Replies to: Anonymous Referee #2. Interactive comment on "Data assimilation of satellite retrieved ozone, carbon monoxide and nitrogen dioxide with ECMWF's Composition-IFS" by A. Inness et al.

Received and published: 17 March 2015

We thank Referee 2 for their useful comments about our paper. We have tried to address all the suggestions and revised the manuscript accordingly. Our replies to their comments are given below in italics and changes to the manuscript in bold italics. We struggled a bit with the page and line numbers used in referee 2's comments, but assume that they relate to the document that we originally submitted to ACP and hope to have addressed them correctly.

General Comments:

Whilst it is clear that this paper represents a substantial amount of work and is suitable for publication, the reader is left with the impression that the results of the chemical data assimilation experiment, which are the subject of the paper, are presented but not significantly analysed. The results section is largely (but not exclusively) a written description of what can be seen in the figures – there is little additional analysis – this is particularly true of the section on CO. A discussion of potential weaknesses in the model, emissions databases etc, which may be indicated in areas with large departures would add interest and make the paper more readable. This has been touched on with respect to the treatment of stratospheric ozone and there is certainly some discussion on the limitations of using the observations to constrain the initial conditions as opposed to the emissions for short-lived species, however more analysis, particularly in the section on CO, would strengthen the paper. In addition, although the desire to include the comparison to REAN as a benchmark is understood, it adds confusion to the paper. There are a significant number of differences between the CIFS-AN and the REAN setup, including the assimilation of different satellite datasets. At a minimum the reason for the assimilation of different datasets and the likely impact that this will have should be briefly discussed. Also, the comparisons between the performance of CIFS-AN and REAN throughout the text are not always in favour of CIFS-AN and it is difficult for the reader to know what to draw from these comparisons. It would be helpful to the reader if more time could be spent on bringing this together in the conclusions.

Concerning the above comments:

'A discussion of potential weaknesses in the model, emissions databases etc, which may be indicated in areas with large departures would add interest and make the paper more readable'. More analysis about problems/biases in CO and link to emissions?'

The cause of some of the biases, in particular for CO, is not entirely clear. Sensitivity studies have shown that the emissions have a large impact on the surface concentrations, but a smaller impact on CO in the free troposphere or on TCCO where the contribution from the assimilated data is more important (e.g. Huijen et al. 2010). Furthermore, changes in the modelling of wet or dry deposition can have had a pronounced impact on the CO concentrations. However, we can not deduce any concrete statements about this from the 2 experiments carried out for this paper and do not want to include pure speculations. Some of these aspects are discussed in the companion paper Flemming et al. (2015) which also includes a more detailed description of the chemistry scheme, while we concentrate on the impact of the assimilated data.

We have added the following paragraph at the end of section 4.1.1:

The most likely reason for the underestimation of CO in CIFS-CTRL in the NH Extratropics is an underestimation of the anthropogenic emissions. This is also discussed in Flemming et al. (2015). It should be noted that low CO values are found by most of the CTMs regardless of the emission inventory used (e.g. Shindell et al., 2006; Kopacz et al., 2010; Fortems-Cheiney et al., 2011), and that the MACCity anthropogenic emissions are in the same range as the emissions provided by the few other emission inventories available for the post-2000 period (Granier et al., 2011). A possible reason for the generally overestimation of CO in the Tropics could be too large GFAS biomass burning emissions (Flemming et al. 2015). The only exception is the strong underestimation of CO in the biomass burning maximum in Southern Africa, which points to an underestimation of the GFAS biomass burning emissions in that area (see Figure 5 below).

Use of REAN for evaluation:

We agree with Referee 2 that REAN is not a clean comparison data set in the sense of a control run, because the model and the assimilated data have changed as the system has evolved over time. We do stress this in the paper (section 3.3). However, we remain convinced that the use of REAN is valid because it is a documented dataset produced with the model that C-IFS (CB05) was due to replace (and has now replaced). Therefore the comparison is of interest. The impact of the assimilation in C-IFS can be isolated by comparing CIFS-AN and CIFS-CTRL and we use independent observations for the evaluation. We hope we have made the reason for some of the differences in data usage clearer in the paper now (e.g. in Section 3.3. where we now explain why IASI CO data were not used in CIFS-AN, see answer to specific comment below).

We do not elaborate the differences between CIFS-AN and REAN in the conclusions. This was deliberate, because we wanted to focus the conclusions on the differences between CIFS-AN and CIFS-CTRL, where we can make a clear statement because we have a clean control run with the identical model setup. We do not think the paper will benefit from a longer discussion about the differences between CIFS-AN and REAN in the conclusions.

Specific Comments:

Page 7, line 7: Is there a plan to include correlations between the background errors of different species? This is mentioned in the conclusions but it would be useful to give an indication here and a short statement on expected benefits, or disadvantages if not taken into consideration.

We prefer to leave the future plans in the conclusions and not to discuss them in section 2.2 which describes the current setup of the system. However, we have rewritten the relevant part of section 2.2 to make it clearer that there are no correlations between the different species yet.

At present, the background errors for the chemical species are univariate, i.e., the error covariance matrix between chemical species or between chemical species and dynamical fields is diagonal. Although Miyazaki et al. (2012a) have shown the benefit of including correlations between the background errors of different chemical species, this is not yet included in the C-IFS system. Hence, each compound is assimilated independently from the others. Furthermore, the coupling of tracers and wind field via the adjoint of the tracer continuity equation is also disabled. This restricts the impact of the tracer assimilation on the meteorological fields and allows us to develop the

assimilation of the atmospheric composition data without the fear of degrading the meteorological analysis.

We have also added the following sentences in the conclusions:

A future study could look at the model response of one assimilated component to another, e.g. the response of model O3 to the assimilation of NO2 and CO data. This could be a first step towards investigating the interactions between the different chemical species before assessing the impact of cross correlations in the assimilation of multiple chemical species.

Page 7, line 12: Why are the background errors for O3 and NO2 those from the coupled MACC system? Please clarify.

This was purely because of practical reasons. All the background errors were re-calculated with the ensemble method, but unfortunately using the O3 and NO2 background errors really degraded the analysis and their use needed more evaluation. We therefore decided to only use the newly calculated background errors for CO and keep the old ones for O3 and NO2. We know this is not ideal and plan to re-calculate all the background errors with the latest C-IFS version shortly, now that the C-IFS data assimilation system is up and running. We have added the following sentence to Section 2.2:

It is planned to recalculate all the background error statistics with the latest version of C-IFS and test these in further assimilation experiments.

Page 7, line 19: What impact does the restriction in vertical coupling to five levels have for example on the coupling between the utls and stratosphere? Is this limitation uniformly applied for all model levels – if so why? The vertical correlation extent could be expected to vary with model level. Please comment?

We have modified the corresponding text to better explain why we limited the correlations in the vertical.

The vertical correlations of the O3 and CO background errors were restricted to 5 model levels below and above a level to decouple the lower troposphere from the upper troposphere and stratosphere. This corresponds to a physical difference of about 0.2 - 1 km in the lower troposphere, 1-2 km in the mid troposphere and about 3 km in the upper troposphere. The reason for this was that the original background errors had vertical correlations between the upper troposphere/stratosphere and near-surface levels that degraded lower tropospheric ozone when there was a bias in stratospheric ozone. By limiting the vertical correlations to the neighbouring levels this degradation was avoided.

Page 7, line 30: Why are the observation errors assumed to be uncorrelated in the vertical – this is highly unlikely to be the case for profile data? Is this because partial columns are assimilated? Please discuss.

Yes, this is because partial columns are assimilated. We have added the following sentence:

By assimilating partial columns we hope to avoid vertical error correlations.

Page 8, lines 18-19: Why are the original MACCCity emissions i.e. without adjustments, used here as the adjustments are considered beneficial? Please discuss.

Flemming et al. (2015) ran their experiments with the original MACCity emissions to be able to compare the results with their MOZART standalone run that also used the original emissions. We used the modified MACCity emissions (which are also used NRT MACC system) in our analysis experiments because we wanted to evaluate the MACC system as used in NRT. Because we use the same emissions in CIFS-ANand CIFS-CTRL the comparison between assimilation experiment and control is clean.

We do not think it is necessary to discuss Flemming et al.'s decision to use the original emissions in our paper. More details can be found in Flemming et al. (2015) which we refer to already.

Page 8, lines 22-23: What is the purpose and configuration of the two minimisations? Please expand.

We have added the following sentences:

The first minimization is run with simplified physics, while the second minimization is performed with improved physics after an update of the model trajectory at high resolution (Mahfouf and Rabier, 2000). Because the physics parameterizations are computationally expensive the second update carries out fewer iterations of minimization than the first.

Page 9, lines 3-4: What is the role of the observation error in the thinning process? Are they used and if not why not? Please clarify.

No, the observation errors are not used in the thinning process. However, the QC-flag check, first-guess check and variational quality control (which use observation and background errors in the calculation) are carried out before the thinning, so only 'good' data are presented to the thinning algorithm.

We make this clearer in the paper now and have restructured the relevant paragraph in Section 3.2 so that the qc checks are mentioned first:

Background quality checks and variational quality control (Andersson and Järvinen, 1999) were applied to all atmospheric composition data. The background quality check rejected observations if the square of the normalized background departure was greater than 5, while the variational quality control reduced the weight of observations that had large departures but still passed the first-guess check. Data flagged as 'bad' by the data providers were discarded. The satellite retrievals of atmospheric composition, which passed all these quality checks, were thinned to a horizontal resolution of 1° x 1°...

Page 12, lines12-13: Why was IASI TCCO data assimilated in REAN but not in CIFSAN? Please explain.

Experience from REAN had shown that the assimilation of IASI, which started in April 2008, had a big impact and led to changes in the CO analysis fields. We did not want to have such a change in our 2008 C-IFS experiments and decided to only assimilate MOPITT CO data which were available for the whole year.

We have changed the text in Section 3.3 to:

For example, IASI CO retrievals were assimilated in REAN in addition to MOPITT CO columns when they became available from April 2008 onwards, which led to a pronounced change in the CO analysis fields. To avoid such a change in the 2008 C-IFS experiments only MOPITT retrievals are assimilated in CIFS-AN. Several of these differences between CIFS-AN and REAN (for example differences in the chemical mechanisms, the biomass burning emissions, the dry deposition velocity fields, and an enhancement factor for traffic CO emissions in C-IFS) are likely to have an impact in the lower troposphere, where the sensitivity of the assimilated satellite data is low.

Page 13, lines 1-11: What is the explanation for the worse fit at Eureka due to the assimilation of MOPITT TCCO and similarly for REAN following assimilation of IASI TCCO. What is the underlying reason for this behaviour? Please expand.

The reason for this are simply differences between the assimilated IASI and the MOPITT CO data. IASI data are lower than MOPITT over land and in the SH, with particularly large differences at high latitudes during winter (e.g. George et al. 2015, submitted to AMT). This leads to a worse fit with surface observations at Eureca, but to an improved fit over the Antarctic, as is described in more detail in Inness et al. (2013).

We have added in the manuscript:

This was the result of differences between the assimilated MOPITT and IASI CO data. IASI data are lower than MOPITT over land and in the SH, with particularly large differences at high northern latitudes during winter (George et al. 2015, submitted to AMT). While the assimilation of IASI CO in REAN improved the fit to surface observations over the Antarctic it led to larger negative biases at Arctic stations (see also GAW validation evaluation below).

Page 14, lines 22-24: Why is the assimilation of CO profile information from MOPITT, IASI or TES not considered for this experiments? Please clarify.

*The assimilation of MOPITT CO profiles is currently being tested in the MACC system. If this leads to an improvement CO analysis field it will be included in the MACC NRT system and the assimilation of IASI CO profiles will also tested. We have not carried out any tests with TES data purely because we have not had a chance to do this. We already mention in the conclusions '**In future work, it will be tested if the assimilation of MOPITT, IASI or TES CO profiles can help to further correct the 3-dimensional distribution of CO.**'.*

Page 15, lines 24-25: What sensors are included in the KNMI's Multi Sensor Reanalysis – how independent is this data set from the satellite data being used in the assimilation? Please discuss.

We already describe in the supplement which datasets are used in the MSR, namely SBUV/2, GOME, TOMS, SCIAMACHY and OMI. These data are not independent to the datasets we use. However, we still think the comparison with the MSR is meaningful as it has been extensively validated against independent data.

We have added for clarification in Section 4.2.2:

Note that the MSR also used SBUV/2, SCIAMACHY and OMI data which are assimilated in CIFS-AN.

Page 16, lines 6-7: the use of “MIPAS” as a validation source is not really understood (despite the similarity to ACE comparisons) as it is not independent as the authors acknowledge. Can this be better explained as a choice?

The data were used because they were available at BASCOE, who produced the validation plot, together with the ACE data. We do not think it is a problem to show them in addition to ACE in Fig. 13, because they are very similar to the ACE data and only confirm what ACE is showing. As Referee 2 states we make it clear in the supplement that MIPAS are not independent, but adding them as extra instrument in Fig. 13 does not do any harm.

Page 20, line 7: What is the plan for adjusting emissions as opposed to initial conditions? Is this anticipated or merely noted as a likely improvement. A comment would be helpful here although this is mentioned later in the conclusions.

We hope to implement this in the MACC system, because it might really improve the assimilation of short lived species. We have added a line at the end of section 4.3.1:

We hope to include emissions in the control vector in the future so that they can be adjusted in addition to the initial conditions in the MACC system.

Technical Comments:

Page 1, line 7: framework program -> Framework Program

Changed.

Page 5, line 7: transport model -> Transport Model

Not relevant any more, as changed to ‘Tracer Model’ in the page setting stage for ACPD.

Page 9, line 12: SCIAMCHY -> SCIAMACHY

Changed.

Page 5, line 13: Please expand CB05 the first time it is used.

Done. Moved the expansion and reference from section 2.1 forwards.

Page 5, lines 13 – 14: Please briefly explain the difference between the CB05 chemical mechanism and the MOZART CTM version.

We think a discussion about the differences between C-IFS (CB05) and MOZART-3 is beyond the scope of this section and refer to the companion paper Flemming et al. (2015) instead who give a detailed description of C-IFS (CB05), a list of references for the old coupled system that used MOZART-3, and discuss differences between the schemes in detail. We have added a sentence:

A more detailed description of C-IFS (CB05) and the differences between it and the previously used coupled IFS-MOZART system is given in Flemming et al. (2015).

Page 23 Line 21 to recalculate – recalculation of

Changed.

Page 17, line 19: durin gthe -> during the

Couldn't find this in the document.