

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

Anonymous Referee #1

Received and published: 3 March 2015

This manuscript provides a detailed evaluation of the surface ozone in the MACC global reanalysis (MRE) compared to surface ozone observations over Europe from the EMEP and AirBase observations. The manuscript introduces several interesting metrics for model evaluation and represents one of the more thorough and self-consistent evaluations of both seasonal and diurnal cycles in modeled ozone. Evaluation of the MRE is important as the reanalysis is intended for use as boundary conditions for other atmospheric chemistry modeling studies. The paper is within the scope of GMD because, although not directly reporting new model developments, the model evaluation process is an important part of model development. The analysis reveals some interesting differences between the MRE and observations in both seasonal and diurnal cycles. The authors speculate about possible explanations for the discrepancy between MRE and observations, but do not settle on a firm conclusion. In spite of this,

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the manuscript does move the field forward and points to directions of future work, so I recommend that the manuscript be published with minor revisions. The writing is generally clear. In particular, the authors do a good job at clearly describing the different situations in the large number of geographic regions that are being compared. However, there are a number of unusual turns of phrase or word choices that should be revised or dealt with during copy-editing.

Major comments:

Throughout the manuscript, there is thorough discussion of both transport and chemistry, but none of deposition. Deposition is notably missing from both the Introduction and Discussion sections. Could deposition play a role in explaining the discrepancies between the MRE and observations? The model is sampled at vertical levels other than the surface to match altitude with observing sites, but this will also impact deposition. This issue should be discussed and the offset between surface and above-surface grid boxes should probably be evaluated.

The failure of the MRE to capture the spring peak in ozone that is noted by the authors requires further exploration, but it is fine with me for the authors to present it as a question for future work. One puzzling aspect is that many models do capture a springtime maximum in ozone, and particularly with the assimilation of column ozone observations, I would have expected long-range transport contributions to spring ozone [Parrish et al., 2013] to be captured.

Some further justification is required in defining the subregions that Europe is broken up into. The authors argue that “Overall, the annual cycles of the observed data reflect the specific subregional characteristics...”. However, there are three counter-arguments to this:

1) For some regions, the seasonal observed cycle varies substantially within the subregion. For example, in the Scandinavian subregion, the sites in the Baltic states and Denmark peak in the summer, while those on the Fennoscandian peninsula peak in

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the spring. This could be complicating the analysis of the offset in seasonality between modeled and observed cycles in the Scandinavian subregion.

2) All of the modeled seasonal cycles shown in Fig. 4 look much more sinusoidal than the observed seasonal cycles, so while the model is doing a reasonable job of the capturing the magnitude of the annual mean and seasonal amplitude, the shape and phase of the seasonality are not captured.

3) The Mediterranean sites are broken into continental and coastal sites, but the other regions are not. There is likely a distinction in the observed seasonal and diurnal cycles between coastal and continental sites for the British Isles and Central Europe

Specific comments:

1078L9: "Annual overall error" is a vague term in the abstract.

1080L7-11: Discussion of sources, chemistry, transport, but no discussion of deposition.

1081L19-21: In addition to stratospheric and column ozone, the MRE also appears to assimilate satellite observations of other relevant gases (CO, NO₂) that will impact ozone chemistry [Inness et al., 2013].

1081L22-28: While the explanation of the configuration for the control run is clear, I am unclear on what is meant by the "control run is not a "clean" control analysis experiment. . ."

1082L10-12: Is there a literature reference for the choice to use background stations for comparison to coarse-resolution model output?

1087L8-11: Why does assimilation make the seasonal cycle worse in some areas?

1092L12: "Other PAN homologues (PANs)" I believe should be abbreviated APNs (standing for acyl peroxy nitrates).

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1106: Figure 2 caption. Describe the box and whisker structure in the figure caption in addition to its description in the text on page 1086.

L1110: Figure 6. If possible, color coding the shaded envelopes to be consistent with the line colors would help to improve the readability.

L1112: Figure 7. 24 subplots is too much for one figure! The profiles become very hard to read when that small.

1113: Figure 8 caption. Change "near surface ozone at 700 hPa" to "lower tropospheric ozone at 700 hPa" to distinguish from the "near surface" observations discussed throughout the rest of the manuscript.

Technical corrections:

Examples of language issues:

1079L12: Change "year-long experience" to "many years of"

1080L18: "(even at near surface)" change to "even near the surface"

1086L4-5: The line indicating the median in Fig. 2 is horizontal, not vertical.

1090L9: "and the fail in MRE. . ." change "fail" to "failure"

1090L10: Add "It" before "Is known that. . ."

Interactive comment on Geosci. Model Dev. Discuss., 8, 1077, 2015.

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