



EGUsphere, referee comment RC1
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Comment on egusphere-2022-1372

Anonymous Referee #2

Referee comment on "Near-real-time detection of unexpected atmospheric events using principal component analysis on the Infrared Atmospheric Sounding Interferometer (IASI) radiances" by Adrien Vu Van et al., EGU sphere,
<https://doi.org/10.5194/egusphere-2022-1372-RC1>, 2023

General comments

The paper describes a method for detection of unusual IASI spectra based on the noise normalised (PC) reconstruction residual. Each channel of the noise normalised reconstruction residual is a linear function of the radiances. 11 channels corresponding to absorption lines of selected gases are chosen for the detection (in the appendix a larger set of channels (ranges) is presented but the relation and role of each of these channel sets is not clear). While, by construction, the method is well suited to identify unusual spectra, the allocation to specific molecules can cause false detections as illustrated in the paper with the (false) HNO₃ detection in a volcanic plume.

Specific comments

Section 3.1 needs some corrections. N is defined as the instrument noise covariance matrix, which is consistent with its use in equation 1, but not in the following equations, where it should be the matrix square root of the instrument noise. Line 102: this is not a projection (look up the definition). Line 107: "conservatively"?? Line 115: "optimal number" optimal in what sense? Line 97: is it really necessary to give a formula for the covariance matrix, especially since this formula is not the best way to actually compute it.

The small size (120000) of the training database is problematic because the computation of the 8461*8461 covariance matrix will be affected by instrument noise as well as unusual spectra (which can be hard to avoid).

There is no evidence presented for the usefulness of the thinning towards the poles "in

order to not over-represent high latitudes”.

Line 131: then? And how can a random selection help to “represent all the conditions” – should be better to keep all.

Section 3.3: 20? Actually 1 PC is enough to “depict (sic!) most of the atmospheric variability”. While 150 PC is a good choice this is not related to any percentage of the total variance but rather the signal to noise ratio of the remaining PCs.

Section 4:

The details, especially regarding the detection thresholds, are hard to understand and should be rewritten. Also I miss justifications for the choices made.

Why not apply thresholds for each individual spectrum, instead of the granule min and max? Faster (line 152)? No, I don’t believe so.

The use of 3 different thresholds seems unnecessarily complicated. And the second threshold might be counterproductive in case most of the granule is affected by a similar anomaly.

The term “signal intensity” is used without being introduced. It is simply the (absolute value of) the (noise normalised) residual and the new term does only confuse.

It is not clear why a second training set was used for the computation of thresholds. Why different threshold for day and night?

Line 442-443 and 506-507: This is what was already discussed on page 10. I feel it is wrong to talk about “artefact” and “reconstruction error” in this context. The detection method of the paper is nothing but the identification of reconstruction error. That an unusual perturbation in a limited number of channels can affect the reconstruction residual in other channels is natural and unavoidable (as you can convince yourself by looking at a two dimensional space with 1 retained PC). Actually maybe the biggest contribution of the paper is that it shows that this kind of “cross talk” seems to be relatively rare in practice.

Technical corrections

Line 19: horizontal => spatial

Line 120: insert "as"

Line 145: "maximum of information"?

Line 154: noise normalised "residuals"

Line 182: I don't understand this sentence