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 H2O mixing ratio. In any case I would expect Jacobians for any species (not just H2O) 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 CO2 and CFCs?).

A description has been added:

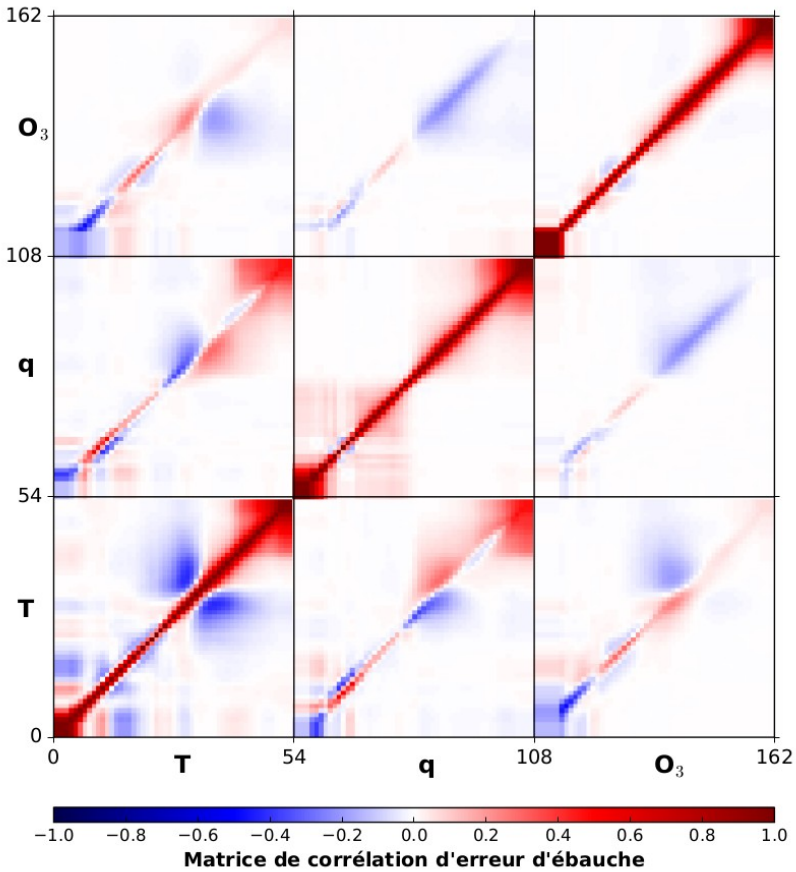
"the input atmospheric profiles (temperature, humidity and ozone) are variable and provided by the users, the other constituents such as CO 2 , CH 4 , CO, N 2 O, etc. can also be provided or are assumed to be constant profiles in time and space (depending on the version of the coefficients)."

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.

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

Anonymous Referee #2

Received and published: 12 November 2019

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.

Interactive comment on "Ozone-sensitive channel selection over IASI full spectrum with correlated observation errors for NWP" by Olivier Coopmann et al.

Anonymous Referee #3

Received and published: 22 November 2019

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Original text from the referee is in black, our answers in blue.

General comments

This manuscript describes a study on selection of channels in the 10 micron ozone band, geared towards improving the representation of not only ozone, but also temperature and humidity in NWP analyses. As indicated by the references, there has been quite a lot of previous work on channel selection and it is not easy to show strong improvements relative to existing channel sets already in use. The authors have obviously taken care and paid attention to details in this study, but in general, I found the discussion of various different channel sets and subsets confusing. I was not able to follow the argument leading to the numbers presented in the abstract that described the magnitude of the improvements in temperature and humidity analyses. I might suggest that the Introduction could be better formulated to provide a clear re-view of what other work is out there and what is important/significant about this work compared to previous studies. For example, the Ventress and Dudhia channel selection study is not mentioned until fairly deep in the paper, but it seems as though if this is an example of another study that utilized a non-diagonal observation error covariance, then it ought to be cited in the introductory material. Would it be possible to include a table or tables to (1) review previous work on channel sets for ozone radiance assimilation and summarize the important advantages (or short comings) of how they were selected and (2) summarize the channel sets considered here and their performance/impact on the ozone/temperature/humidity analyses?

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 material in a more legible manner.
Other studies now are also cited in the introduction.
Please note that we are not focusing only on ozone this revised version of the paper.

Minor comments/typos:

Page 5, line 24: Is PAN the appropriate abbreviation for Peroxyacetic Nitric Anhydride? My understanding would have been that PAN would usually refer to Peroxyacetyl Nitrate, which decomposes to form thermally stable Peroxyacetic acids (PAAs), which are then photolyzed. I am not a chemist, but it may be worth checking this one.

The description of MOCAGE has been shortened and there is no reference to PAN anymore.

Page 9: “The ozone profiles from MOCAGE are realistic but do not represent reality.”This is an odd statement. The profiles may be closer to the truth than using a standard RTTOV profile, but that isn’t necessarily saying much. It would be better to just say that the ozone profiles from MOCAGE are, on average, biased (high) relative to reality.

Agreed, the description has been modified (the whole section containing this statement has been revisited).