**Atmospheric
Measurement
Techniques
Discussions**

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

N. C. Atkinson et al.

nigel.atkinson@metoffice.gov.uk

Received and published: 12 May 2010

The authors would like to thank Paolo Antonelli for his comments. These are reproduced below, along with the replies.

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.

REPLY: We will replace this sentence with "Studies on PC analysis of hyperspectral IR sounder data have been undertaken in the context of numerical weather prediction (NWP), instrument monitoring, and geophysical variable retrieval, as well as data compression."

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.

REPLY: We will add the following in Section 2: "In each case, data from all four detectors were included in the training set. Some experiments were performed in which separate training sets were used for each detector, but whilst some differences were observed between the detectors, the reconstruction scores and spatial correlations were not substantially affected. Therefore this issue is not discussed further."

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.

REPLY: We will add an explanatory paragraph: "For sets 2 and 3, the optimal number of PCs was selected for each band by plotting the PC score spatial correlation as a function of eigenvector rank, as described in Atkinson et al. (2009). Low-order eigenvectors show high spatial correlation as they mainly represent atmospheric structure, whereas high-order eigenvectors show low spatial correlation as they are dominated by random noise. In the case of set 1 a simpler method was used: a plot of the eigenvalues was examined and PCs were selected up to the point where the slope of the curve stabilised."

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.

REPLY: We agree that a study of reconstruction residuals for particular lines, including their correlations, could be informative. Essentially, this would be trying to answer the question of whether the residual is dominated by noise or whether there is some atmospheric signal contained in the residual. We do show reconstruction scores evaluated over the band (Figure 13), which conveys similar information. As suggested by reviewer #2, we will add a figure showing the reconstruction score for the Kasatochi case, with the set 4 eigenvectors.

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

REPLY: Accepted.

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

REPLY: We assume the reviewer is asking about the reconstruction matrix, which provides information on which channels in the original spectrum contribute to the reconstructed spectrum in the region of the ammonia lines. This would be an interesting additional study but is beyond the scope of this work.

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

REPLY: We would speculate that the training sets contain some low-level SO2 content, but with poor signal to noise. So when a high SO2 situation is encountered the errors

are amplified. But we would not wish to put anything definitive in the final paper.

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.

REPLY: The observation error covariance was the same in both cases, since the purpose of the paper is to verify that reconstructed radiances can be used as raw radiances without damaging results to any great extent. This will be clarified in the final version.

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

REPLY: This will be changed to "higher".

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.

REPLY: The approach that we have followed is in fact similar to that suggested by the referee. We propose the following clarification to the text: "The refined noise covariance estimate was computed as a sum of the covariance of the reconstruction residual and the noise covariance of the reconstructed radiances. The latter is equal to $\mathbf{NEE}^T\mathbf{N}^{-1}\mathbf{RN}^{-1}\mathbf{EE}^T\mathbf{N}$ (Hultberg, 2009) and can be computed if an estimate of the original IASI noise covariance, $\mathbf{R}$, is known. The IASI noise covariance from CNES

was used for **R** in this computation. In a second iteration the previously obtained overall noise estimate was used for **R**.". The Hultberg, 2009 reference is the IASI PCC "Frequently Asked Questions" page on the EUMETSAT web site.

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.

REPLY: For some channels the radiance noise varies with scene temperature. Therefore the CNES noise, which is derived from measurements of the on-board black body, may not be the same as the PCC noise, which is derived from measurements of the atmosphere. There may still be discrepancies between the two, but we are not in a position to make any firm statements on this issue.

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

REPLY: It should be nW (capital W). We will correct the plot.

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

C387