

Interactive comment on “High-resolution air quality monitoring from space: a fast retrieval scheme for CO from hyperspectral infrared measurements” by N. Smith et al.

Anonymous Referee #1

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After having reviewed this paper, I am of the opinion that it should not be published, unless subjected to a thorough rewriting, taking the following major points into consideration:

- 1) The clarity of writing: The paper is littered with grammatical errors, on top of which the way in which it has been written is often far too colloquial for a scientific journal. I cite as an example the sentence on pg. 3790 line 13 that begins ‘A popular method. . .’, this sentence is very clumsily written, and the use of the word popular is an odd choice.
- 2) The Introduction: The introduction should serve as a summary of the work that is

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related to your field of study, with plenty of references. It should also explain in detail exactly why you have carried out your research, as well as outlining the structure of the remainder of the paper. Your introduction makes far too many sweeping statements, with no viable references, e.g. pg 3789 line 16 'Most of the reported success has been achieved for retrievals spatially averaged to a resolution lower than what is measured by the instrument. . .' on what are you basing this piece of information?

3) Section 2.1: Having re-read this section a number of times, I am still not entirely sure what it is that you are doing to the data. I understand the process of using modelled data as the a priori in a linearized model, but you have failed to explain in clear and certain terms the steps that you have taken. This whole section needs to be drastically rewritten, outlining the steps you have taken in far clarity. On top of this you need to go into more detail, for example you say that there has been an integration from a profile to a total column density, but has this been done based on a system of pressure layers or pressure levels, and why has this particular methodology been chosen? Also, you give no indication as to why you have chosen Jacobians with a perturbation of 10% magnitude. In addition to this Section 2.3 reads like it has simply been copied and pasted from the IASI website, and need to be rewritten in the context of your actual research.

4) The Retrieval Scheme: More detail is required in relation to the retrieval scheme (both the linear approach that you have identified and the non-linear approach that you refer to). This does not mean that reams of equations are required, but more information that simply quoting Rodgers is essential for two reasons: 1) it helps the reader (who is not necessarily an expert in retrieval theory) fully understand what is going on; and 2) it shows solidarity with your work, and gives the reader greater faith in your level of understanding and by association the accuracy of your results.

5) The use of the RAQMS as a suitable a priori: I am slightly concerned as to the legitimacy of using RAQMS as a suitable a priori product. As you point out yourself (pg. 3794 line 21), the choice of the a priori product is critical to the retrieval process,

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therefore I find it slightly confusing as to why you have chosen a model which only has a ground resolution of $2^\circ \times 2^\circ$, given the resolution of each IASI pixel. I understand that an a priori product must be chosen from somewhere, but with no actual details of how the RAQMS model actually works given in the paper, this is quite a concern, and would go some way as to explaining why there are such large discrepancies between your results and those of the IASI level 2 operational product (as shown in Fig. 1). For example, it is quite conceivable that a $2^\circ \times 2^\circ$ grid box contains mountain, desert, and oceanic regions. More detail of how the model works, and how you have chosen the input parameter is necessary to allay these fears. In particular very careful consideration needs to be given to the surface altitude, as it could be that the model is choosing a height that is different to the satellites measurements by several hundred metres, which will have a profound effect on the retrieval.

6) The Presentation of data: In short, there is not enough. One plot of the retrieved CO column amounts, really tells us very little about your scheme and its possible suitability as a retrieval algorithm. As a bare minimum I would expect to see similar plots for the DOFS and the error terms. Choosing one specific geographical area of focus is not a problem, but you need to make sure that a full and detailed analysis of the results has been produced.

7) The Retrieval Efficiency: It is stated in the abstract that the main benefit of this retrieval scheme, in comparison to those that are already available, is its efficiency, and yet nowhere in the paper do you actually make clear what this is. A plot of the processing speed for each IASI pixel is essential, and a discussion on the required computer processing power should also be provided. At the moment I am left pondering the following question: 'If the IASI level 2 operational CO data can be processed in real-time using a full retrieval scheme, which it can, then what is the benefit of your simplified linear retrieval scheme?' If, however, you were to demonstrate that such a retrieval could be performed without the need of very heavy computer processing (which is what I think that you are implying), then this would be of benefit to users who were unable to

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access such processing power.

8) The Conclusion: Where is it?

I have not listed in any detail any of the minor grammatical and stylistic errors that I noticed, as it is far more important that the above major points are dealt with before publication can be considered.

Interactive comment on Atmos. Meas. Tech. Discuss., 4, 3787, 2011.

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