

Interactive comment on “Evaluation of near surface ozone over Europe from the MACC reanalysis” by E. Katragkou et al.

Anonymous Referee #4

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This article proposes an evaluation of the MACC global ozone reanalyses over Europe. This model dataset is widely distributed and used in the community, in particular to serve as boundary condition of regional air quality models. It is therefore important to propose thorough evaluations, and the present paper addresses this need. Its submission to GMD is thus justified. Quite a few interesting features are uncovered in the study, but a few major remarks need to be addressed before it can be considered for publication.

Assimilation

The most significant and preoccupying result of the paper regard lower performances of MRE compared to CTRL. From Table 1 it is clear that FGE is better in CTRL for 5 out of 9 regions, and monthly correlation is better for 9 out of 9 regions, while the

C140

reanalysis outweigh the control only for MNMB (in 7 out of 9 regions).

The degradation of model performances when implementing the assimilation is a strong concern. A few hypothesis are indeed mentioned in the paper with regard to the larger weight of assimilation in the stratosphere / upper troposphere (P1087L20), but it does not explain why would it be detrimental at the surface.

A few statements also need to be modified in order to better reflect that assimilation is not improving the overall model performances: * P1078 L12 : "assimilation reduces the bias in near surface ozone" is not fully correct. * P1087 L1: reorganise the whole paragraph to start by making the case that assimilation is detrimental overall, before going into the exceptions where it improves model performances. * P1087L14: it is not objective to discuss only the improvements brought about by assimilation in Fig 3 while it is clear from that Figure that assimilation can also degrade performances in many instances.

P1081 L15 : please add a couple of sentence to explain which type of observation and chemical compounds are assimilated. It is not satisfactory to limit to an external reference, especially given that this reference is not available (even on GMDD) to date.

P1083: please confirm that none of the measurement used for validation are assimilated.

Springtime ozone maximum

The potential processes contributing to this springtime peak are introduced too late in the paper (Section 4 P1090 L15). Given the importance of this feature throughout the article they should be presented in the introduction (P1080 L 24-25), also discussing how the model is expected to capture these processes.

P1080 L25 : I am struggling with the logical link with the previous sentence, I don't see how assimilation can help in better understanding processes, please explain.

P1087L22: include CTRL in this section and corresponding Figures. The difference in

C141

temporal coverage is not a good enough reason, it would not be a problem if this figure would be limited to 2003-2010. It seems that other reviewers are sharing that concern.

L1089 L8: a link to the following discussion section should be included here since possible causes of the failure to capture the early springtime peak will be given there. There is also a risk of inconsistency when mentioning the findings of Inness (ACP 2013) here whereas the present paper seems to point out new causes for this model caveat.

P1092 L3: The potential role of the loose coupling between ABL and FT is very interesting. Please include a detailed formulation on how turbulent mixing at the top of the ABL is handled in the model and how it could be improved.

P1093L7: the more intense oxidation from NO_x to NO_z in BI and ME is also interesting, what could be the reason for this? You may consider adding a comparison with the model indicators proposed by Beekmann and Vautard (ACP 2010) in order to better document chemical regimes. Using such indicators would also allow drawing more robust conclusion than leaving production and loss analyses to further work in the conclusion (P1096 L1).

Seasonal cycles

It is surprising that the present paper is limited to comparison of monthly values and daily cycles, while the model is available on a 3-hr basis (P1082 L14). Validation of daily ozone variability was presumably deliberately left aside of this paper. Please explain why.

P1085L20: it is non-standard to compute correlations on the basis of monthly values for surface data, please repeat throughout the text that monthly statistics are used to avoid confusion.

P1086 L1: consider transposing Figure 2. The text discusses the amplitude of seasonal scores which would be much easier to grasp with one panel per region instead of one

C142

panel per season.

P1086 L26: it is likely that the correlation is influenced by the amplitude of the cycle, please compare with a rank correlation

Minor remarks

P1079 L20 : the correct acronym is "AQMEII"

P1079L21 : "it is therefore useful"

P1080L8 : "of these precursors"

P1080L17 : Vestreng et al. 2009 does not address trends in peak ozone

P1081 L24: define what would be a "clean" control

P1082 L10: what is the temporal coverage of the stations selected here, did you limit the study to stations covering the whole period?

P1086 L5: the coloured point is not next to the boxplot for Fig 2.

P1095 L23: wrong indentation.

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C143