

Interactive comment on “Potential for the use of reconstructed IASI radiances in the detection of atmospheric trace gases” by N. C. Atkinson et al.

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Received and published: 4 April 2010

General considerations

The paper provides an assessment of the impact of Principal Component Compression on trace gas observations. This is one of crucial points in defining the feasibility of an operational PCA compressor for high spectral infrared observations, which will be especially important when data from 2 IASI instruments will have to be disseminated in real time. In this sense the paper represents a good contribution to scientific progress, and the presented applied methods are well thought and properly described. According to the referee, the key point in the paper is that "an iterative procedure, involving refinement of a base training set by the addition of outlier spectra, is successful" in applying PCA compression. Even if further studies are recommended, evidence presented in

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the manuscript to support the statement are solid and clear.

Minor considerations

This section describes minor modifications which could further improve, but not substantially change, the presented work. They might be beyond the scope of the authors and therefore are suggested and not required.

Abstract, page 502, Line 7: many studies of PCA impact on radiances were done on a broader context than NWP. Results of PCA impact on high spectral resolution infrared data have been done and published in the context of instrument monitoring, atmospheric and surface parameter retrieval, data compression, level 1 and level 2 product validation.

Paragraph 2, page 506, Line 22: Given the specific architecture of IASI it would be nice to have some information on the statistical distribution of the detectors as well as of the FOV angles associated to the observations used in the training sets. It is clearly beyond the scope of this publication, however it would be useful to understand if a detector (or FOV angle) dependent training set performs better than an heterogeneous set.

Paragraph 2 , page 507, lines 1-3: No explanation is provided regarding the criteria used to select optimal number of PCs. It would be useful to the reader to know which procedure was used.

Subparagraph 3.1, page 508: if possible it would be useful to the reader to have an estimate of the spectral correlation of the reconstruction residuals for selected lines for each of the different training sets.

Subparagraph 3.1, page 508, line 25: "we can see", impersonal form would be preferred.

Subparagraph 3.1, page 509, lines 5-9: by projecting PCs 38 and 40 on the training spectra, it would be possible to determine which channels are actually contributing the

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most to the PCs and also which fraction of the signal the channels are contributing. This might be of interest to reader.

Subparagraph 3.2, page 509, lines 22-24: it is interesting that training set 2 over-estimates the SO₂ content, while set 1 under-estimates it. Do the authors have an idea why this might be happening?

Subparagraph 3.4, pages 510-512: was the error covariance matrix used in the physical retrieval of CO updated when reconstructed radiances were used? Or the same error covariance matrix was used in the two cases? This point is also relevant for figure 11 where the results are shown as fraction of retrieval error. This retrieval expected error should be different for the two cases and it would be nice to see how it changes.

Subparagraph 3.4, pages 511, line 16: "Enhanced CO levels", in this context "enhanced" seems to be an ambiguous term according to referee.

Paragraph 4, pages 512, lines 17-23: refinement of the noise normalization matrix is a good idea however if refinement is done by adding to initial guess the covariance of the residuals, then improvements are expected only where initial guess under-estimates real noise. From studies carried out by referee himself, there are spectral regions where CNES noise seems to over-estimate the real noise. A better approach could be to use covariance of the reconstruction residual as estimated noise to be used in normalization.

Paragraph 4, pages 512, lines 17-23: the spatial correlation, up to the referee knowledge (based on preliminary studies), seems due to peculiar characteristics of a few IASi channels where indeed the noise covariance matrix does not seem to be properly characterized. If the authors of the paper came across the same impression, it would be nice to have it mentioned in the paragraph.

Figure 12: y axis label uses "nw" which should probably be "mW".

Interactive comment on Atmos. Meas. Tech. Discuss., 3, 501, 2010.

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