# Interactive comment on “Ozone-sensitive channel selection over IASI full spectrum with correlated observation errors for NWP” by Olivier Coopmann et al. Anonymous Referee #1

We would like to thank the Referee for his/her valuable comments. Referee’s comments will be answered one by one in the following. As the manuscript has been thoroughly modified after the suggestions of several referees, some minor points will not be addressed here, as the corresponding sections may have been deleted or replaced.
Please note that the objectives of the paper have changed a bit. We now are using the full band 1 and band 2 of IASI to carry out a new channel selection from scratch, as advised by referees. Title has been modified accordingly: Update of IASI channel selection with correlated observation-errors for NWP.
Original text from the referee is in black, our answers in blue.

Overview

The paper proposes a new method of IASI channel selection based on a full representation of the observational error covariance matrix rather than the standard diagonal (ie uncorrelated error) matrix assumption. This observational error matrix is derived from the difference between a total covariance matrix based on the residuals from (IASI - model simulations), and a separate estimate of the model-only (or background) error covariance matrix. The results are analysed with respect to the reduction in standard deviation of the residuals after (effectively) a retrieval has been performed using these radiances. The performance of the new channel selection is also compared with an old channel selection based on the uncorrelated error assumption.

General Comments

The essence of the method presented is to derive an observation error covariance So(or R) from St = So + Sb where St is the total error covariance based on the difference between IASI observations and a RTM calculation based on MOCAGE model output, and Sb (or B) is the background error covariance associated with MOCAGE. The tricky part is to estimate Sb with sufficient accuracy that So can be derived. As the authors mention, this is routinely performed in data assimilation. In that frame work usually I would expect Sb « So so that So∼St (ie differences are dominated by the observational error) and inaccuracies in Sb are less critical. In this case it also seems to be concluded that Sb « So (judging by the small differences between the red and blue curves in Fig 9a), meaning that most of the total covariance is ascribed to So. And these errors - 1 or 2 K - really are large. Do the authors believe these come from the IASI instrument or the RTTOV model? And surely if they are this large someone else would have noticed by now? Is there any other evidence to support this magnitude of error? However whether Fig 9(b) really represents So (=B) depends crucially on whether Sb has been correctly evaluated, and I suspect it has been underestimated. It is not clear from the paper whether the IASI ozone channel observations have been assimilated into MOCAGE in the first place. If not that might explain why MOCAGE produces an artificially uniform ozone field which would be misinterpreted (in the NMC method) as a low background error covariance. Even if ozone channels are assimilated, the B matrix seems to be estimated as a global mean (again, not clear from the paper) rather than specifically for each site or for the mean of the set of sites. The complexities of establishing the observational error covariance matrix aside, it is not demonstrated that the channels (or results) obtained with such as matrix represent any improvement on the channels obtained with a diagonal covariance assumption. This, and the irregular behaviour of the RED as a function of the number of channels used (Fig 11) both seem symptomatic of a misrepresentation of the R matrix, presumably originating from the estimate of the B matrix. Although the authors argue that their selection is also designed to improve temperature and humidity, I am unconvinced that it represents any improvement in channel selection. The improvements in T, q seem small and may even be achievable with a random selection of additional channels in this spectral region. The authors confine themselves to considering only additional O3 channels. A more robust test of their method would be a complete channel re-selection and a demonstration that this does indeed lead to improved results for T,q as well as ozone. So, as it stands, while the proposed method is plausible, I am not convinced that it has been correctly applied (or indeed if it can be) and neither am I convinced that the additional complexity of this method results in any objective benefit.

In our revised manuscript, we still rely on the same methodology to have the best estimate of the R matrix. As we now are focussing on the full band 1 and band 2, representing 5500 channels together. Thus, instead of using 345 profiles, the first estimate of the observation error standard deviations is now done on 6123 profiles, same for the Desroziers diagnostic. Both first estimates and diagnosed values are smaller using much more profiles, they are given in our new figure 5. Diagnosed sigma_o are rather close to the instrumental noise, except for the ozone band and, to a lesser extent, for some parts of the water vapour band. Please note that “blue curve” and “red curve” are much different from each other, as we are using thousands of profiles to compute the statistics. Which means that sigma_o and “sigma_b projected into the observation space” are balanced, with the exception of the beginning of the CO2 band (temperature profiles in the background have a small error) and on part of an atmospheric window. Then, diagnosed sigma_o (blue curve) can be compared to the instrumental noise (provided by CNES and translated at a scene temperature of 280K, black curve): total sigma_o seems to be composed half of the instrumental noise and half of the forward model error. Errors in the forward model may include: emissivity error, spectroscopic errors (mainly in the water vapour band) and also errors in the atmospheric profiles in input. The latter have contributions around 668 cm-1 (high stratospheric temperature), in the ozone band (MOCAGE still have deficiencies, for instance in the UTLS) and in the water vapour band. Our diagnosed values are consistent with those diagnosed at other centres such as ECMWF or the MetOffice.

MOCAGE does not assimilate any observation in our setting. All methods to derive background error estimates have deficiencies. The NMC may have a tendency to under-estimate these errors, especially when no assimilation is used (which is the case for ozone in MOCAGE). We added a sentence in the text :

“It should be noted that the ozone background-errors estimated here are the result of differences in meteorological forcing from ARPEGE and not chemical differences.”

The B matrix is indeed computed as a global average and no scene-dependence has been accounted for in our study.

Finally, as pointed out by the Referee, instead of adding channels from the ozone band to an existing channel selection, we have carried out a channel selection from the beginning. The differences between our method and the previous selections are described in the text. We hope that the scope and the objective of the manuscript is now clearer.

Minor Comments

I found it difficult to keep track of all the different sets of channels referred to in various parts of the paper. Perhaps a summary table would help? I find the text reads better if references which are to be read as part of the text are presented, for example, as "... was performed by Collard (2007) ..." rather than put the complete citation in brackets "... was performed by (Collard, 2007)". But that's just my personal preference.

We have adopted the "Collard (2007)" notation in the manuscript. Furthermore, the subsample of Collard's selection in band 1 and band 2, which counts 275 channels, is named CS275 in the text.

P1 L16: quoting these percentages without explanation of context is a bit misleading.
For example it might be understood that the humidity error has been reduced from, say,50% uncertainty (or whatever it was) down to 30.1% uncertainty - which is certainly not the case.

Rates of improvement are not used anymore in the paper.

P2 L6: 'uses 75% of observations from infrared sounders'. Does this mean, of all the observations made by infrared sounders, it uses 75% of these. Or does it mean that 75% of the observations used come from infrared sounders? Also, does an observation count as a single IASI pixel or a single IASI channel ?

We now quote only IASI usage in the text:
"Assimilated radiances from IASI (a sub-set of 124 channels from Collard's selection) represent more than 60 % of all assimilated observations (conventional and satellite) in 4D-Var data assimilation process."
The observation count is done for each individual value (i.e. a single IASI channel or, for radiosounding for instance, a variable at a given vertical level).

P2 L11-12: I suggest rearranging to make it clear that the 0.25cm-1 sampling is what leads to 8461 measurements rather than the 0.5cm-1 resolution. It's also probably worth mentioning at this point that the reduction from 0.25cm-1 to 0.5cm-1 is largely due to the Gaussian apodisation routinely applied to the spectra as part of the processing rather than an inherent property of the interferometer itself.

We have written in a clearer way:
"IASI spectrum ranges from 645 to 2760 $cm^{-1}$ with a spectral sampling of 0.25 $cm^{-1}$ leading to a set of 8461 radiance measurements with a spectral resolution of 0.5 $cm^{-1}$ after Gaussian apodization."

P3 L4: One point which should perhaps be mentioned is that although Collard selected channels assuming a diagonal observation-error covariance, he also imposed a requirement that adjacent channels are not selected. This was specifically to avoid the noise correlation between adjacent channels that is introduced by the apodisation.

The referee is right. It is now clearly stated in the text:
"In addition, in order to reduce the impact of spectrally correlated errors, the selection was made by excluding adjacent channels, which removes more than half of all IASI channels."

P3 L19: RTTOV is a notorious example of a 3rd-order acronym, probably best left unexpanded, especially since the TOVS part is now largely historical (and TIROS even more so). However, you should provide a reference at this point.

Agreed, RTTOV is not expanded anymore.

P3 L24: later you say you discard the inter-variable correlations in favour of a univariateB matrix (P13 L1)?

It should be clearer now.

P4 L2: I suggest 'radiosonde launch sites' However, are these all actually ozonesondes rather than radiosondes (or both?). Otherwise how else do you get your ozone profiles for later?

Radiosondes are not used anymore.

P4 Fig1: The caption is confusing. The map *only* shows the radiosonde launch sites from the WOUDC network. The mention of 345 profiles selected from these sites is better left in the main text.

Radiosondes are not used anymore.

P4 L15: I assume you are referring to the cloud fraction reported in the IASI L1C spectra (rather than, say the Eumetsat L2 product). But how do you know that the matched radiosonde measurements are not taken in cloud?

Right, we are referring to the cloud fraction provided in IASI L1C BUFR.
Radiosondes are not used anymore.

P6 L21: '... multiplied by 10% ... etc' - I don't understand what this means or why you have done it.

In fact, water vapour and ozone are varying a lot on the vertical (several orders of magnitude), thus to have a fairer view of the sensitivity with respect to these quantities, the Jacobian can be multiplied by a fraction of the actual vertical profile. This is not need for temperature Jacobians, as temperature remains within the same order of magnitude along the atmosphere.

P6 L20: It is not clear what units are used for humidity and ozone. This will affect the definition (and shape) of the Jacobian.

cf. previous comment.

P7 Fig3 I'm not convinced that this figure is helpful since the magnitude of the Jacobian also depends on the thickness of the model layers and the use of channel index as the x-axis is confusing. What do the two vertical lines indicate? I suggest it would be more informative to have a plot similar to Fig 2 but showing the pressure at which transmittance to the top of the atmosphere reaches 1/e since the accompanying text largely discussion the altitude from which the information comes (this would be the same for temperature or composition). For the window channels, where the total transmittance is always greater than 1/e, the appropriate pressure would be a weighted average of the atmospheric and surface contributions.

Lines in the Fig3 in the first version of the paper were separating the window channels from the temperature and water vapour channels. A similar figure is shown in the new version of the paper (Figure 3 of the new version). It covers the full band 1 and band 2. It helps identifying the various sensitivities along the spectrum at a glance. We have modified our colour scale to be able to see more details on the vertical.

P8 L14: 'where there is on average the most humidity' ? Is this relative humidity or $H_2O$ mixing ratio. In any case I would expect Jacobians for any species (not just $H_2O$) to be most sensitive in the mid-troposphere simply because this is where the combination of the product of the temperature contrast (against the earth surface background) and number of molecules of absorber reaches a maximum. Lower down the number of molecules is larger but the temperature contrast vanishes, so you see nothing. And higher up the temperature contrast is larger but the number of molecules becomes vanishingly small, so you see nothing.

Humidity is in ppmv for the Jacobian computations and RTTOV simulations.
The referee is right on the mixed impact coming from both temperature and humidity. We tried to account for the variation in the number of molecules by plotting humidity Jacobians times a fraction of the actual humidity profile.

P8 Fig 4: Since this is the first reference to IASI bands, the caption should at least say where Band 1 and Band 2 lie on this plot.

IASI bands and sensitivities are now described in Table 1 of the new version of the paper.

P8 L16: There should be some mention of which molecules have been included in the RTTOV calculation. I believe that RTTOV lumps a number of these together as well-mixed gases, so the concentrations are presumably constrained to some fixed value(appropriate for a particular year, if it includes $CO_2$ and CFCs?).

A description has been added:

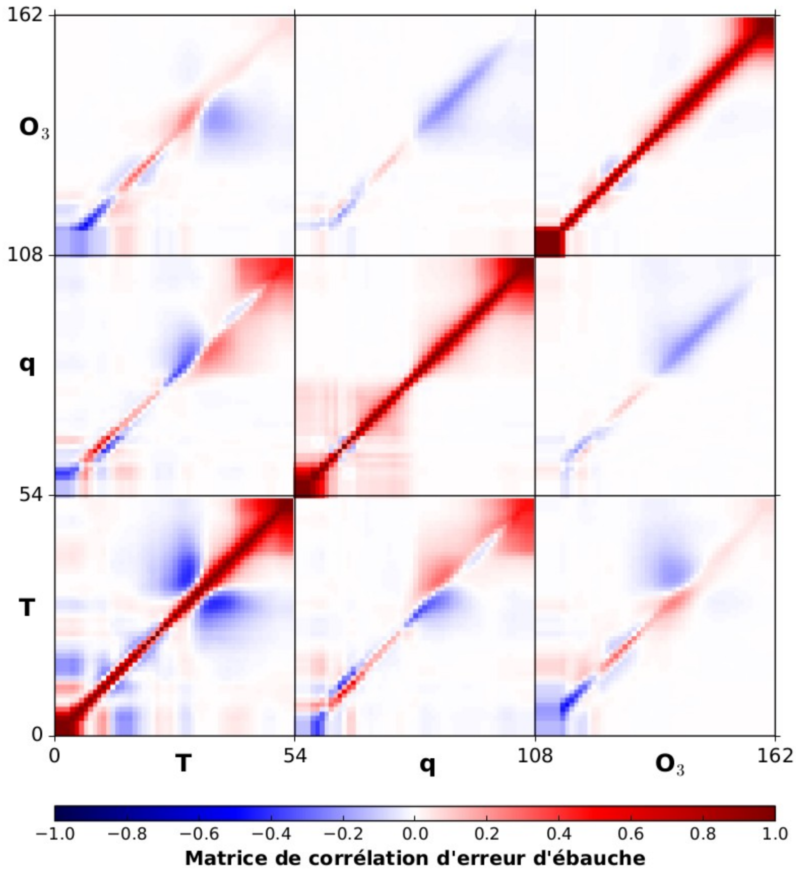
"the input atmospheric profiles (temperature, humidity and ozone) are variable and provided by the users, the other constituents such as CO 2 , CH 4 , CO, N 2 O, etc. can also be provided or are assumed to be constant profiles in time and space (depending on the version of the coefficients)."

P13 Fig 8: Given the inversion of the y-axis to have level 0 at the top, it seems more natural to similarly invert the x-axis so the main diagonal extends from bottom left to top right.

We are sorry, but we kept the same representation in this version.

P13 L1: I note the restriction to univariate B matrices but, even so, it would have been interesting to see the full correlation matrix for T,q and O3.

Please find the full correlation matrix below. We have decided not to include this figure in the paper.




P13 L8: Bormann et al discuss only microwave instruments, which are very different to IASI. They make no comment on the applicability to hyperspectral infrared sounders.

We now refer to Bormann et al 2016 which is about IASI.

P13 L10: Ventress and Dudhia constructed their R matrix using a 'bottom up' approach of estimating separate sources of forward model uncertainty, as opposed to the 'top down' approach used here.

We kept a description of the work of Ventress and Dudhia for discussion. We highlight the difference between their “bottom up” approach and our more “top down” approach.

P13 L12: Perhaps I have misunderstood, but the SD represented in Fig 4 is surely a combination of observation error (by which I mean instrument noise and RTTOV modelling error) *and* background error represented by the failure of MOCAGE to represent the real atmosphere?

That is correct: standard deviations of observation minus simulation include both observation and background errors.