

Interactive comment on “Tropospheric chemistry in the integrated forecasting system of ECMWF” by J. Flemming et al.

Anonymous Referee #2

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This paper describes the ECMWF Integrated Forecasting System for composition (C-IFS) model in the configuration that seems to be intended for operational use. This configuration uses the CB05 chemical mechanism. Comparisons are made to the IFS-MOZART and MACC reanalysis simulations in parallel to evaluation with observations of O₃, CO, NO₂, formaldehyde and SO₂. The model description is thorough, documenting well the physical processes that affect the composition (deposition, transport, lightning emissions) as well as the chemical mechanism. I think the paper could be improved, however, with regards to the evaluation with observations. I recommend publication after attention to my recommendations below.

p.7736, l.26: WRF-Chem is a regional model, but it is implied to be global in this sentence.

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p. 7755 and Table 2: North America ozone average of MOZAIC profiles and ozonesondes – It does not seem valid to average together all the stations of US and Canada. I would not consider Atlanta and Vancouver as having similar conditions at all. Tilmes et al. (ACP,2012, doi:10.5194/acp-12-7475-2012) shows significant differences among 4 ozonesonde sites spread across N. America, recommending against averaging them together for model evaluation. Please explain in more detail how this comparison was done. Was the model extracted for each site and then averaged? It seems it would be better to determine a model-measurement bias for each site, and then perhaps it is ok to average the biases.

p. 7757: The description of the MOPITT data set is not written very clearly. Was the Level 3 product used, or did the authors perform their own gridding to 1x1 degree? Presumably the model profiles were transformed, taking into account the a priori profile as well as the averaging kernel (this should be stated more clearly - I.21-23 seems a little confused - it is 2 operations). I.20: The increased sensitivity at the surface of the joint (NIR+TIR) retrieval is due to the inclusion of the NIR channel. I.24-26: I don't understand the point of this sentence.

p.7758: As with the MOPITT description, it is not clear if the authors performed some of the processing of the GOME-2 retrievals or if they are describing the product they used. Please clarify. Was any transformation of model profiles performed to account for the sensitivity of the GOME-2 columns to the true profile (i.e., averaging kernels or airmass factors)?

p. 7760: The bias in surface ozone in MOZART could be at least partially due to a recently documented error in the dry deposition calculation for all versions of MOZART, which led to reduced deposition velocities than intended, and thus over-estimate of surface ozone, as described in Val Martin et al. (GRL, 2014, doi:10.1002/2014GL059651).

p. 7761: It would be much easier to follow the arguments about the size of biases if the actual bias were plotted for each model. For example, the argument that "the bias

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of MOZ seems stronger over land" is hard to verify from these plots.

Fig. 10 is only mentioned in passing in between discussion of Figs 8 and 9. It should be put in order and discussed more completely.

p. 7762, l.18-19: The altitude levels that have the highest sensitivity for MOPITT should not have any bearing on the performance of the model, if the averaging kernels and a priori have been taken into account. I would remove this sentence.

Technical corrections: p.7741, l.16: Extraneous sentence fragment, apparently.

p.7746, l.3: African -> Africa

p.7751, l.11: is -> are

p.7754, l.4: with -> to, l.24: field->fields

p.7759, l.28: in -> and

p.7761, l.27: in -> into

Fig.3 caption: Arctic and Antarctic columns are switched.

Fig. 10 & 13 titles: "Meaned" not a word. What does "FC&OB: mean?"

Table S1 has C10H16, but table S2 uses TERP. In Table S1, IC3H7O2 and HYPROPO2 need more descriptive names.

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