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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.

Anonymous Referee #1

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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.

Specific comments ———— Section 5: The use of the mean-field metrics 'modi-

fied 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.

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.

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

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.

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.

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.

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.

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.

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'

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