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Geoscientific Model Development Discussions
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Interactive comment on "Evaluation of near surface ozone over Europe from the MACC reanalysis" *by* E. Katragkou et al.

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We thank the reviewer for his/her comments. We responded to all of the points raised and changed the revised manuscript accordingly.

R4.1. "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 CTLR for 5 out of 9 regions, and monthly correlation is better for 9 out of 9 regions, while the reanalysis outweight 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."

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We believe that an important outcome of this evaluation study is the confirmation that the modeling system provides an adequate representation of near surface O3. Model weaknesses are identified and suggestions for future improvements are provided. It has been demonstrated that the assimilation (correction of stratospheric and total O3 column) in many cases corrects the surface O3 bias. The lower temporal coefficients (R) in the control run versus the MRE, is not an inherent problem of the modeling system. We attributed the deterioration of R the data assimilation procedure, related to the MLS bias correction, described in detail in the paper of Inness et al., 2013. The bias correction of MLS data, has caused drifts in the tropospheric ozone concentrations between August 2004 and December 2007, an issue which have been tracked down and alleviated after year 2008 of the MRE. After 2008, R improves. Comments are inserted in the revised manuscript. Figure 1 shows that temporal correlation of the MRE increases after bias correction (2008-2012).

R4.2 "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.

We modified the text in the revised manuscript to be in agreement with Table 2 (Table 3 in the revised manuscript). “Assimilation reduces the bias in near surface ozone in most of the European subregions, with the exception of the British Isles (13% in the MRE and 7% in the CTRL) and the Iberian Peninsula (15% in the MRE and 10% in the control) “

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.

We do not believe the impact of assimilation is overall detrimental, on the contrary. We expanded the discussion on the impact of assimilation more in detail and we discuss more thoroughly the issue of MLS bias correction and the misbehavior that has caused

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in surface ozone, an issue that has been alleviated after the year 2008 in the reanalysis. The manuscript is modified accordingly.

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.

We agree with the reviewer: The issue of the deterioration in the temporal correlation is more thoroughly discussed in the revised manuscript.

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.

Table 1 has been added in the revised manuscript, along with some additional information in section 2.1 with respect to assimilation.

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

We do confirm. A note has been made in section 2.2

R4.2 "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."

We agree with the reviewer, and so we move this part of the discussion in the introduction.

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.

This sentence is removed and a new small paragraph is added in the introduction section, to summarize the structure of the presented work.

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

In the current manuscript we provide the basics of the comparison between the ctrl and the reanalysis (Fig 3 and Table 2). We provide a document for further reading, which is an extended report on the impact of assimilation on surface ozone for the 2003-2010, available as a VAL technical report, Deliverable D84.2 “Validation report on the Comparison of surface ozone in the global (2003- 2010) and regional reanalysis (2011) over Europe”. We note that the basic findings of our analysis are robust and do not change, when the analysis is limited to the 2003-2010. We prefer therefore, to have the analysis cover the whole time period (2003-2012).

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.

A link to the following section is provided : “This shortfall of MRE to capture the early spring peak has been also noted by Inness et al. (2013) and it is further discussed in the following sections”

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.

It is not so easy to reach any definite conclusions on the issue of coupling ABL and FT. Transport in the MRE is a mixture between the MOZART diffusion scheme and the IFS diffusion scheme. On top of it, there is data assmilation and the prescribed vertical correlations (see Inness et al. 2013), which also has an influence on the profiles.

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The vertical transport is treated more consistently in the new on-line integration (C-IFS) (presented by Flemming et al., 2015) than in the coupled system IFS-MOZART, which was used in the MACC reanalysis.

P1093L7: the more intense oxidation from NOx to NOz 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).

The differences in local photochemical ozone production at BI and ME versus IP are consistent with the chemical regime indicator analysis for near surface ozone over Europe by Beekmann and Vautard (2010), who defined three particular regions: a) the region in North-Western Europe with a pronounced VOC sensitive regime (1W–6 E, 50 N–53 N), b) the Mediterranean region (6W–20 E, 38 N–43 N) with an average NOx sensitive chemical regime and c) Northern-Eastern Germany (9 E–14E, 50 N–54 N) which is a transition region between both regimes. Comparing this chemical regime analysis with our selected sub-regions BI, ME and IP we note that BI and ME sub-regions are a mixture of a VOC sensitive regime and a NOx sensitive regime while IP is a NOx sensitive regime.

We added this discussion in the manuscript.

R4.3 "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."

The metrics were calculated twice. The first time with monthly mean values and the second time using daily mean values. The metrics are most of the times improved when the monthly values are used, but the differences do not alter our main findings, and

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therefore we prefer to keep the manuscript concise, including only the monthly analysis. The results based on daily values are presented in the technical report Deliverable D84.2 “Validation report on the Comparison of surface ozone in the global (2003- 2010) and regional reanalysis (2011) over Europe” in Table 1 (provided as Supplementary file in this response).

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.

We add in section 2.3 that R is calculated out of mean monthly data.

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 panel per season.

We prefer to keep fig in the current orientation format to be in accordance with the rest of the figures.

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

In our manuscript we calculated Spearman correlation coefficient. Additionaly, we provide R Kendall as a metric for rank correlation. Results are very similar (Figure 2 of this review)

Minor remarks

P1079 L20 : the correct acronym is "AQMEII"

Done

P1079L21 : "it is therefore useful"

Done

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P1080L8 : "of these precursors"

Done

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

The sentence was slightly changed, to cite correctly the work of Vestreng, et al. “Although a number of measures aimed at reducing NOx and VOC emissions have been effective in reducing concentration of precursor species (Vestreng et al., 2009) and peak ozone values in Europe (EMEP/CCC-Report 1/2005) . . ..”

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

Done. A full definition of the clean control is now provided, according to the Inness et al, 2013 paper.

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

Yes, as mentioned in the same section, we used only stations that fulfill the criteria of the Jolly-Peuch classification and are available for the whole time period examined, with 75% data availability for near surface ozone.

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

The sentence is corrected to: “The colored point on each box indicate. . .”

P1095 L23: wrong indentation.

Done

Please also note the supplement to this comment:
http://www.geosci-model-dev-discuss.net/8/C1134/2015/gmdd-8-C1134-2015-supplement.pdf

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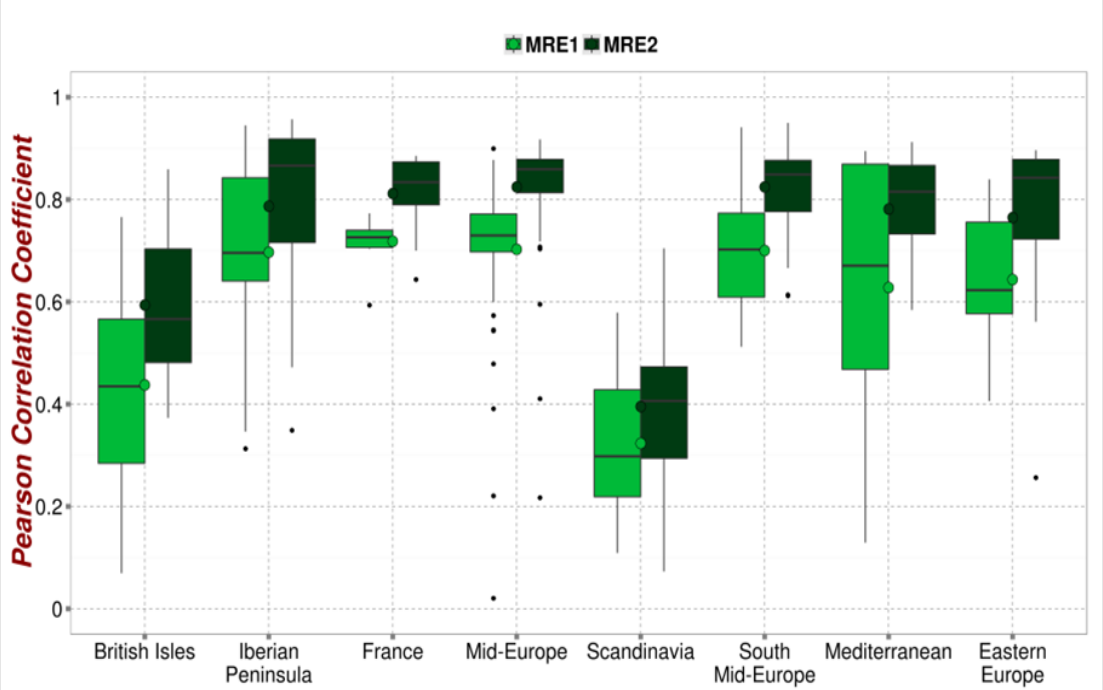

Fig. 1. Whisker plots for surface temporal correlation for MACC reanalysis averaged over 2003-2007 with bias correction (MRE1, light green) and over 2008-2012 without bias correction (MRE2, dark green)

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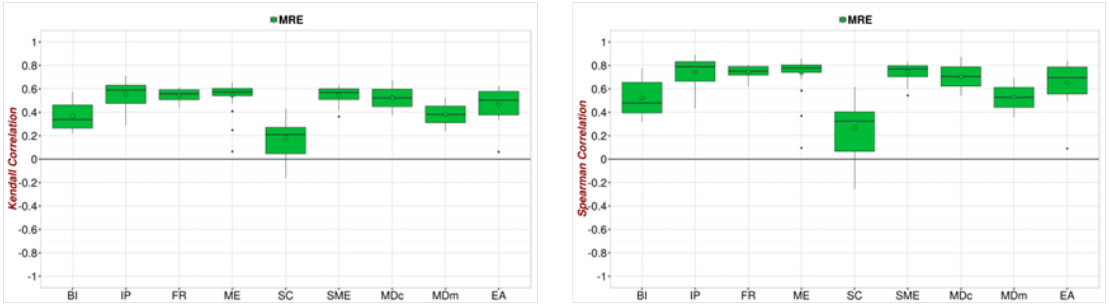

Fig. 2. Calculation of R_kendal (left) and R_spearman (right) for the different subregions

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