Atmospheric Measurement Techniques Discussions

Atmos. Meas. Tech. Discuss., author comment AC2
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Reply on RC2

Marco Ridolfi et al.

Author comment on "Synergistic retrieval and complete data fusion methods applied to simulated FORUM and IASI-NG measurements" by Marco Ridolfi et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2022-82-AC2, 2022

Please find below, Author Replies to Joern Ungermann's comments. Original reviewer's comments are included in "italic".

RC2: Comment on amt-2022-82, Joern Ungermann, 28 Jun 2022

GENERAL COMMENTS

The paper compares two different methods of fusing the measurements of the IASI-NG and FORUM satellites to derive atmospheric states. One operating on level 1 data, one operating on level 2 data. It is found that both methods deliver (under reasonable assumptions) equivalent results. Such, the method simpler to implement can be chosen.

The topic fits the journal well.

The paper is concise and well written. I recommend publication after addressing the comments below.

We thank Joern Ungermann for revising the manuscript and for the constructive comments. We decided to change our test experiment setup to address the reviewer's main comment reported here below. With this modification and with the integration of the results of two new test experiments at Mid- and Tropical- latitudes, we believe that now the results presented are more robust and consolidated than before.

MAJOR COMMENTS
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lines 327ff
The test profiles for the perfectly co-located measurements are generated using a (fixed) a priori profile perturbed by random vectors according to the CM 0.5 S_m. This is strange as the matrix S_m was designed to represent the typical differences between two mis-aligned FORUM and IASI-NG profiles, not to represent the typical variability of Antarctic profiles in general in this seasons. I would assume that such neighboring profiles exhibit - on average - only small differences whereas Antarctic profiles exhibit a large variety of shapes.
I would have expected a much larger variety of profiles here based on a different CM matrix. Such more diverse profiles could then be perturbed using 0.5 S_m to generate the

two differing profiles for the second part of the study. Please adapt the study or elaborate why the current choice sufficiently captures the required variability of atmospheric states. I do not expect a different outcome, but the examination of 900 probably very similar profiles seems questionable.

We agree with this comment. Originally, we designed the test experiments as if the atmosphere was "ideally" sounded for 900 times in a very short time interval, thus the reference atmosphere was varying only within 0.5 $\mathbf{S}_m$ (i.e., the sounded atmosphere was considered almost constant). In the revised version of our work, we changed this approach and the reference atmosphere changes from sounding-to-sounding by a much larger amount, consistently with a covariance matrix $\mathbf{S}_s$ that represents the seasonal variability of the Antarctic atmosphere throughout the whole Antarctic winter. As anticipated by the reviewer, the conclusions of the work do not change; however, with the new approach, the results rely on a more solid basis.

SPECIFIC COMMENTS

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line 94: It would be interesting to note how exactly the modification is performed. I would assume by an additional factor in front of the a priori precision matrix as described by Rodgers or with, e.g., a factor and an identity matrix as is often used in numerical libraries?

We multiply the diagonal elements of the Hessian of the cost function (2), i.e., the inverse of matrix $\mathbf{S}_i$ of Eq.(5), by (1+λ), where λ is the Marquardt damping factor. We will specify this detail in the revised version of the manuscript.

Line 285: Is the emissivity linearly interpolated between the grid points? Please specify.

Yes, spectral emissivity is linearly interpolated between the retrieval grid points. In the revised text, we will specify this detail.

Line 293ff: This paragraph sounds as if there are different S_a matrices being used depending on the use case, if only w.r.t. to the surface emissivity. This should be taken up in the mathematical notation (e.g. with an i index).

Actually, the same $\mathbf{S}_a$ is used for all test cases and for both the SR and the individual retrievals, so we do not think that an additional index is required. Like $\mathbf{S}_m$ (see eq. 21), $\mathbf{S}_a$ is a block-diagonal matrix, each block referring to a specific section of the state vector: T(z), Ts, H2O(z), O3(z), e(v).
In the revised version of the paper, we will include this additional note.

We realize now that at line 296 we promise to show the a priori errors in the subsequent figures, while they are not actually shown… We apologize for this inconvenience, in the revised version of the paper we will include also a priori errors in the plots.

Line 345ff: It is surprising that the nadir sounders seem to replicate the true profile near-perfectly (on average). Due to the spatial smoothing of the true profile, I would have expected, e.g., systematic discrepancies close to the local extrema of the temperature and *water vapour profiles. I.e. there should be a difference between true and retrieved profiles, simply due to the lower spatial resolution of the retrieval result. Or was the true profile folded with an averaging kernel as well to compared within the same spaces? Is there another explanation for the excellent performance?*

There are several factors contributing to the excellent performance of the average of 900 profiles:

- The reference, a priori and retrieved profiles are all represented on the same 61 pressure levels grid, thus there is no smoothing error owing to profile interpolations.
- Therefore, the smoothing error is uniquely originated by the use of optimal estimation with an a priori profile $\mathbf{x}_a$. Both $\mathbf{x}_a$ (lines 333 – 334) and the true profile $\mathbf{x}_{t1}$ (lines 327 – 330) are obtained by applying to $\mathbf{x}_0$ a "zero-mean" random perturbation that changes from a test retrieval to the next. Therefore, the average difference (or bias) between $\mathbf{x}_a$ and $\mathbf{x}_{t1}$ is very close to zero. This implies also a "zero-mean" random smoothing error that produces no bias on the retrieved profiles. Being random, the smoothing error is then damped in the averaging process.

These conditions will not be fulfilled in the case of real measurements, where the a priori atmosphere is usually taken from a model that may be represented on levels different from the retrieval grid and may include also small biases. However, this setup is functional in our study to investigate the small differences between the SR and CDF solutions.

Data Availability
Is the test data set too big (or restricted by licenses) to be placed on, e.g., Zenodo?

OK, we will upload the final data set to Zenodo: although rather large (~15Gb) the dataset does not exceed the max allowed dataset size for Zenodo (50Gb / dataset).

MINOR REMARKS
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line 23: errors -> error
line 97: It'd be helpful to associate the terms with the mathematical notation in the text. I.e. CMs S_i and AKMs A_i.
line 101: convergence -> x^hat_i
line 279: This notation does not express properly, that T(p_k) expresses a subvector T(p_1) to T(p_n). Perhaps {T(p_k)}_k=1..61 ? Why T and not consistently x_T ?

The minor corrections will be all included in the revised text.
Thanks again for your kind support.