

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

Anonymous Referee #3

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Review of “Evaluation of near surface ozone over Europe from the MACC reanalysis” by Katragkou et al, in GMDD.

This manuscript proposes a detailed evaluation of the near surface ozone from the MACC reanalysis (MRE), which corresponds to the use of ozone observations assimilated in a CTM coupled to the GCM of the ECMWF model. This evaluation is very clearly presented, with a logical and systematic approach that makes the manuscript pleasant to read. The authors indeed give an analysis of annual and diurnal cycles, seasonal behaviours, combined to a geographical areas analysis, to draw a general representation of the near surface ozone MRE. This analysis is moreover based on usefull and widely used metrics in the community. The authors point good and not-so-good behaviours of the MRE, and, in the last part of the manuscript, propose some

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explanations for the discrepancies between the reanalysis and the observations, notably by looking at NO_x cycles and ozone above the boundary layer. This evaluation is useful, and fits the scope of the journal GMD, since model development has to include its evaluation.

For all these reasons, I recommend this manuscript for publication in GMD, with minor revisions.

However, I have some questions and remarks that I would like to be taken into account before publication :

General comments

1) All along the manuscript, the authors should be more precise, both in their qualification of the results and the terms they use in general. For instance, in the abstract, what is 'the annual overall error' accounting for? What is the value of the 'average correlation' (p 1086, L1) etc . . . There are many points like these, I will go back to these in the specific comments 2) The use of the CTRL simulation was very promising but is finally disappointing because too short. In particular an explanation of the drop of the correlation from CTRL to MRE in Mediterranean marine stations and in Scandinavia would be expected in the discussion part. Finally, either the CTRL simulation should not be used at all, or compared to MRE all along the manuscript, with, if necessary, an adaptation of the time period to be analysed. 3) Even if the paper describing precisely the MRE is referred to I would like the assimilation process to be described more precisely. In particular, the time-steps of the assimilation, its vertical extent, and the chemical species that are assimilated. Only one sentence (p1087, L17-18) mentions that point : this is not enough. Moreover, even if it is implicit, the authors should explicitly mention that observations they use for this evaluation are independent from the assimilated ones. 4) Figures and captions are generally too small (however they are in general very informative). 5) I would appreciate a conclusion that would give more perspectives to this work.

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Specific comments

p1078 L9-10 : define the “annual overall error” and “on average” (spatial, temporal, both?) p1082, L20-25 : can you give an estimation of the impact of taking real-altitude of the station instead of surface ? p1083, L28 : the precision of the ozonesondes is no more referred to hereafter. In 4.1, you should recall it to the reader and comment the results correspondingly. P1085, L8-9 : “the confidence interval . . . subregion” : this sentence is unclear to me. p1085-86, L20-1 : I do not agree for having British Isles and Scandinavia at the same level. Their correlations are really different. I would put together BI and MDm (0.51 and 0.54) and separate Sc (0.26). This is implicitly what you mention later. (L26-27), so this sentence could finally be removed. P1086,L10-11 : I suppose the numbers you give (40:28% etc..) correspond to the mean value of the data. It should be specified, since the median could also be taken into account and give significantly different results. P1087,L9-10 : for a correlation that drops from 0.74 to 0.49, ‘a slightly reduction’ is not an appropriate description. Moreover, how do you explain that drop? P1087,L23-25 : The terms “cycles have differences in the shape”, although it is true, are too imprecise. This differences should be numerically estimated through correlation, to make sure the analysis is objective. P1088, L8-12 : you should mention that this point will be discussed later. P1089, L18-19 : “the MRE captures quite well. . .” : once again, this should be more precise. P1090, L12 : the word “ozone” is lacking between “near surface” and “between”. P1090, L13 : “It” is lacking before “is known”. P1091, 4.1 : this subsection would really benefit from a comparison to CTRL simulation. P1093,L1-4 : “The amplitude. . . , which indicates that we have more intense local oxidation”. I find this interpretation too rapid. I agree that photochemical processes will play an important role. But a too active convection, or a bad representation of emissions could for instance lead to the same behaviour. P1094,L12 : a word is missing at the end of the line.

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