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Geoscientific Model Development Discussions

Interactive comment on "Validation of reactive gases and aerosols in the MACC global analysis and forecast system" *by* H. Eskes et al.

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Reply to anonymous referee 1

Summary: This paper presents a thorough overview of the validation activities relating to the MACC global composition forecast model. The program of activities developed under MACC/MACC-II will form the basis of validation for the new Copernicus Atmosphere Service (CAMS) and hence represents an important framework for future European activities in atmospheric monitoring. The paper is well-structured and written to a high standard, using precise language and grammatically accurate English. Only a small number of equations appear in the paper, but these are accurately transcribed, with all symbols described in the text. The large number of references provided is consistent with the wide scope of the paper and reliance on modelling and datasets

produced by other workers.

We thank the referee for his/her appreciation of our paper!

Specific comments

Section 5: The use of the mean-field metrics 'modified normalized mean bias' and 'fractional gross error' is fully justified and provides a consistent reference scale which allows a forecast skill for a wide range of species to be meaningfully compared. In the course of time it is likely that the model resolution will increase. However with increasing model resolution mean-field metrics are susceptible to the 'double penalty' problem. Other metrics, for example relating to model skill in predicting the magnitude of elevated pollutant levels, will typically improve with increasing model resolution. It is useful therefore to also include these types of metrics in order to give a more balanced picture of model performance. This is not necessary for the present paper but is suggested as a comment for future evolutions of the work.

We are aware of the "double penalty" issue related to the metrics used. This is something that will be picked up in the future by the validation subproject in the Copernicus Atmosphere Monitoring Service, where the introduction of new metrics is one of the work packages, and where steps in increasing the resolution of the model are foreseen. Our paper describes the current status of the validation work, and the MNMB and FGE are the metrics we have been working with in the past three years. We are not in favour of including explicit formulas for metrics we have not been using so far.

The following sentence will be included in the modified paper at the end of section 5 to discuss this point: "In the coming years, the resolution of the CAMS system is expected to increase to below one degree. The MNMB and FGE scores scores in this case become less appropriate to monitor the model improvements. Small filaments of polluted air may be slightly displaced, and the mean norms will lead to a "double penalty" for the higher resolution model, even though the simulated peak values are more realistic. The introduction of new metrics is needed for a more appropriate evaluation of the

improvements, and this is one of the tasks of the future validation subproject of CAMS."

Section 6.2 and elsewhere: Please clarify whether the results in the paper relating to C-IFS refer to the free-running model or with data assimilation.

The C-IFS modelling system is used with, and without data assimilation, and results are presented for both. The main product of MACC is the o-suite analysis and forecast assimilation system.

This sentence has been added to section 6.2: "We remind the reader that "o-suite" always refers to the IFS-based analysis and forecast system including the assimilation of the full suite of aerosol, chemical and meteorological observations."

Section 7. In future it would be useful to also include ozone measurements from suitable rural/remote surface air quality measurement sites.

We fully agree, and in fact these observations are used.

The following sentence is added at the end of section 7: "Apart from the GAW and ESRL in-situ observations, also measurements from rural and remote surface air quality measurement sites are considered. The sites have to be carefully selected, because they should be representative for a larger area of the size of the model resolution. Furthermore, validated datasets are typically only available after a few years and only unvalidated data can be used for the near-real time evaluations. In particular, observations from the European Monitoring and Evaluation Programme (EMEP, http://www.emep.int), and the European air quality database "AIRBASE" (http://www.eea.europa.eu/themes/air/air-quality/map/airbase) are used to evaluate the reanalysis results. Also evaluations based on the USA "AirNow" observations (http://www.airnow.gov) are in preparation."

Section 8, page 1135 line 20 onwards: The large negative bias in the model extinctions is presumable partly due to the difficulty in estimating the source strength of the biomass burning emissions.

We agree, and added a line to section 8: "Many aspects influence the quantitative comparison, including uncertainties in the source strenght (fire radiative power observation and aerosol mass produced) uncertainties in the transport over several days, removal processes, resolution of the model and local representativity issues. Part of these modelling errors may have been corrected by the assimilation of the MODIS observations."

Section 9.2, 17: The under-estimation of NO2 columns may be partly due to underestimates in the emissions, but is probably also partly related to the 'low' model resolution, which unavoidably spreads emissions over a minimum of a grid box.

We agree and added a line to section 9.2: " The relatively low model resolution will lead to an underestimate of strong localised emission sources."

Section 9.3, page 1140, line 1,2: It would be helpful if these correlation coefficients could also be added to the caption of Figure 6.

This is added to the caption in the new manuscript.

Section 9.5, line 23 onwards: The bias figures quoted in the piece of text do not seem consistent with Figure 7. Also, does the term 'bias' imply the 'Modified Normalised Mean Bias' used and defined earlier or is this a different 'bias'? If it is the MNMB then units of % seem incorrect. Please review this text and figure carefully and ensure consistency / clarity.

The two panels of figure 7 will be replaced in the revised manuscript and will contain the MNMB.

A confusion may arise because in the text of Section 9.5 (line 23 onwards) the described bias figures use AERONET 2.0 level as a reference, while the bias is plotted using AERONET Level 1.5 as reference in Figure 7. Given that AERONET Level 1.5 has a bias of + 20% over AERONET level 2.0, this explains the apparent inconsistency. Even though it is mentioned in the caption of Fig.7, it is confusing for the readers. Note

also that the end of section 9.5 discusses dust, while the first part of the section deals with all aerosol types.

In the new manuscript we will use AERONET 2.0 in figure 7.

Section 9, page 1141, line 20: As for ozone, for future work please consider using the surface air quality networks for deriving aerosol composition measurements.

We have added a line to section 7: "Apart from ozone, also the aerosol composition measurements from these networks will be considered, as well as other compounds like CO and NO2."

Minor typographic corrections: page 1131, line 6: suggest replacing 'in-line' with 'on-line'. line 22: 'longterm' –> 'long-term' page 1144, line 24 'Ceilometer' –>'ceilometer'

Corrections included in revised manuscipt.

Interactive comment on Geosci. Model Dev. Discuss., 8, 1117, 2015.