Reply to review 1 of paper by Lefever et al. (ACPD, 2014)

Dear reviewer,

The authors would like to thank you for your in-depth review of our manuscript. The interpretation of our validation results in the original manuscript was misleading, which led you on a false track. We will remove this obsolete interpretation of our validation results, re-write the interpretation of our sensitivity tests and our conclusions, and clarify the small but numerous imprecisions of the original text. Below is a point-by-point response to your comments (copied in italics); paragraphs highlighted in bold font will be inserted into the revised manuscript.

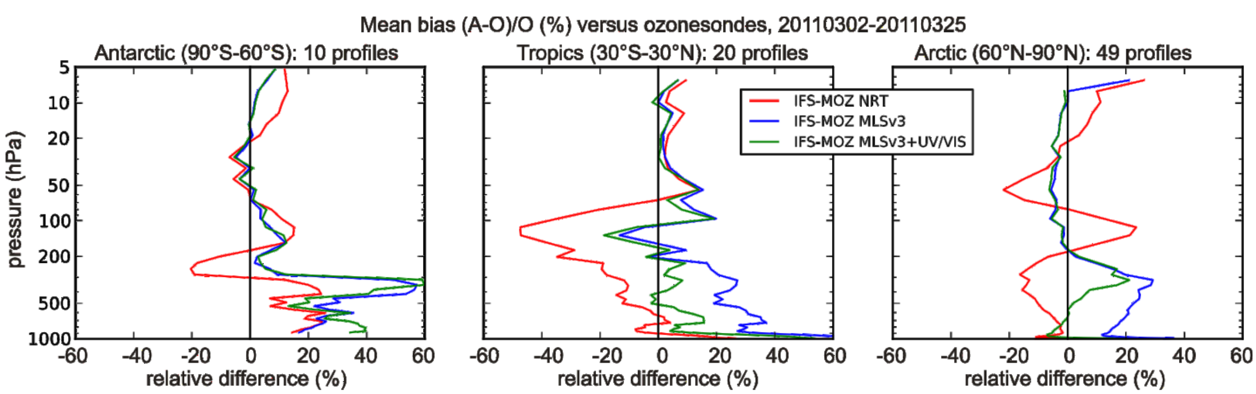
General comments

1. *In my view, the main weakness of the study is in the interpretation of the results presented. The authors don't explore the reasons behind the differences between the assimilation systems. If explanations are given they are often speculative (phrases like 'this may be due to. . .' are used). For instance, BASCOE appears to have the best performance and IFS-MOZART is worse despite the fact that it assimilates more data. Why? Section 4.1 discusses alternating bias patterns in IFS-MOZART; Section 5.2 ascribes unstable performance of IFS-MOZART to assimilation of UV/VIS but offers no explanation as to why these observations would degrade the analysis. Section 6.2 demonstrates that turning off UV/VIS is beneficial but, again, it doesn't explain why.*

   We thank the reviewer for this comment, as it helped us realize that we must rephrase several sentences of the paper, clarify our conclusions and most importantly show the results of our last sensitivity test. Indeed, the last sentence in section 6.2 of our original manuscript reads:

   *"*An additional IFS-MOZART experiment assimilating, besides MLS V3, also the other column products (as defined in Table1) shows that the analysis is well constrained by MLS alone in the stratosphere, while it is beneficial to have the combination of profile and total column data in the troposphere.*"*

   This refers to a last sensitivity test where IFS-MOZART assimilates MLS v3 down to 215 hPa *and* the UV/VIS data from SBUV/2, OMI and SCIAMACHY. We will add in the revised manuscript one last plot to show how these three IFS-MOZART runs compare with ozonesondes in March 2011:



These results will be explained in section 6.2 of the revised manuscript, which will clarify the interpretation of our results:

To identify the exact cause of the large improvement in IFS-MOZART analyses, we ran a last sensitivity test with IFS-MOZART assimilating the usual set of UV/VIS data (OMI and SCIAMACHY total columns; SBUV/2 partial columns) in addition to the offline MLS v3 dataset. As can be seen in figure [XX], the bad performance of IFS-MOZART NRT was not due to the assimilation of UV/VIS data but rather to the assimilation of the MLS v2 NRT data. If the MLS v3 and UV/VIS observations are assimilated together (green curves), the quality of the ozone analyses delivered by IFS-MOZART improves: tropospheric ozone is improved over the previous sensitivity test assimilating only MLS v3 (blue curves), and the simultaneous assimilation of UV/VIS observations does not degrade the analysis of stratospheric ozone.

The worse performance of IFS-MOZART NRT is probably not due to the earlier version of the MLS dataset either, because our sensitivity test with BASCOE (figure 12, blue lines) shows that the analyses of MLS v2.2 SCI (left) performed nearly as well as the analyses of MLS v3 (right) despite the usage of an earlier version of BASCOE. Hence the better performance of BASCOE NRT is primarily due to its assimilation of MLS v2.2 <u>SCI</u>, all the way down to 215 hPa, while IFS-MOZART had to assimilate MLS v2.2 <u>NRT</u> which was not valid below 68 hPa. This subtle difference in configuration is due to an operational constraint: IFS-MOZART had to be run closer to real-time and could not wait 3 extra days for the distribution of MLS SCI.

The words "despite an earlier version of BASCOE" are explained at the end of this letter, in our reply to your specific comment about P12495 L19.

We will also re-write the end of our conclusions (P12498 L. 4-15) to reflect the important messages to be drawn from these sensitivity tests:

From a system design point of view, the sensitivity tests performed in section 6.2 deliver important conclusions:

(i) All systems used in MACC require profile data to provide a good vertical distribution of stratospheric ozone.
(ii) This profile data must include the lower stratosphere.
(iii) IFS-MOZART is able to assimilate limb profiles and nadir products successfully. The profiles constrain well the stratosphere, allowing the partial and total columns (by UV/VIS instruments) to constrain well the troposphere.

These conclusions will also be added to the abstract of the revised manuscript, and the title will be slightly modified to reflect the importance of the input datasets:

Copernicus Atmospheric Service for stratospheric ozone, 2009-2012: Validation, systems intercomparison and roles of input datasets

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At the face of it this is surprising because SBUV agrees quite well with MLS – if its averaging kernels are taken into account. Are they used in the assimilation systems? If not then one has to contend with large smoothing errors inherent in nadir data. In the case of SBUV it may help to interpret this in the context of [Kramarova et al., 2013]. I think that the interplay between different input data types is an important aspect of chemical data assimilation and this study is the right place to dive into these issues - but none of them gets much (if any) attention. Addressing this point can be considered a major revision of the manuscript.

As explained above, the assimilation of SBUV/2 was not responsible for the worse performance of IFS-MOZART NRT. However we acknowledge that the original manuscript did not describe clearly the SBUV/2 dataset assimilated in MACC. We will re-write section 2.1.4 as follows:

SBUV/2 is a series of seven remote sensors on NOAA weather satellites (McPeters et al., 2013), of which three were assimilated by IFS-MOZART during the period investigated here (September 2009 to September 2012): NOAA-17 and NOAA-18 during the whole period; NOAA-19 after 2011-06-22. Bhartia et al. (2013) describe the two latest versions of the SBUV/2 retrievals: v8 which was available during the period investigated here, and v8.6 which was released more recently. While SBUV v8.6 includes the averaging kernels (AK) for each retrieved profile, these were not available in the v8 BUFR data used operationally at ECMWF. Hence we used the same procedure as first described for ERA-40 (Dethof and Holm, 2004): in order to decrease unwanted vertical correlations between errors at different levels, the thirteen layers of the original SBUV v8 retrievals were combined at ECMWF over six thick layers (0.1–1 hPa, 1–1.6 hPa, 1.6-4.1 hPa, 4.1–6.4 hPa, 6.4–16 hPa, 16 hPa–surface). Among the resulting partial ozone columns, the last one contributes most to the total columns.

Table 1 will be corrected accordingly. The following sentence will be inserted in the conclusions:

The newer SBUV/2 v8.6 profiles are distributed over 21 layers and each profile is distributed with its matrix of Averaging Kernels. Kramarova et al. (2013) illustrated the importance of using properly this information. While it is planned to implement SBUV/2 Averaging Kernels in the MACC NRT system at ECMWF, our last sensitivity test shows that this improvement was not necessary to assimilate successfully SBUV/2 v8 after a vertical re-gridding over 6 thick layers.

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2. *This paper will be of interest to scientists less familiar with data assimilation. It is important to make it easy to read for someone who is not fluent in DA terminology. The word 'model' is used in several places where a better term would be 'data assimilation system'. There are phrases like 'models which assimilate data' or (P12482) '. . .the model underestimates total ozone. . .' where you are really talking about analysis. Be more precise. Models don't assimilate anything!*

   In the revised manuscript, all occurrences of 'model' will be replaced with 'Data Assimilation System', its acronym 'DAS', 'Chemical Data Assimilation System', or 'ozone analysis' when the output dataset is discussed. The word 'model' will be used only to describe the forward model component of each DAS.

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3. *Explain the advantages of assimilating data as opposed to simply using retrieved observations. In particular, the conclusions section says that the four analyses perform well where data are present and not so well where there are no data. The reader may wonder why you need assimilation at all? Why not just use observational data?*

   We will re-write the relevant paragraph in our introduction (l. 11-17, p.12465) to better explain the interest of constituent analyses compared to observational datasets:

Satellite observations of stratospheric composition are retrieved with varying spatial and temporal resolutions which depend on the instrument design, the retrieval strategy and the circumstances of its operational use. Data Assimilation Systems can process these datasets (Lahoz and Errera, 2010) to deliver, at regular time intervals, analyses which are meshed on a two-dimensional grid (total column) or on a three-dimensional grid (vertically resolved field). The spatial and temporal gradients in these analyses are expected to reflect dynamical and chemical processes rather than the details of the observing system. This feature is exploited in several studies of the photochemistry of the middle atmosphere, especially in the polar regions (see e.g. Robichaud et al., 2010; Lahoz et al., 2011; Sagi et al., 2014).

Thanks to their gridded and instantaneous description of the atmospheric composition, chemical analyses enable short-range to middle-range forecasts (Flemming et al., 2011) and are much easier to use and to interpret than satellite observations. The resulting "snapshot" maps show stratospheric composition at a specific time and are routinely used to monitor the evolution of the ozone layer, e.g. above the Antarctic (Antarctic Ozone Bulletins distributed by WMO/GAW: http://www.wmo.int/pages/prog/arep/gaw/ozone/index.html).

   We could not find in our conclusions any paragraph stating specifically that "*the four analyses perform well where data are present and not so well where there are no data*". In any case the end of our conclusions will be re-written (see above our reply to the first comment).

   While our study does not intend to explore specifically the added value of Chemical Data Assimilation in the stratosphere and upper troposphere, this area has indeed gained interest: Richard Ménard (Enviroment Canada) and Quentin Errera (BIRA-IASB) have recently formed at the International Space Science Institute a Study Group to explore precisely this topic (http://www.issibern.ch/program/teams.html#Teams2014).

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4. *Figures: The labels are too small to read without strong magnification. Almost all plots have that problem.*

   We apologize for this technical issue. It looks like figures 2, 4, 5, 8 and 10 were erroneously re-sized to allow them to fit the special page layout of ACPD. We will make sure with the production office that all figures are easily readable in the published version of the revised paper.

Specific comments

Abstract. Too long. In particular, when you state a result you do not need to provide explanation. For example, on P12463 L20, everything after the comma can be deleted.

We will re-write most of the abstract, using the new introductory and conclusion text given in the replies to reviewers. This revised abstract will be no longer than 35 lines (current length is 45 lines), taking into account the guideline reminded by the reviewer.

P12464 L1 'may be related'. That's speculative. I would delete this part of the sentence.

This will be done.

P12465 L12 'information of satellite observations' → information obtained from satellite observations.

This will be done.

P12468 L4. '(…) with a vertical resolution of about 3 km.' Add 'in the stratosphere'. It's actually closer to 6 km in the mesosphere.

This will be done.

Section 2.1.3. Please clarify. Do SACADA and TM3DAM use both, total ozone and profiles from GOME-2? How many independent pieces of information do the profiles have?

As stated in the title of this section and summarized in Table 1 ("TC" versus "PC" in column "Assim. Data"), the GOME-2 data assimilated by SACADA and TM3DAM consists only in total columns. One sentence will be added at the end of section 2.1.3 to clarify this:

This study used only GOME-2 total columns: the GOME-2 ozone profiles were not assimilated.

P12469 L 23. '(: : :) of which four are currently still operational: NOAA-16/17/18/19' Please, double check that. As far as I know NOAA-18 SBUV is not operational (http://www.oso.noaa.gov/poesstatus/spacecraftStatusSummary.asp?spacecraft=18) and NOAA-16 SBUV data are not usable at the moment.

Indeed this must be corrected. As explained above, section 2.1.4 will be re-written and will start with the following sentence where the correct dates and satellite numbers are given:

SBUV/2 is a series of seven remote sensors on NOAA weather satellites (McPeters et al., 2013), of which three were assimilated by IFS-MOZART during the period investigated here (September 2009 to September 2012): NOAA-17 and NOAA-18 during the whole period; NOAA-19 after 2011-06-22.

Section 2.1.4. As for GOME-2, I suggest discussing the number of independent pieces of information and/or smoothing errors inherent in ozone partial columns retrieved from nadir observations. For SBUV there is a good analysis of smoothing errors in Kramarova et al., 2013. Typically, the actual information content in the vertical is much less than suggested by the number of layers. This applies to GOME-2 as well.

This is adressed in our reply to the first general comment. Since this issue is not the cause for the poorer performance of the NRT analyses by IFS-MOZART, we think that it is not necessary to discuss the number of independent pieces of informations and/or smoothing errors in SBUV/2. This is not necessary for GOME-2 either since we assimilated only its total columns. Repeating our reply above, the following sentence will be inserted in the conclusions:

The newer SBUV/2 v8.6 profiles are distributed over 21 layers and each profile is distributed with its matrix of Averaging Kernels. Kramarova et al. (2013) illustrated the importance of using properly this information. While it is planned to implement SBUV/2 Avergaing Kernels in the MACC NRT system at ECMWF, our last sensitivity test shows that this improvement was not necessary to assimilate successfully SBUV/2 v8 after a vertical re-gridding over 6 thick layers.

P12470 L6. The MLS Data Quality Document lists two components of error: precision and accuracy. Are both taken into account in these estimates – and in assimilation?

P12470 L6 refers to the validation of MLS v2 by Froidevaux et al. (2008). The abstract reads:

> "The uncertainty estimates are often of the order of 5%, with values closer to 10% at the lowest stratospheric altitudes".

These estimates are obtained from comparison with observations by other instruments, hence should be considered as typical biases. We think that this can not be compared quantitatively with the estimations of accuracy and precision in the MLS Data Quality Document because those are obtained "upstream" by the retrieval team. In our revised manuscript each sentence about the validation of an input dataset will be moved to the dataset description. With respect to MLS v2.2 we will state more clearly:

Froidevaux et al. (2008) estimated from comparisons with other instruments that the the MLS v2 ozone profiles have an uncertainty of the order of 5 % in the stratosphere, with values closer to 10% at the lowest stratospheric altitudes.

P12469 L24. Version 8 SBUV retrievals are given on 21 layers between 1000hPa – TOA. Does IFS-MOZART combine the layers somehow to get six? How is it done?

See above: SBUV version 8.6 gives retrievals on 21 layers but we used version 8 retrievals given 13 layers. In any case the reviewer guessed correctly: this dataset is combined into 6 layers prior to assimilation. Repeating the reply above to 1st general comment:

(…) we used the same procedure as first described for ERA-40 (Dethof and Holm, 2004): in order to decrease unwanted vertical correlations between errors at different levels, the thirteen layers of the original SBUV v8 retrievals were combined at ECMWF over six thick layers (0.1–1 hPa, 1–1.6 hPa, 1.6-4.1 hPa, 4.1–6.4 hPa, 6.4–16 hPa, 16 hPa–surface).

P12470 L8-10. Again, MLS errors are reported as precision and accuracy. Is the combined error 10%? When you say that most biases disappear in v3.4 you are talking about the accuracy component. Please, be more precise here.

The original manuscript is indeed not clear as it probably confused the input data of our sensitivity tests (MLS v3.4) with the output of these tests. There is unfortunately no validation paper similar to Froidevaux et al. (2008) but for MLS v3. So after reporting about the MLS v2 validation (see reply above), the revised manuscript will state:

Sensitivity tests were perfomed with IFS-MOZART, BASCOE and SACADA using the offline MLS v3 dataset (see section 6.2). The accuracy and precision of these retrievals (Livesey et al., 2013b) are very similar to those reported for MLS v2 (Livesey et al. , 2013a) so the uncertainties of MLS v3 are expected to be at least as small as those reported for MLS v2.

P12472 L2. 'The system also includes a parameterization of the effects of Polar Stratospheric Clouds (PSCs) on the gas-phase species'. This is vague. What kind of parameterization? Does it include catalytic ozone destruction? While Table 2 provides references to model chemistry schemes it would help if it were stated explicitly whether or not each of these models includes heterogeneous ozone loss on PCS (as it is done in the case of IFS-MOZART). This is important because the paper talks about the representation of ozone holes in the analyses.

For IFS-MOZART and SACADA, clear descriptions of the heterogeneous chemistry and PSC parameterization can be found in Stein et al. (2013) and in Elbern et al. (2010), respectively. With respect to BASCOE, the vague sentence on P12472 L2 will be replaced by the following paragraph:

Heterogeneous reactions on the surface of Polar Stratospheric Clouds (PSC) particles are explicitly taken into account. The BASCOE version used here adopts a simple cold-point temperature parameterization to represent the surface area available for these reactions: type Ia (Nitric Acid Trihydrate) PSCs are set to appear at temperatures between 186 K and 194 K with a surface area density of $1x10^{-7}$ cm^2/cm^3. At gridpoints colder than 186K they are replaced by type II PSCs (i.e. water ice particles) with a surface area density of $1x10^{-6}$ cm^2/cm^3.

P12472 L14. 'In contrast, IFS-MOZART assimilates other satellite instruments apart from Aura MLS, but those are measuring only ozone as species relevant for stratospheric chemistry.' I don't understand this sentence.

This sentence is not necessary and will be removed in the revised manuscript.

P12475, L13. 'The background variance is set to 50 %.' Please, state this more clearly: The background error variance is 50% of what? The units of variance are the square of the units of the field itself. You can't get, say, ppmv2 by taking 50% of ppmv. The proportionality coefficient has to be a dimensioned factor.

This statement will be corrected:

The background error standard deviation is set to 50% of the background field, which is quite low…

P12476 L26. Define μ

Brewer data at μ > 3 were filtered out, where μ is the increase in the ozone optical path length due to the obliquity of the sun's rays (Brewer, 1973).

P12482 L4. Looking at Figure 2, the two large positive excursions in SACADA are punctuated by sharp dips before the onset of the ozone hole. This likely indicates some underlying errors in the model chemistry. I think this warrants more discussion than what is provided in the following subsection, which basically simply explains that there are no data in the polar night.

It was found that the sharp dips in SACADA Total Ozone Columns at Syowa are due to courrupted datafiles on 2011-07-09, 2011-07-10, 2012-06-29, 2012-06-30 and 2012-07-01. Figure 2 will be re-plotted with re-processed SACADA output files for these dates.

P12482 L20 positive biased ! positively biased

This will be corrected.

P12483 L9. 'Latitudes not covered by observations can therefore only be influenced via tracer transport.' – and chemistry.

This will be corrected.

P12483 L24-27. Does this mean that only the two sonde measurements directly below and above a model level were used in the interpolation? Doing it this way will introduce a lot of noise. Assuming that assimilated ozone at a given layer represents the mean mixing ratio within that layer, the best strategy would be to integrate the portion of a sonde profile that falls within that layer and compare that (pressure weighted) integral with the mixing ratio value from assimilation. Looking at Figure 4, there is a lot of scatter in the sonde data vs. assimilation. I wonder if some of it is due to the way the data are interpolated. I suggest that the sonde – assimilation comparisons be repeated using mean (mixing ratio integrated w.r.t. pressure and divided by delta p) sonde observations within each layer.

The reviewer is correct: the interpolation used only two sonde measurements directly below and above a model level. We agree that this method introduced some noise in the sonde observations. This noise cancels out when the vertical profiles are averaged over time, which explains that the sonde data shows no scatter in Figures 3, 5, 11 and the new figure (at top of this reply). Figure 4 on the other hand shows sonde data from individual profiles, so the scatter may very well be due to the imprecise vertical interpolation. **Figure 4 does not add much information compared with figure 3, so we will remove it from the manuscript as well as the two paragraphs which discuss it** (P12484 L3 and L26).

P12487 L5 'Mixing ratios are appropriately scaled by an altitude independent factor using the model's one ozone profile.' I'm not sure if I understand. Do you mean that the background (guess) profile is scaled proportional to the ozone column increment by applying the transpose of the observation operator? People who are not well versed in data assimilation will wonder if this is some ad hoc trick or if it is part of the mathematics of the best linear unbiased estimation.

This sentence was not clear indeed. You understood it correctly, and this is a result of the observation operator using the modelled mixing ratios as input (not an ad hoc trick). But in practice the assimilated profiles will not be simply scaled with respect to the first guess, because SACADA has a flow-dependent background error correlation (Elbern et al., 2010) and also because it is a 4D-VAR system which transports the information from each observation away from its location. So we prefer to **delete this confusing sentence** and add one word on line 8:

"We find that, in the case of SACADA, the lack of information constraining **the shape of** the ozone profile leads primarily to…"

P12487 L26. I wouldn't say that the tropopause location is hard to define. We just go ahead and define it. The hard part is to correctly represent the sharp ozone gradients near the tropopause.

Indeed. We will re-write this sentence and the next one:

These sharp ozone gradients in the Upper Troposphere-Lower Stratosphere (UTLS) are very difficult to represent in three-dimensional models and probably require a very fine vertical resolution (Considine et al., 2008). Furthermore relative differences are amplified in this region due to its low ozone abundance.

P12490 L22+. How do these standard deviations compare to the ACE-FTS errors?

Standard deviations are on average around 6–7%. **This is only slightly larger than the relative mean difference between ACE-FTS and coincident MLS profiles, reported by Dupuy et al. (2009, table 7) as +4.7%.**

P12491 L6. 'Anticorrelation between levels'. Three levels are shown. Which two are anticorrelated? From the plot it looks like maybe 50 hPa and 100hPa but it's not very clear. What is the correlation coefficient?

This will be corrected in the revised manuscript:

…, interpolation at specific pressure levels (10, 50, and 100 hPa) reveals alternatingly positive and negative biases in the vertical for IFS-MOZART, both in the Arctic and in the Antarctic, especially during ozone hole events (Fig. 7),…

P12491 L14-28. I don't see how the missing MLS data can explain the differences between BASCOE and IFS-MOZART given that, as you say, the effect in March is minimal, there are no ACE data in all of April, and both systems were assimilating MLS for most of May. It's more likely that the difference arises from either different chemistry schemes or the fact that IFS-MOZART assimilated UV/VIS data the whole time and BASCOE did not. Some of the UV data may have influenced the composition inside the vortex either directly or through transport. This harks back to my general comment (1).

Indeed this interpretation was not correct. Only the sensitivity tests discussed in section 6.2 can explain the significantly worse performance of IFS-MOZART NRT in March 2011, and these tests show that the assimilation of UV/VIS data is not the culprit (see our reply to your first general comment). This whole paragraph will be replaced by a much simpler and shorter statement:

In the Arctic, the biases of SACADA and IS-MOZART become largest in March 2011. The obvious explanation is the occurrence of exceptionally low ozone abundances in the Arctic during this period (Manney et al., 2011). This event will be discussed in section 6.

The first paragraph of section 6.2 will be replaced as follows:

On 26 March 2011 Aura MLS stopped sending data and resumed normal operations on 19 April 2011. BASCOE ran freely (unconstrained CTM mode) during this time, and started again to assimilate MLS as soon as observations came back. IFS-MOZART assimilated only UV/VIS observations from 26 March 2011 until 10 May 2011, when the assimilation of Aura MLS was switched back on. Unfortunately, ACE-FTS did not collect any measurements in the Arctic during April 2011 (see Sect. 2.4.4).

These uncontrolled modifications of the observing system led us to explore in a more systematic manner the impact of the assimilated observations on the quality of the analyses. We chose a one-month period with the Arctic ozone depletion already well underway while MLS and ACE-FTS were still scanning the area, i.e. the month of March 2011. We first defined three new experiments with IFS-MOZART, BASCOE and SACADA assimilating the same dataset: Aura MLS version 3.3 offline ozone, keeping all observations down to 215 hPa. BASCOE was not allowed to assimilate any other species than ozone. To allow a short spin-up period of about one week, the three systems were started on 25 February from the BASCOE analysis delivered in NRT for that date.

P12492 L5-8. '…may be due to the fact…' – very speculative. Does this mean that UV/VIS data degrade the analysis? Why is that? See my general comment (1)

This confusing text will be removed from the revised manuscript.

P12492 L21-23. Again, can you explain why UV/VIS degrade the performance?

No, the assimilation of UV/VIS observations did not degrade the performance of IFS-MOZART. This confusing text will be removed from the revised manuscript.

P12493 L26-28. Please re-write. You don't have to go into details here but I don't think it's fair to say that ozone depleting gases are 'trapped' in PSCs. If anything is trapped it is nitric acid. The PSCs particles serve as a surface for heterogeneous reactions, which convert the reservoir species into ClO and Cl2. Then, in the presence of sunlight, active chlorine catalytically destroys ozone.

Inside the vortex, the air masses were cold enough to allow PSC particles to condense. Heterogeneous rections took place at the surface of these particles, converting chlorine reservoir molecules HCl and $ClONO_2$ into chemically active ClO and Cl_2 . Hence catalytic destruction of ozone could start as soon as sunlight came back to illuminate these air masses.

P12495 L10+. You mean underestimation of mixing ratio not ozone depletion, right? It's clear from the plot but not from the context.

At the level where ozone depletion is maximum (θ ~= 485 K), we see that the depletion is much too severe in IFS-MOZART NRT analyses and completely absent in SACADA NRT analyses.

P12495 L19. '(..) may be related': are there any other differences between the two runs of BASCOE? If there aren't than the slightly better performance is due to different versions of MLS (not 'may be').

This may be due to two different causes: the assimilation of MLS offline v3.3 instead of MLS offline v2.2, and/or an improvement in the pre-processing of the ECMWF wind fields which drive the transport in BASCOE. Indeed the BASCOE version used in NRT suffered from aliasing errors in the input wind fields, leading to some erroneous noise in the horizontal distribution of chemical tracers (Fig. 9).

P12495 L21-22. Either the extended range of MLS or the absence of other ozone data

In the revised manuscript this paragraph will be replaced by the new text which is given in bold font on page 2 of the present reply letter.

References to add in revised manuscript

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Sagi, K., Murtagh, D., Urban, J., Sagawa, H., and Kasai, Y.: The use of SMILES data to study ozone loss in the Arctic winter 2009/2010 and comparison with Odin/SMR data using assimilation techniques, Atmos. Chem. Phys. Discuss., 14, 7889-7916, doi:10.5194/acpd-14-7889-2014, 2014.

References to correct in revised manuscript

Livesey et al. (2011) becomes Livesey et al. (2013b):

Livesey, N., Read, W., Froidevaux, L., Lambert, A., Manney, G., Pumphrey, H., Santee, M., Schwartz, M., Wang, S., Cof eld, R., Cuddy, D., Fuller, R., Jarnot, R., Jiang, J., Knosp, B., Stek, P., Wagner, P., and Wu, D.: EOS Aura/MLS Version 3.3 and 3.4 Level 2 data quality and description document, Jet Propulsion Laboratory, Pasadena, California, http://mls.jpl.nasa.gov/data/v3_data_quality_document.pdf, 2013b.

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