Geoscientific Model Development Discussions

Geosci. Model Dev. Discuss., referee comment RC2
https://doi.org/10.5194/gmd-2022-197-RC2, 2022
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Comment on gmd-2022-197

Anonymous Referee #2

Referee comment on "Long-term evaluation of surface air pollution in CAMSRA and MERRA-2 global reanalyses over Europe (2003–2020)" by Aleksander Lacima et al., Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2022-197-RC2, 2022

The authors present a thorough evaluation of two global reanalysis products with focus on air quality aspects over Europe, in terms of quality of the climatology, and its trends.

For this they use observational data from the EEA AIRBASE and AQ eReporting databases, for which a thorough filtering and quality assurance has been setup. Based on this they find that the CAMS Reanalysis overall outperforms MERRA, although significant biases remain, for which a further bias-correction methods should be developed to make the data most useful.

This manuscript is well written, and well fitted for publication in GMD, after consideration of a few comments:

- Table 1. The study by Huijnen et al. (GMD 2020) is missing in this list, even though they report to some extent on the performance of surface ozone in the CAMSRA, even with focus over Europe. Is there any reason why it is not needed to include it here, or is it an oversight?
- l. 85 Please include a reference to the aerosol scheme used in the CAMS Reanalysis (Morcrette et al., I believe)
- l. 90: "Meteorological observations and fields are taken from ERA5" while this is probably very close, please note that the CAMS Reanalysis applies its own assimilation of meteorological variables, i.e. it is not identical to ERA5.
- Being to some extent involved in the generation of the CAMS Reanalysis, I have some small comments to better refer to the configuration specifications as described in Sec. 2.1.1, see also below
- Appendix B: This is useful information, which could be taken over by others in the community. It might be useful if you can additionally better specify flags 110-111: How are these 'scientifically feasible values' defined exactly?
- Line 153: "urban": While the global reanalyses are known to have difficulties to represent urban-type conditions, would it make a difference to exclude the "urban"

sector from your evaluation? I wonder if particularly for NO2 and O3 (but possibly also for PM) this could still result in significantly different, and more relevant, performance statistics? (although I can see that for your future applications you exactly require knowledge about the bias correction wrt urban stations, probably).

- Along these lines, it could be useful if the authors include (if not now, possibly in future) a correction factor for interference in the in-situ observations of NO2 for PAN and HNO3, see also a discussion of this impact in Poraicu et al. (GMDD 2022), and references therein.
- Figure 1. There is a considerable change in the observational network over time. Do you have any indications to what extent this has impact on the computed trends? Furthermore, a suspicious peak in the number of observations appear around the end of 2012 for various compounds. Do you have any understanding what has caused this?
- Sec. 3.1 the authors now focus on monthly mean values. It would be nice and interesting to try also other metrics (e.g. biases, and trends in summertime, daytime ozone), particularly as this is more relevant for health-related applications. But I can understand if that is out of scope of the current work.

Technical comments

- The first couple of sentences of the abstract are of course true; on the other hand to my taste this is possibly a bit over-the-top, and high-level to motivate the work that you present here, that is not needed. It would work if you are a bit more modest here.
- l6.: "…they do not integrate surface measurements…" :I'd suggest to include the word 'generally': it is true that current, specified global reanalyses do not include surface obs, but it is not a rule, I'd say.
- l.71-72: I understand it's easier to simply talk about concentrations, but when reporting O3 in units ppbv this is really 'volume mixing ratio' - some further clarification at this point might be good
- l. 94: "The bias…" I think it is more accurate to write something like: "The biases present in the different AC satellite retrieval datasets.."
- l 97. "separate chemical compounds" better write "separate aerosol compounds"?
- Table 2, "Assimilation system: IFS Cycle 42r1 4D-Var ; Meteorology: ERA5" better write here: "Assimilation system: 4D-Var ; Meteorology: IFS Cycle 42r1"

References

Poraicu, C., Müller, J.-F., Stavrakou, T., Fonteyn, D., Tack, F., Deutsch, F., Laffineur, Q., Van Malderen, R., and Veldeman, N.: Cross-evaluating WRF-Chem v4.1.2, TROPOMI, APEX and in situ NO2 measurements over Antwerp, Belgium, EGUsphere [preprint],

https://doi.org/10.5194/egusphere-2022-882, 2022.

Huijnen, V., Miyazaki, K., Flemming, J., Inness, A., Sekiya, T., and Schultz, M. G.: An intercomparison of tropospheric ozone reanalysis products from CAMS, CAMS interim, TCR-1, and TCR-2, Geosci. Model Dev., 13, 1513–1544, https://doi.org/10.5194/gmd-13-1513-2020, 2020.