Interactive comment on“Ozone-sensitive channelselection over IASI full spectrum with correlatedobservation errors for NWP” by Olivier Coopmann et al.

Anonymous Referee #2

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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.

General comments:

The general theme of this paper, increasing the channel selection used operationally for IASI to include ozone sensitive channels, is relevant and useful. It's clear a lot of work has been done by the authors to generate a background error covariance matrix, an observation error covariance matrix, do a channel selection, and test various combinations of channels in a 1D-Var context. However, I can't quite convince myself that there is problem here that is worth solving. The authors end up choosing 15 ozone-sensitive channels from the 306 available, with a stated aim of increasing both ozone and T/q information in the retrieval. Along the way, they test the addition of many more ozone channels to look at the combined information content. That seems to me to be a pointless exercise – why would you add 306 ozone channels to 122 operationally assimilated T/q/Tskin channels? If you wanted to increase the information content for T and q as well as ozone, you'd add 15ozone channels, and another 100 T channels and a few tens of q channels, not 306ozone channels. Furthermore, as is common with proposed innovations in channel selection, the result ends up gaining little over the channels chosen by Collard (2007). Many IASI channels are almost equivalent, and in choosing 15 from 306, this enhanced method manages to pick out four channels that are the same as the original method. I feel that I want more out of this paper. Some proof that the new channel selection is genuinely better than the Collard selection. For example, add a comparison to Figures 13 and 14. See whether the addition of 10 Collard ozone channels together with a couple of high-peaking T sounding channels, a couple of low-peaking T sounding channels and a couple of extra water vapour channels would give an even better result. You could also compare fits of the retrieved profiles to the IASI channels, i.e. O-R statistics, particularly for independent (non-assimilated) channels. Something else that would make the paper feel more complete would be to present results of a real assimilation experiment. So, overall, I feel that the paper needs something extra to complete the picture of why this work is necessary. It would also benefit from editing by a native English speaker.

Although the writing is for the most part easy to understand, there are quite a few grammatical errors (too many to correct in this review). The citations are presented in a strange way and could do with editing – e.g. "(Han and McNally, 2010)" should quite often be "Han and McNally (2010)".Finally, the paper is a little heavy on unnecessary background information – very old references to use of ozone data; overkill on the acronym expansion for RTTOV (I don't think you need any at all); too much information about the IASI instrument – and repetitive information about the spectral range and channels.

We fully agree with the Referee overall comment. We now present results from a channel selection which is not aiming at adding channels to an existing one, but at building a new channel selection with nowadays standards. Large parts of the paper have been removed, re-arranged, re-written. We hope that the paper is now offering a more useful materials for the NWP community. For sake of simplicity and time, this paper focuses on the channel selection and evaluation in a 1D framework. Results from this will be evaluated in the ARPEGE 4DVAR afterwards.

Specifics:

P1 L5: I am sure you know this, but one of the main reasons we need channel selection is because of high levels of null-space in the measurements and the effect that this has on the mathematics of inversion. In the end, data transmission isn't really an issue(many NWP centres receive the full spectrum).

The Referee is right. The text has been modified in the introduction and removed from abstract:
"The high volume of data resulting from hyperspectral infrared sounders such as IASI presents many challenges, particularly in the areas of data storage, computational cost, information redundancy and information content for example."

P1 L14: In general, analysis is used to describe a full NWP analysis. I think "retrieval" is a more appropriate term to use for this study, and I would suggest you replace it everywhere (except, of course, where you definitely mean analysis, e.g. analysis of results). It's not clear, in this paragraph, how the 345 profiles are used – is it in the channel selection or the 1D-Var study that follows?

Fully agreed on the word analysis. Analysis now refers to NWP analysis. It is also used in sigma_a (analysis error standard deviation) of our retrievals.
The number of profiles for the simulation computations, Desroziers diagnostic and final evaluation is 6123 profiles. A subset of 60 representative profiles from the 6123 is used for the channels selection. This has been described in various paragraphs.

P1: L16: The way this is written the results are quite astonishing – a 20.9% reduction in humidity error relative to leaving out the 15 extra channels? I can't follow the calculation of these numbers. Fig 13 a,b show virtually no benefit of adding the extra 15 channels.

Rates of improvement are not used anymore in the new version of the paper.

P2 L5: It sounds like you assimilate 75% of all IR sounder obs, whereas you mean 75% of all observations assimilated are from IR sounders.

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."

P2 and generally: "Metop" not "MetOp"
done (only 1 occurrence left).

P2 L25: "Ozone is beneficial" – that's not what you mean. I think you are trying to say" Use of ozone-sensitive channels could be particularly beneficial because they may additionally provide information on temperature and humidity".

The Referee is right. This text has been removed, as now the objective of the paper is to compute a new channel selection from the beginning and not only adding channel from the ozone band to an existing subset.

P3, L19: I don't think you need to expand RTTOV. It's just annoying to read all that. But I guess that decision is up to the journal. Also, see below, P5 L4.

Several Referees agree on this point. RTTOV is not expanded anymore.

P4, L10: Are your match up criteria tight enough? Pougatchev, N., August, T., Calbet,X., Hultberg, T., Oduleye, O., Schlüssel, P, Stiller, B., St. Germain, K., & Bingham, G.(2009). IASI temperature and water vapor retrievals â˘A˘TError assessment and validation. Atmospheric Chemistry and Physics, 9(17), 6453–6458.P4, l20: Why 54 levels and not 100, which is recommended for hyperspectral sounders?

Good point. Anyway, radiosoudings are not used anymore.

P5, L4: You don't say which version of RTTOV you are using. If you are using RTTOV-12, it no longer uses ISEM, but has a new sea surface emissivity model.

Correct. We use RtTOV v12 and now correctly reference IREMIS:
"This retrieval relies on the specification of emissivity values over land from The Combined ASTER MODIS Emissivity over Land (CAMEL) (Borbas et al., 2018) and from a surface emissivity model (IREMIS) (Saunders et al., 2017) over the open sea and sea ice."

P5, L11-29: This is far too much information on MOCAGE.

Agreed. The description of MOCAGE has largely been reduced.

P6, L5-14: Repetitive and too much about IASI. The swath stuff isn't relevant to the study.

The description of IASI has been shortened.

P7, L24: “Realistic but do not represent reality” – I don’t really like that sentence!
Realistic means like reality…Maybe you should say they are realistic but biased (and find a reference for that!)

It has been worded differently as parts of the manuscript have been changed.

P8, L3: Remove “typically” – either it was 10% or it wasn’t. If it wasn’t always 10%, you need to be more specific.

The “physical method” is not used anymore, this has been removed.

P8, L24: The problem is to ensure that all this information is partitioned correctly and doesn’t end up with ozone signal being transferred to Tsurf, or Tstrat, or whatever. This is why, normally, one might consider it best to pick channels with pure sensitivity to one species where possible.

We fully agree on this. Nevertheless large parts of the spectrum are sensitive to at least two variables. And some particular sensitivities, like lower tropospheric humidity, can only be found in such parts of the spectrum.

P8, L29: “MOCAGE was run to provide temperature, specific humidity (from ARPEGE) and ozone 3D distributions.” – It sounds like your temperature fields came from MOCAGE. Is that the case? If so, why?

All meteorological fields come from ARPEGE. MOCAGE is forced by these variables. As MOCAGE and ARPEGE do not use the same vertical and horizontal grids, ARPEGE fields have been projected onto MOCAGE geometry (runs for the NMC inputs). It should be worded a better way in this version.

P9, L1: I found this description nearly impossible to understand. “every day an ozone forecast up to 24 h range is produced using ARPEGE forecasts” – it sounds like you are using ARPEGE to do the ozone forecast. See previous comment about P8, L29, which was the opposite!

In section 3.4 of the new version, we hope this is better dscribed. Indeed MOCAGE is used of run the ozone forecasts. ARPEGE is used for the meteorological forcing.

P9, L23: “This assumption prevents feedback effects of ozone on temperature and humidity” – except that you say you want to extract information on temperature and humidity from the ozone channels…IF you were that concerned, you would try to pick ozone channels that were not sensitive to humidity, as Collard did, or Ventress, or Gambacorta…

We have to agree the decision not to consider the background error correlations between variables may seem not consistent with our goal. Nevertheless, this first estimate using the NMC method provides us with good estimates of B blocks but the cross variable correlations still can be improved. A next step could be to bring all this in a 4DVAR with an Ensemble Data

Assimilation, adding ozone to prognostic variables of ARPEGE, to estimate the full B in a similar way as we do in operations for meteorological variables.
The description of the sensitivity of a given channel to both ozone, temperature and humidity is quite well know thanks to Jacobians. Thus there is no reason to pick only channels which are sensitive to one variable.

P9, L25: Why are you using 49 levels, and no the 54 RTTOV levels, or some other number of levels that matches any of your input datasets?

We hope this is now better described:
"As the MOCAGE fields are provided up to 0.1 hPa, the interpolated fields have 4 levels above 0.1 hPa with similar values. Thus, we have chosen not to use the levels above 0.1 hPa for temperature and ozone background-errors. In the same manner, the interpolated fields go up to 1050 hPa, which is in fact rarely reached. We have therefore chosen not to use the first 2 levels. Finally, as for the B matrix provided by the 1D-Var, we have chosen not to use the levels located in the stratosphere for the humidity background-errors.
In conclusion, the 1D-Var experiments and the channel selections will use the temperature [K] and ozone [ppmv] background-errors in over 48 levels from 1013 to 0.1 hPa and the humidity background-errors [log(kg.kg −1)] from 1013 to 100 hPa."

P9,L29-30: There are quite a few nowadays that use full error covariance matrices. Some in publication, some not peer reviewed (e.g.https://ethos.bl.uk/OrderDetails.do?uin=uk.bl.ethos.647097), but there are some that are already in the literature. E.g. https://journals.ametsoc.org/doi/pdf/10.1175/MWR-D-14-00249.1. Also, I think you've cited the wrong Bormann paper there – you want the other one that deals with IASI correlations, not the microwave paper.

The Referee is right. We now cite the correct Bormann et al pape, as well as other references:
"This is the case at the MetOffice (Stewart et al. 2014; Weston et al. 2014), the Environment and Climate Change Canada (Heilliette and Garand, 2015), Météo-France (Guidard, pers. comm.) and the European Centre for Medium-Range Weather Forecasts (ECMWF) (Bormann et al., 2016)."

P10, L30: "We note large positive correlations between O3 and stratospheric CO2 sensitive channels" – they look to be about 0.2 to me, which I wouldn't say was especially " large".

Sentence has been removed. The colour scale has been modified.

P11, L11 on wards: It is not at all clear how you have made the actual channel selection. You talk about, and show, a mean and standard deviation in DFS, over 345 profiles. But how do you pick each channels? Based on one main profile (and if so which?), or do you use the method that involves picking the channel that is chosen by most of the 345cases? If the latter, you need to point out that there is a possibility that you would tie for most-chosen, and that the selection amongst those two would then have an important influence on which channels were subsequently chosen, due to correlations.

We now have make it clearer. We use 6123 profiles to compute the Desroziers diagnostic and the final evaluation of the various selections. Channel selection is made on 60 profiles which are representative of the diversity in the 6123 profiles. For each of the 60 profiles, an independent ranking of the channels is done (stopped at 400 channels for each profile). New figure 8 shows the average evolution of DFS (total, T, Q, ozone, and Tskin) over the 60 profiles.

P11, L31: "Choice of selection" – that's an odd title, and doesn't really describe what you're doing in this section, namely seeing how few channels can be added yet still provide information content.

This title is not used anymore.

P12, L14: What did you do above the top of the sonde profiles? Because of long tails in the Jacobians, what you do above the model top can have a profound effect on the averaging kernels.

Radiosondes are not used anymore.

P13, L24: "Conversely, the objective of our study is to select ozone-sensitive channels with information to also improve temperature and humidity analyses" – again, why? Why not aim to select a few ozone channels and add some more T or q channels as well if you want to increase DFS there.

We now have carried a channel selection from scratch.

P14, L19: There's very little vertical information on ozone from IASI. Your own DFS plots hows that with 15 channels you have about 1.5 DFS in this system. That's not enough to locate the changes in the vertical; it's the background error covariance matrix that is defining where the information on ozone is placed in the vertical.

We agree that, with very few DFS in ozone in IASI, the vertical shape will mostly be driven byt the background error covariance matrix.

P15, L15: I don't think this is an important result. You would get a good increase in temperature information content by adding in any additional 300 channels. And nobody in their right mind would add 300 ozone channels to an operational system assimilating just 120 mainstream channels

Agreed. New selection is now made from zero.

Fig 7: I found this figure difficult to understand – what is the point of the first line of circles? It would be useful to add another day to the figure?

We left the figure as is. We hope our description in the text will help to better understand.

Fig 9a: Your IASI noise looks wrong (too low) – see Figure 3 of Hilton et al, 2012 (The BAMS paper) for example. 9a and b: how does this look compared with the normal Desroziers matrix derived from ARPEGE.

IASI instrumental noise in our new figure 5 seems to be consistent with the curves in Hilton et al 2012. We use values provided by CNES.

Fig 11: I can't see the blue dashed line at all. I found the caption confusing – you mean"Collard's ozone channel selection" – otherwise it seems like you mean the "Collard selection" as a whole (and then you'd be degrading to begin with….

This figure has been removed.

Fig 13c: I can't help but feel a little unexcited by this plot and result

We hope that our new results in new figure 12 will help.