the AVIRIS-NG flights were selected for this study. Please try to rephrase and clarify.

Page 3, line 18: Although it is explained later on, it would be good to have a short definition of the "true TCWV" here, e.g., "…, which was used as input for our forward simulations…".

Page 6, line 8: Please define the quantity ρ_s.

Page 6, line 10: Don't you miss to list the spherical sky albedo here when mentioning the flux calculations coming from MODTRAN?

Page 6, line 11: I think it would be better to say "spectral response function (SRF)" instead of "line shape (ILS)". This might be more common in the remote sensing

community.

Page 6, lines 13-15: You could add references to Rothman et al. (2009) for HITRAN and to Stamnes et al. (1988) for DISORT here.

Page 6, lines 14-15: Is the number of DISORT streams of importance for your application? Either remove it or explain why you used 8 streams.

Page 6, lines 24-25: You define the used reflectance quantity as the hemispheric-directional distribution function on page 7. However, it would be good to have the definition here, directly after introducing Eq. (1).

Page 7, lines 4-6: Did you normalize the surface prior distribution to avoid constraints on the reflectance magnitude as described in Thompson et al. (2018)?

Page 7, line 16: Which performance do you mean here? Give quantities.

Page 8, lines 1-2: What about other types of surfaces such as artificial surfaces or snow? Do you plan to extend your analysis to those types as part of future work? If yes, this could be mentioned in the discussion/conclusion of your results.

Page 10, line 8: What does "retrieved well" mean? Give quantities.

Page 10, line 12: Which prior mean and covariance did you use for the TCWV state vector parameter in your ISOFIT setup? And did you use the default first guess estimation based on a heuristic band ratio retrieval? It would be good to provide this information earlier in Section 3.1.1 and to discuss it in a few words as it can influence your retrieval results.

Technical corrections

Page 2, line 10: Although "LES" is defined in the abstract, it would be nice to have the full expression here again.

Page 2, line 26: Rephrase to "…via two demonstrated approaches **in order to** provide a single value…".

Page 3, lines 4-5: "More TCWV **leads to increasing depth of H_2O absorption features** relative to other wavelengths."

Page 3, line 10: "**The** retrievals…".

Page 6, line 8: Rephrase to "Conceptually**,** it targets ρ_s and **the estimation of** TCWV is seen as part of an atmospheric correction."

Page 6, line 24: "…**the cosine of** the solar zenith angle,…"

Page 6, line 31: "…**generated** using…"

References

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