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Geoscientific Model Development Discussions

Interactive comment on "Quantifying uncertainties due to chemistry modeling ndash; evaluation of tropospheric composition simulations in the CAMS model" *by* Vincent Huijnen et al.

Anonymous Referee #2

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This manuscript evaluates tropospheric composition simulations in the CAMS modelling system and quantifies uncertainties related to different chemical schemes. It is well structured and written and illustrates original and interesting results for the . CAMS modelling system. I suggest acceptance of the manuscript for publication after taking into consideration the following comments.

Main Comments 1) I guess that the simulations were carried for the year 2011 but I think the authors should describe in Section 2.3 which was the time period that the simulations were carried out. 2) The authors mention that the averaging of large number of measurements over space and time partly solves the problem of interannual

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Discussion paper

variability (lines 273-275 in page 11). Can this dataset of Emmons et al. (2000) be representative to compare with the CAMS simulations for the year 2011? I understand the uniqueness of this dataset but could the authors clarify this issue and discuss the uncertainties and the weaknesses of this comparison? 3) I would suggest the authors to provide a short description of the method used to calculate the weighted values of bias and correlation in Table 4. 4) The authors write in line 375 (page 15) that " CBA is the only model version to deliver a satisfactory bias" . Is this a robust conclusion? What is statistically satisfactory? Looking Table 4 I see that in some species CBA bias is smaller than in other schemes, in some other species the biases are comparable and in other species the CBA bias is worse. 5) In the evaluation of ozone the authors conclude that "overall, the evaluation at individual station provides reasonable agreement between model simulations and sondes". How these evaluation results compare with other ozone evaluation studies which were based on MACC and CAMS products (e.g. Inness et al., 2015; Katragkou et al., 2015; Akritidis et al., 2018). I think this conclusion could be also supported by these studies. 6) In lines 448-449 (page 20) the authors write "Approximately half of the CO burden is directly emitted, and the rest formed through degradation of methane and other VOC's". Please add a relevant reference. 7) On how many data points (and years) the temporal correlations shown in Figure 10 are based? 8) In lines 526-528 (page 26) it is written "The vertical profiles (see Figure 13) are strongly biased (e.g., SONEX, Newfoundland and PEM-Tropics-A, Tahiti), with positive biases occurring at the surface and negative in the free troposphere." Could this result also related to inadequate outflow from the atmospheric boundary layer (ABL) to the free troposphere (FT) and hence to model weakness in ABL-FT exchange? 9) The authors refer to correlation R (that span from -1 to 1) but showing R2 which practically describes the explained variance. Although this is not crucial in the discussion it could propagate a misunderstanding on these statistical parameters when the article is published. I would suggest to modify this accordingly. 10) Generally, I think that the discussion in model difference is rather technical and I would suggest the authors to discuss also the possible scientific reasons for discrepancies among the simulations with different chemical schemes for the different chemical species. Minor comments page 13, line 348: should rather be "relative shorter" instead of "relative short"

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