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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Summertime stratospheric processes at northern mid-latitudes: comparisons between MANTRA balloon measurements and the Canadian Middle Atmosphere Model" *by* S. M. L. Melo et al.

Anonymous Referee #1

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GENERAL COMMENTS

This manuscript reports on the evaluation of modeling results obtained by Canadian Middle Atmosphere Model (CMAM). The evaluation relies mainly on comparisons with a large set of standard ozonesonde measurements of ozone and temperature profile and on profiles of 4 long-lived tracers measured by balloon-based Fourier Transform spectrometry during 4 late summer campaigns.

The paper starts with clear and ambitious objectives. A main objective is to document

current capabilities of CMAM to reproduce on an average ozone, temperature and tracers measurements obtained during the 4 MANTRA campaigns. The part addressing the value of the vertical diffusion coefficient is particularly sound and convincing. However, the presentation and discussion of CMAM/MANTRA comparison results lack of rigor. The main problem is that there are too many qualitative statements like "agree very well" and "very good agreement" without any quantitative statement, any reference and any further explanation on this high degree of satisfaction. E.g., in Section 4.2 (page 9), the authors state that "CMAM N2O agrees very well with MANTRA observations". Looking at Figure 5, we see up to 24km an agreement of about 4-9% which, if the 4% high bias of tropospheric N2O is taken into account, reduces to 0-5%. Is that fine for N2O? At higher altitudes, this agreement degrades to +25%. Is that still good? And what about +- 50% observed for HNO3 and +100% observed for CH4 and HCl, while recent ACE-FTS validation results show agreements better than 25% up to 50km for CH4 and 10% for HCl? Are there any fundamental differences between CMAM outputs and satellite measurements (e.g. ACE-FTS) that could explain such a large difference in agreement? Could changing trends in HCl not play a role in the apparent disagreement between CMAM climatological results and real, punctual measurements? References to other model studies, to relevant satellite validation papers, or at least a few lines of text, should be added to explain why the reported agreements are considered as good.

The part on correlations among long-lived species (second objective) increases the confidence in the model, although the discussion of results is a little bit short.

In Section 5 (discussion), we would expect a discussion of results obtained in previous sections. Actually, there is only a short paragraph on the vertical diffusion coefficient, nearly nothing on CMAM/MANTRA comparisons and the tracers correlations, but a long discussion on the possibility to have observed "fossil" debris of the polar vortex. The latter discussion is based mainly on results published elsewhere. This review of existing literature is interesting to some point, but the CMAM/MANTRA results reported

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in this paper do not bring sufficient information to conclude on this subject. Figure 4 even show that CMAM is not able to reproduce this event satisfactorily.

The general feeling that this paper gives is that excellent material is at the disposal of the authors, but that the presentation of the comparison results should be closer to scientific standards (more rigorous, more quantitative), and that discussion of the results needs to address more consistently the claimed objectives of the paper.

Finally, some sentences are somewhat long and sometimes difficult to understand at first glance.

———————————————— SPECIFIC COMMENTS

Section 1, line 2: I would suggest "anticipated" recovery of ozone.

Section 1, paragraph 2, sentence 2: This sentence lacks of balance between the description of polar and mid-latitude ozone depletion. Polar loss estimates should be quantified and the trend character of mid-latitude depletion highlighted.

Section 2.2, sentence 3: Do the Canadian standards differ from WMO standards for ECC preparation and analysis? A reference or at least a description of details controlling the measurement error (manufacturer, sensing solution, pump efficiency correction type) would be appreciated.

Section 2.2, sentence 5: Please write that ozonesondes "measure" in situ ozone rather than "produce". This misleading statement also appears in Section 4.2, paragraph 4, sentence 1.

Section 2.2, last sentence: 5% error estimate for ozone: Is this a systematic or random error? Instead of, or in addition to, Davies et al., 2000, please adopt as reference "Smit, H.G.J., and Straeter, W., Juelich ozone sonde intercomparison experiment 2000 (JOSIE-2000), World Meteorological Organization Global Atmospheric Watch (WMO-GAW), TD N. 1225, 2004." which is more complete, more general and more accessible to the widest community.

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Section 2.2: Although used in the paper, there are no error estimates for RS-80 temperature measurements: precision of 0.1 K, and pressure-dependent accuracy: 0.2 K from the ground up to 50 hPa, 0.3 K from 50 to 15 hPa and 0.4 K below 15 hPa. Proposed reference (for RS-90, but including RS-80 results): "Luers, J., K.: Temperature Error of the Vaisala RS90 Radiosonde, Journal of Atmospheric and Oceanic Technology, 14(6), 1520-1532, 1997."

Section 3, paragraph 1: Description of temporal characteristics of CMAM output is missing. Without this information, it is nearly impossible to understand what has been done in Paragraph 2 of Section 4.1 and subsequent discussion.

Section 3, paragraph 3: This paragraph would preferably start with a list of major factors generating inter-annual variability. This would help the reader understanding why CMAM might underestimate inter-annual variability.

Section 4.1: It is really hard to understand what has been done here. What does a "model day" mean?

Through suitable scheduling of the MANTRA campaigns, near the turnaround of stratospheric winds, great care is given to reduce dynamical sources of variability, thus comparison errors associated with spatial and temporal mismatch of the measured information. Moreover, ozonesonde data sets are averaged with a view to mimicking the climatological nature of the CMAM output. This is a strong point of the methodology. One could regret that such a care is not given here to vertical mismatch errors: ozonesonde data acquired at 100 m resolution are simply interpolated onto the CMAM grid points. This introduces likely large comparison errors that might explain the significant scatter observed in the comparison plots. Smoothing vertically ozonesonde data with a low pass filter close to the CMAM vertical resolution might help.

In paragraph 2 of Section 4.1, looking at T and O3 correlation plots in Figure 2, the authors conclude to a good overall consistency between model and measurements. Looking at the scatter of the correlation plots, which seems to me to exceed the ECC

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and RS-80 error estimates for O3 and T, I have some concerns with this conclusion. Maybe the authors could comment on this. They should include error budgets in the discussion in order to support their conclusion.

The authors also note that temperatures above 20 km tend to be below the 1:1 line. I suggest that they rather write that CMAM overestimates or underestimates MANTRA observations, preferably with an estimate of this under/overestimation. Note that in Figure 2 the 1:1 line has a wrong position: it should not pass via (260:250).

How do the authors justify the choice of the altitude ranges: 10-20 and 20-30 km? Is that an arbitrary choice, or is there something happening at 20km that justifies a separation of the results ?

In Figure 5, horizontal scales for CH4, HNO3 and HCl are rather coarse: up to 100% and even 200%. This is fine to see large deviations pointing to real problems, but much too coarse to discuss the quantitative agreement with correlative data at altitudes where it reaches more classical values. A suggestion would be to add, for each species, on the right of existing graphics, the same results but now with a a reduced scale allowing closer look at the quantitative results.

Section 4.2, line 5: How many CMAM profiles are used in the calculation of daily averages?

Section 4.2, line 9: What does model variability mean?

Section 4.2, paragraph 2: Are the diffusion coefficient and the vertical resolution the sole changes between CMAM-WMO and CMAM-V7 that might have first-order effects on the tracer profiles?

Section 4.2 and 4.3: Please add relevant references to ACE-FTS, ATMOS and HALOE.

Section 4.2: More details on the HALOE climatology would be needed to understand how far this data set can be suitable for the current study. E.g., are the first years of UARS operation (post-Pinatubo era) included in this climatology?

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Section 4.2, paragraph 4, line 12: Please quantify this "very good agreement" with HALOE.

Section 5: At the end of the paper, the reader would expect a conclusion, even short, recalling the three objectives of the paper and explaining to what extent they have been achieved.

——————————————— TECHNICAL CORRECTIONS

- Affiliations: wrong numerical position of affiliation indexes 7, 4 and 5. - Abstract: Sentence 1 is much too long. Please cut and rearrange. - Page 2, Section 1, paragraph 1, last sentence: please adapt standard reference format; complete reference should not appear in the text. - Page 3, paragraph 2, line 4: "reportED" - Page 7, paragraph 2, line 2: "measurementS" - Page 9, paragraph 1, last line: remove parenthesis - Page 11, paragraph 2, line 1: please remove "HAVE" - References, general: please review reference formats, adopt agreed standards (e.g. "Geophys. Res. Lett." and "Ann. Geophys.") and complete with right page numbers. - References, Avallone et al.: "Comparisons" instead of "Xomparisons" - References, Davies et al.: editor, page numbers? - References, Komhyr: remove accent in "Geophys." - References, Murcray et al.: character font is different. - References, Ross et al.: in 2007, a paper published in ACPD in 2004 should be either published in ACP or rejected. Please update reference. - References, Solomon et al., "temperatuRe" instead of "temperatuTe" - References, Tegtmeier et al., "anoMaLies" instead of "anoLaMies" - Figure 2: the 1:1 reference line should obviously not pass via (260:250). Please correct the abscissa range and the position of the 1:1 line.

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Interactive comment on Atmos. Chem. Phys. Discuss., 7, 11621, 2007.