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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Copernicus atmospheric service for stratospheric ozone: validation and intercomparison of four near real-time analyses, 2009–2012" *by* K. Lefever et al.

Anonymous Referee #2

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In this paper, the authors report on an extensive validation and comparison exercise on the ozone product from the MACC NRT stratospheric service. Total columns, partial columns, and profiles are compared between the four models which are part of this service and are validated with ozone sondes, UV/vis observations, and ACE-FTS profiles. In addition, results from a dedicated experiment are reported where three of the models used the same data for assimilation for the March 2011 Arctic ozone hole situation.

The paper is well written, clearly structured and provides a good description of the comparisons performed and the results found. The MACC system is a precursor of

the COPERNICUS atmospheric service and as such warrants detailed validation to provide the community with the information needed to decide for which applications to use this system. ACP is a journal read by this community, and therefore I recommend publication of this manuscript in ACP.

I have however some problems with the manuscript which the authors should consider for a revised version, although I'm aware that some of my comments are very general and difficult to address.

1. The whole set-up of the MACC stratospheric system strikes me as strange and more explained by politics than by science. Why are there 4 models at all, and why these? I can see the difference between using MLS NRT and MLS science data, but why is there not just a delayed mode IFS run using MLS science data? If the BASCOE model outperforms the other models so clearly in most respects, what is then the added value of SACCADA and TM3DAM? Which of the data sets is the user supposed to use, and what are possible applications for such assimilation systems? Why not use measurements directly or one dedicated model having good stratospheric chemistry such as BASCOE?

2. The one message that I will remember from this paper is that all systems perform well where they have assimilated the right data and perform disappointingly weak where there is no data assimilated or not the right one used (in this case O3 profiles from an IR limb profiler). This is an important message with large implications for the planning of future satellite missions but could have been delivered in a much shorter manuscript which would have been read by many more people.

3. For the same reason, large parts of the manuscript feel repetitive even if they are using different data sets for comparison – as BASCOE is strongly constrained by MLS, and MLS is in good agreement with other measurements (ground-based,

sondes, ACE-FTS), comparison of BASCOE results with different validation data sets comes down to a repetition of MLS validation. The same is true for the column assimilating systems – if SCIAMACHY / GOME-2 columns are as good as stated in the text, one would hope that the assimilation system will agree well with other observations in those locations where this data is assimilated.

In summary and somewhat provocatively, I think this is a detailed, thorough and well written validation study but I do not see a lot of readers for it.

Minor Comments

- I find the first part of the introduction a bit arbitrary and even confusing in some places. For example, I don't think that interest in stratospheric ozone and the measurement systems really started only after the ozone whole was discovered (the first TOMS and SBUV instruments were launched way before that, as were the studies about ozone depletion by gas-phase chemistry involving ClOx and NOx and the use of ozone as tracer for stratospheric transport). Also I think that the first PROMOTE project actually preceded GEMS. It might also be worthwhile to mention that data assimilation has a longer tradition for meteorological models than for models of the chemical composition.
- Aura satellite: The statement that Aura provides coverage between 82S and 82N is misleading as coverage depends also on the swath width of a satellite instrument and therefore measurements by instruments such as OMI cover all latitudes, at least when there is enough light.
- SCIAMACHY spatial resolution should be separated between nadir (32 x 60 km2) and limb
- GOME-2 nadir profiles are mentioned which is confusing as they are not used later. It might also be good to mention Metop-B in this context.

- SBUV-2 – I would replace "larger precision" by "lower precision"
- SAOZ / DOAS description is mixed up as it is not clear which parts of this are common to all UV/vis instruments and which are specific to SAOZ. To my knowledge, the only important difference is that there is a large network of similar SAOZ instruments while the other UV/vis instruments tend to be designed and operated by individual research groups.
- Alert comparison – I find differences of 50 DU for summer in the Arctic quite a lot and wonder what the reason for such large discrepancies could be in systems assimilating measurements.
- page 12487, line 6 and page 12498, line 1. I find this use of "the models one ozone profile" confusing and would suggest to replace by "the model's own profile"
- page 12490 line 22: similarly => similar
- page 12493, last sentences: The description of ozone depletion through heterogeneous processes is not quite correct, please rewrite.
- many figures are very small and have even smaller labels and axis numbers. Please enlarge.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 12461, 2014.