Review of the manuscript

A comparative analysis of UV nadir-backscatter and infrared limb-emission ozone data assimilation

submitted to ACP by R. Dragani

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General Comments

I believe that this paper is a useful and valuable contribution to the field of ozone data assimilation but it fails to consider related work, and many appropriate references are missing. Hence while there is no need for any additional assimilation experiment, the text should still undergo major revisions.

1. This paper gives a false impression that ozone data assimilation is still in its infancy. There is a whole community working on this topic for a long time but none of its previous work is mentioned in the introduction nor considered in the discussion. I think that the introduction should be extended to provide proper context, and that this context should be used in the discussion.
2. The datasets assimilated in this study were developed for the ESA project O3-CCI. That project led to several validation papers which discuss extensively the uncertainties and information content of the corresponding datasets. Since a good evaluation of observational uncertainties is paramount for data assimilation, such prior work is highly relevant for this paper. Hence the O3-CCI validation papers should be at least cited in section 4, and the choices made for the present assimilation study should be discussed in this context.
3. The averaging kernels of GOME-2 nadir profiles are still not taken into account by IFS. This is a serious limitation of the present study, because many other assimilation systems now take properly into account such vertical smoothing errors. This limitation should at least be clearly stated in the conclusions and abstract of the paper: "*This study demonstrates the potentials and limitations of each dataset and instrument type*" – but only in the context of data assimilation with the current IFS at ECMWF.

Specific Comments

1. The introduction does not mention the results obtained in previous projects about data assimilation of stratospheric ozone, giving a false impression that ozone analyses are available only in two meteorological reanalyses (i.e. ERA-Interim and MERRA-2). Yet simultaneous assimilation of limb and nadir ozone datasets was reported and discussed as early as 2002 (Struthers et al., 2002). Nearly ten years ago, Lahoz et al. (2007) were already able to review this field. Even considering only the European projects, I believe that it is not possible to ignore such prior work as the ASSET intercomparison (Geer et al., 2006), the developments for the PROMOTE project (Viscardy et al., 2010) or the numerous results obtained for the MACC series of projects (see e.g. Inness et al., 2013; Inness et al., 2015). The absence of any citations about ozone assimilation in MACC is especially strange, because the MACC projects were coordinated

by the same Institution as the author (ECMWF) and relied on a version of the same model (IFS). MACC allowed an intercomparison of the ozone analyses delivered in Near Real-Time by four different systems assimilating nadir and/or limb datasets (Lefever et al., 2015). Even though the assimilation experiments were very different, this earlier study reached a very similar conclusion with a very similar Data Assimilation System (DAS): "*IFS-MOZART is able to deliver realistic analyses of ozone both in the troposphere and in the stratosphere, but this requires the assimilation of observations from nadir-looking instruments as well as the assimilation of profiles, which are well resolved vertically and extend into the lowermost stratosphere*". Overall it is necessary to extend significantly the introduction in order to provide the missing context, and to take prior work into consideration in the discussion of the results (section 5.2.2).

2. P.1, line 24: the concern for the ozone decline is primarily due to the expected increase of Ultraviolet radiation at the surface. This should be mentioned in the introduction, along with a general reference about the issue.

3. The description of the assimilated datasets (section 2) and the data quality analysis (section 4) both fail to consider the extensive validation work realized for the O3-CCI ozone datasets. At least three papers investigate the quality of the MIPAS and GOME-2 datasets which are assimilated here. Hassler et al. (2014) present an overview of stratospheric ozone profile measurement data, document measurement techniques, spatial and temporal coverage, vertical resolution, native units and measurement uncertainties; Laeng et al. (2015) and Keppens et al. (2015) compared the available retrieval algorithms for MIPAS and GOME-2, respectively, explaining the choice of the algorithms selected for the O3-CCI datasets. These studies about observational uncertainties should be used in the description of the assimilated datasets and could be useful for the discussion of the results. Miles et al. (2015) should be cited, not only as a reference for the assimilated GOME-2 dataset, but also for its specific validation results.

4. The limited vertical resolution of the GOME-2 dataset should be explained more extensively, citing a specific paper (e.g. Keppens et al., 2015) in addition to the overarching reference (Rodgers, 2000). Since GOME-2 profiles have "*between 5 and 6 degrees of freedom*", figure 2 does <u>not</u> show their vertical resolution. It shows instead the vertical grid of the retrieved product. This confusion could be seriously misleading for the novice reader. While it is less of a concern thanks to its limb-viewing geometry, MIPAS does not have perfect vertical resolution either (von Clarmann and Grabowski, 2006; Laeng et al., 2015). This should also be mentioned in section 2.

5. Description of the DAS (section 3): what is the IFS version number ("cycle") used here? How does it compare with the versions used in ERA-Interim (Dragani, 2011) and the MACC reanalysis (Inness et al., 2013) as far as ozone assimilation is concerned?

6. The modelling of ozone in IFS is not properly described, again leading to a lack of context for the discussion of the results. How is ozone photochemistry represented in the forecast model? Assuming that the parameterization by Cariolle and Teyssèdre (2007) is used here, this is not an explicit modelling of ozone photochemistry. So what does the sentence (p.5, line 15) "*In this forecast model and analysis system, ozone is fully integrated (Dethof and Hólm, 2004)*" mean

exactly? The parameterization by Cariolle and Teyssèdre has some limitations which should be stated as they could explain some of the assimilation results.

7. P. 5, lines 23-26: "*accounting for the vertical sensitivity of any retrieved product as provided by the data averaging kernels (AKs) is currently not possible in the IFS*". Please provide a reference for this limitation of IFS. Many other DAS now do take AKs into account, implementing a straightforward approach (explained e.g. by von Clarmann and Grabowski, 2006). Hence this limitation of IFS is a key caveat for this study because it limits the applicability of its findings to other DAS (see third major comment). "*With such an approximation, the vertical spread of the ozone information provided by the assimilated ozone observations depends on the background error variances and covariance (B) for ozone*". Please provide a reference about this. This approximation also fails to properly take vertical smoothing errors into account, and may constrain the analysis with *a priori* information contained in the retrievals. For example, in some viewing geometries the GOME-2 retrievals do not contain any usable information close to the surface.

8. Figures 5-6 could be quite interesting for the retrieval and validation communities who are not familiar with the estimation of observational errors allowed by data assimilation (i.e. the method by Desroziers et al., 2005). The attempt to explain this method (p.6, lines 28-32) is quite unclear, it should be re-written and expanded. P. 7, lines 18-19: "*the differences between provided and estimated uncertainties appear to be rather large*". This is an important result for the aforementioned communities (even though these uncertainties "*only represent up to about 4% of the observation values*"). Hence it should be shown, i.e. figures 5-6 should be expanded with similar latitude-pressure cross-sections showing the provided uncertainties and using the same color scale.

9. P. 7, lines 1-4: paragraph is unclear, please re-write. "*The reason for this is still under investigation at the time of writing*": indeed, this is not expected from the comparisons of ozone total columns between SBUV and GOME (Chiou et al., 2014)

10. P. 7, lines 9-11: "*The first-guess check implemented in the IFS discards all observations that, after successfully pass the data quality control, show discrepancies from the background of 30DU or more over the column"*. Please re-write (e.g. "after having successfully passed"). "*Figure 4 shows that the observations from both instruments are well within such a threshold*". Figure 4 cannot be used to justify this Background Quality Check (BGQC) because it shows a global mean of the O-A departures while the BGQC is applied to individual observations.

Minor Comments

- P. 3, line 23: remove extra closing parenthesis.
- Legend of figure 1 : state the year plotted here (i.e. 2008)
- Figure 3: right panel would be much nicer as a color-coded contour plot.
- P. 7, line 6: "instruements" → "instruments"
- P. 7, line 9: "*that, after successfully pass the data quality control*" – please re-write

- P. 7, line 18: “differencies” → “differences”
- Table 1: Acronyms “BUV” and “ODS” are missing. I suggest to list first the satellite instruments, followed by the other acronyms
- Figure 7: color scale does not work well. Use same red-blue scale as for figures 5 and 6.

Additional bibliographical references

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