EGUsphere, referee comment RC2
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Comment on egusphere-2022-365

Anonymous Referee #2

Referee comment on "Comparing Sentinel-5P TROPOMI NO_2 column observations with the CAMS regional air quality ensemble" by John Douros et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-365-RC2, 2022

John Douros and co-workers report on comparisons between TROPOMI NO2 column observations and results from the 7 air quality models which are currently operational within CAMS, providing forecasts and analyses over Europe at 0.1x0.1 degree resolution. The comparison shows a reasonable agreement during summer, but a substantial (factor of about two) model overestimation in winter. The use of high-resolution a priori profiles from the CAMS model ensemble (instead of the global 1x1 profiles) in the tropospheric NO_2 column retrieval from TROPOMI results in higher retrieved columns over emission hotspots by about 30%. The authors further performed validation of the new satellite TROPOMI NO2 dataset using remote sensing column measurements and found that despite the overall overall bias reduction compared to the operational TROPOMI product, the new dataset is not able to close the large gap between observed and modelled NO_2 columns in wintertime.

The manuscript does not include significant advances in modelling and has quite limited novelty. It uses pre-existing models and their outputs which are routinely available. However, the study proposes an alternative TROPOMI NO2 dataset over Europe based on high resolution model profiles which could be useful for the community. The method used for this derivation has been already applied in previous studies. I find the comparison of the
data with the output of the 7 models interesting, in spite of the fact that the reasons for the large mismatches are not investigated in the manuscript. The scientific approach and the methods are not new but they are valid and widely used in the literature. The results are discussed in a balanced way,
although in most instances the discussion is only qualitative. The writing is not always very precise. The language should therefore be improved in the revised version. Some references are incomplete or not defined, and additional references are needed. I could recommend publication after the following points are adequately addressed.

Comments:

l.8: "7 up to 11 models". Not precise and misleading since the manuscript presents only results from 7 models.
l.13: remove "quantitative"

l.13: provide information (e.g. bias, correlation) about how close this agreement is
l.14: 'significant discrepancy', provide numbers
l.25-28: here again provide figures of the bias reduction and correlation obtained from this validation
l.34: 'values above the surface which are otherwise very scarce', replace by 'measurements at the surface which are very scarce'
l.36: read 'at kilometer scale'
l.39-44: This information does not seem relevant for this paper.
l.50: What are the CAMS systems? I would replace by 'CAMS makes'
l.51: Inness et al. 2019b is not defined
l.53: 'consistent' appears twice in the same line, replace by "to daily (re)analyses of concentrations and emissions which are consistent with..."
l.57: changes are not sharp for pollutants other than NO2, see https://doi.org/10.1029/2020GL091265, https://doi.org/10.3390/atmos12080946, DOI: 10.1126/sciadv.abg7670. I suggest to drop 'sharp' from the sentence and add some more references.
l.58-60: poor wording, Replace 'dedicated studies have been launched to study' by 'dedicated studies have been performed to investigate'
l.62: near daily basis
l.72: TROPOMI appears twice, replace 'measurement series' by 'measurement period', mention that TROPOMI NO2 is derived using the global TM5-MP profiles
l.73: mention clearly the horizontal resolution of the CAMS and the TM5 models
l.71-75: improve the clarity
l.81-82: remove 'very small', replace 'very large' by 'high'
l.84: provide references for your statement
l.85: remove 'the paper by' here and throughout the manuscript
l.85-93: check your references, for example Eskes et al., 2021a is missing
l.97-98: 'to force the stratosphere to be consistent with TROPOMI', weird statement
l.108-114: 'do not have a large impact', 'rather stable', 'considerable change', provide quantification
l. 121: MAXDOAS or MAX-DOAS, not both
l.120: mention that the Verhoelst et al. comparisons do not account for averaging kernels
l.124: reference missing
l.135: could you mention the impact of the new version v2.2 described in https://doi.org/10.5194/amt-15-2037-2022 ?
l.146-48: link not accessible (and too long)
l. 153: correct typo
l.164: 'have', not 'has'
Figure 1: Acronyms are not explained in the caption.
l.179: not necessary
Section 4.1: I find this section describes well-known methods in a confusing way.
l.225-26: avoid repetition of the word 'gridded' in the same line
Sections 5.1, 5.2, and 5.3 could be merged, all of them consist in briefly presenting the figures 5-8
l.325: I could not find Huijnen et al. 2010b in the list
Table 3, add additional columns with the ratio S5P and S5Pcams and CAMS-RG-A. Or add another table. This would help your discussion.
l.455: did you use 8 or 9 MAX-DOAS stations for validation? In the abstract you mention 8
Section 6.1, the discussion is again only qualitative. For example, 'CAMS is higher close to he suface': higher by how much?
Fig.15 inset statistics are too difficult to read
Fig.16: Is S5Pcams and S5P-RG the same thing?
l.500: 'this is not done here', improve the wording
l.564: '10% column enhancement', is this on average?