

Atmos. Meas. Tech. Discuss., referee comment RC2
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Comment on amt-2021-232

Anonymous Referee #2

Referee comment on "Emissivity retrievals with FORUM's end-to-end simulator: challenges and recommendations" by Maya Ben-Yami et al., Atmos. Meas. Tech. Discuss., <https://doi.org/10.5194/amt-2021-232-RC2>, 2021

I have read the paper "Emissivity Retrievals with FORUM's End-to-end Simulator: Challenges and Recommendations" by Ben-Yami et al. with interest. It is clear that this paper is the result of very careful and extensive work, and I congratulate the authors for this. Most of the results are interesting (but see below) and I also liked the figures. Despite this, I have several general comments that could lead to an improved paper (see below). I leave it up to the authors to decide whether and to what extent these general comments are addressed. They are given merely to help increasing the impact of the paper. Even without addressing these, I think the paper is suitable for publication in AMT after minor revision.

General comments

- (i) Explanations are not always expressed very clearly. Very frequently I found myself reading a paragraph twice or even three times to understand. The paper is also too long-winded, especially near the end. It is also repetitive at times. In all, I think the paper would really benefit from careful copy-editing, to increase its impact and make the paper more pleasurable to read (and to increase the chances that readers actually make it to the end).
- (ii) It is not always clear what the point is of the analysis or whether what is presented is really new. Some of the analysis is more appropriate for a thesis, or a technical report. A paper is not meant to show all results obtained in a study, but should present those results that are new and useful to the community. As an example, consider section 6.3 - did the reader really learn something? From the concluding paragraph (lines 509-515) one would say no. A ruthless shortening of the paper (both with respect to its wordiness and the material presented) would be highly beneficial in my opinion.
- (iii) the study is geared towards FORUM and the specific tools that have been developed, but is this paper going to be useful to anyone else? (especially since it is mentioned several times that the paper is just a step in the development of a future product).
- (iv) Point (2) in the abstract illustrates these three points. It is hard to read, difficult to understand what is meant, and not sure that it is useful to anyone but the developing team of FORUM.

Specific Comments:

Line 2: its FIR contribution > that the FIR is..

Line 10: absolute or relative uncertainty? is this % or absolute?

Line 13: not sure if I agree with the first recommendation (even after reading the paper). Why limit the range artificially with an ad hoc threshold? Why not simply provide the emissivity over the entire range together with the estimated uncertainty (or information content quantifier) and let the user decide what to use?

Line 15: Point (2) is very hard to understand, in particular the "Thus" - how does the second sentence follow from the first (which doesn't even mention uncertainty). An abstract should be understood by itself, without the need for going through the entire paper. If I understand it correctly, what you want to say is that because emissivity and temperature cannot be retrieved independently, a priori knowledge on at least one or both is needed - however errors on e.g. the a priori of the Tskin, might result in unrealistic uncertainty estimates; and a way has to be found to improve these estimates. If this is what is said, it is a rather weak way to close an abstract.

Abstract: overall, the abstract could be much stronger and would highly benefit from copy editing. It would also benefit from a few strong, easy to understand take-home messages.

Introduction: I didn't see this paper cited <https://doi.org/10.1175/BAMS-D-20-0155.1> which could be relevant

Line 25: "never been observed"; not exactly, as there have been aircraft measurements. Perhaps add "from satellite"

Line 34-35: a bit strange to say that it is exciting to measure because only one instrument can measure it. There are many things unique that are very unexciting. The second reason is much more convincing.

Line 95: I would move this to section 3.2, as it is there that the retrieval parameters are discussed.

Line 107: This deserves a reference or justification

Line 115: I would move the start of Section 3 to the end of the introduction

Line 177 spectral region considered > considered spectral region

Line 187: to this end four > to this end, four

Line 192: "The reason of the focus on the water vapour" .. this sentence is very important, but should be mentioned much earlier in the manuscript (in the introduction), because water vapour is mentioned over and over, without saying why it is important.

Figure 3: This is a very nice figure, but here are a couple suggested improvements: (1) Panel a: plot 1 sigma uncertainties as a semi-transparent shaded region, because as-is, the error bars draw all the attention. (2) I would add a 5th panel with the total transmittance (surface to TOA) as a function of wavenumber, since you discuss transmittance many times, without ever showing it. This property is probably available from the forward model without much effort. On this plot you could also indicate the main spectral ranges of absorption due to the different species (O3, CO2, H2O, CH4, ...) As a side comment, the paper emphasizes strongly on H2O but does not mention the other strong absorbers - I think it is a shame not too. This does not need to be long, but you could for instance raise the following question: With current trends in CO2 increases, can we expect that the retrieval range will become more narrow by the time FORUM is operational? (3) For the bottom panel, how about just displaying the diagonal of the averaging kernel? You mention that it is not smooth (line 267), but from the plot this does not seem to be the case for the diagonal. The diagonal will probably look similar to the IQ.

Line 236: "below the CO2" - might be unclear to some readers, but see previous comment on inclusion of extra panel.

Figure 4: since a discrete number of pwv are used, it would be good to use a discrete colorbar with the same number of colors.

Figure 5: From figure 5, and the visible "noise" in the retrieval, it can be argued that the retrieval is not smooth enough on the one hand, but gives too much importance to long range correlations. One way of tuning this is with the correlation length, as the paper does, but the results in Figure 5 suggest that the formula used for the covariance matrix (line 614) is far from optimal. Perhaps replacing Δ/CL with $(\Delta/CL)^n$ would already do it, with $n > 1$ so that the short range correlations become relatively more important than the long). The value of CL would need to be reevaluated. You could optimize by minimizing the RMS for different choices of n and CL (like you do in appendix B).

Figure 5: Please add the value of the RMS to each of the panels

Section 5: The word "extent" is a bit of misnomer, as it doesn't express the width of the interval. I would simply replace it with "lowest wavenumber" everywhere in figure, table and text.

Line 327: Only pwv is mentioned while obviously some other species are equally important (see comment above on the other species).

Caption of Figure 7: please shorten. Only at the end of the caption the reader actually learns what the lines in the main plot represent. No need to give needless details (like the time of the observation) or repeat everything that is in the text, or what should be clear from the legend (like that the dashed line corresponds to the true emissivity).

"same in all four cases": do you mean exactly the same, this is very surprising!
uncertainty > uncertainty.

Figure 7: the legend in the figure (i)-(iv) could be improved, e.g. true +/- 0.5K etc would be clearer if it is written as, True a priori, +/-0.5 K uncertainty.

Line 333-335: this is incorrect in the sense that we have a robust lower bound on the temperature (corresponding to an assumed emissivity of 1, whereas lower temperature would need an emissivity above 1). The statement is correct for temperatures higher than the actual temperature.

Line 337: unclear as you already use MIR? Please also define what you understand under MIR, as there are many definitions.

Section 6.1: I believe that temperature and emissivity will be easier to disentangle once you find a more appropriate covariance matrix per comment above.

Line 362 "the the"

End of section 6.1 + section 6.2: I do not understand the choice of a priori value of 1 for the emissivity. Why not say a constant 0.98 or 0.99? This will in almost all cases be closer to true emissivity, and will therefore: (1) result in faster convergence and (2) result in retrieval results closer to true value (as your a priori value is closer to the truth). It is a free gain.

Figure 8: not a big fan of this figure, as it is very hard to read / interpret. First, the study of initial guess is not that interesting/relevant as there is little reason to take the initial guess different from the a priori. Easier, more comprehensive and more interesting would be a combined analysis of the a priori and constraint. How about a scatter plot with X-axis the a priori (say 0.8-1 in steps of 0.01), Y-axis the a priori uncertainties and then the datapoints colorcoded by RMS (averaged over the 4 cases)?