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Interactive comment on “Data assimilation of satellite retrieved ozone, carbon monoxide and nitrogen dioxide with ECMWF’s Composition-IFS” by A. Inness et al.

Anonymous Referee #2

Received and published: 17 March 2015

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

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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 set-up, 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.

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.

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

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?

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.

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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.

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

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.

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

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.

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.

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.

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?

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.

Technical Comments:

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

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

Page 9, line 12: SCIAMCHY -> SCIAMACHY

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

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

Page 23 Line 21 to recalculate – recalculation of

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

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