

## ***Interactive comment on “The chemical transport model Oslo CTM3” by O. A. Søvde et al.***

### **Anonymous Referee #2**

Received and published: 24 July 2012

This paper provides a comprehensive description of the homogenization of the Oslo CTM2 with developments made to the UCI CTM mostly concerning transport, photolysis and deposition, named as CTM3. Many of the new parameterizations and approaches have already been published in the literature providing a solid foundation onto which this paper is based. However, it needs to be more quantitative and less subjective concerning the scale of the changes introduced for certain trace species.

### General Comments

I recommend publication after the authors have addressed my comments, many of which are concerned with style and clarity rather than actual content. For instance some of the Figures are not useful in their current form either being too small or assuming the reader can calculate differences by eye (not so simple in some instances). The many short paragraphs (sometimes only one line) should be condensed to remove

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sentences which are either repeating details or not providing relevant information. It is confusing for the discussion to flit about comparing many sensitivity tests often without any strong conclusions. I would advise that if the changes between two sensitivity studies are not important then do not mention them.

I think that the naming of the models in the text should be sequential e.g. “when comparing CTM2 with CTM3” rather than being the other way around as it currently does not follow a logical style. This should be corrected throughout the text.

Apart from the wet scavenging of trace gases is there any conversion of e.g. N<sub>2</sub>O<sub>5</sub> on wet surfaces? Please add a statement.

One weakness of the current paper is that there is no comparison of the distributions of either CO or CH<sub>4</sub> against measurements which is provided. Considering the dominant effect of CO on oxidative capacity and CH<sub>4</sub> lifetime it is necessary to provide at least one figure comparing some surface values from e.g. the GMD dataset. One dominant question related to CO is related to how good the seasonal cycle represented at the high northern latitudes and whether the mixing ratios in the Southern hemisphere are of the right order of magnitude and not over-estimated. One question related the CH<sub>4</sub> is how the model captures the regional variability (polluted vs remote) which can then be used in the discussion related to burdens, etc.

The LiNO<sub>x</sub> sensitivity study does not really add much to the paper as it is currently presented as it is not clear whether this update is important for improving e.g. NO<sub>x</sub> distribution of UT O<sub>3</sub>.

Specific comments:

Pg 1563: lines 14-19: This paragraph should be removed as it is not providing any new information. It is almost a mini summary of the paper the authors are about the present.

Pg 1563, Ins 21-25. All these details regarding possible future aerosol versions of

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CTM3 should be shifted to the model description and not appear in the middle of the plan of the paper.

Pg. 1564, In 1: More info on the sensitivity studies is needed. Why are they presented? What is the aim of each study? An introductory paragraph on some of the other contents related to the Polar transport issues and LiNO<sub>x</sub> update should be included.

Pg. 1564, In 23: Where or what is section 2.5?

Pg 1565, In 18: Remove “in addition”.

P 1567, In 8: Please refrain from using the colloquial term “blob”. Surely a more scientific notation is “local maxima”?

P 1567, In 13: The phrase “. . . shredded the vortex” sounds like irreparable damage. I think you mean “. . . more diffusive therefore perturbing the isolation of the Arctic Vortex allowing air to leak out”.

P 1568, In 5-7: Merge paragraph with proceeding paragraph.

P 1569, In 4: Quantify ! Terms such as ‘slightly different’ are too subjective.

P 1569, In 8: solved -> dissolved

P 1569, In 9: This implies that upon evaporation chemical constituents are dumped back into the gas phase but I am not so sure this is the case in the model. For HNO<sub>3</sub> it is most likely to remain as NO<sub>3</sub><sup>-</sup> on the CCN/particle.

P 1569, In 22: Is the HNO<sub>3</sub> uptake reversible or irreversible ? Burial or Langmuir? Is this an important sink for NO<sub>x</sub> in the model?

Pg 1570, In 22: ‘individually tuned’. This opens a whole realm of possibilities concerning the treatment of the absorption characteristics for use in fast-JX. Is this to reduce errors compared to a full solution of the J value on a higher resolution grid. Have the J-values been compared against measurements at all? If not how do you know this

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aspect of the model upgrade results in an improvement in terms of performance.

Pg 1572, ln 3-5: Is sulphate aerosol included in the scattering component? If not then why only account for the BC absorption as the scattering component above the boundary layer will introduce an additional redistribution of the UV flux in the vertical (more important on clear days).

Pg 1572, ln 8: Energy and Industrial production will NOT have a pronounced diurnal cycle (there is never a shut down for industrial processes as this costs too much money and time). Is this diurnal cycle going to zero during the night?

Pg 1572, ln 9: Motivation for the 96%/4% split? Is this based on measurements.

Pg 1572, ln 15: Again motivation for the 90%/10% split.

Pg 1572, ln 19: What effects did the plume parameterization have on the results? Summarise in one sentence. It could ultimately be tailored to the new model set-up.

Pg 1572, ln 21: Please give the total emission N yr<sup>-1</sup> from lightning.

Pg 1572: Please add a sentence explaining what is used to constrain the CH<sub>4</sub> in the model (climatology based on measurements or is it a fixed mixing ratio). Is there a seasonal cycle and is there a latitudinal gradient as observed?

Pg 1573, ln 8: remove 'with'. Ln 9: add '(

Pg 1573, ln 16-20: Are these profiles in the convective column also used in other parameterizations in the model or are they not from the meteorology used to drive the model?

Pg 1575, ln 6-9: Condense into preceding paragraph.

Pg 1575, ln 16: is -> 'has been'. Ln 19: 'transport-chemistry' -> 'chemistry-transport'.

Pg 1577, ln 26-27: Figure 4 needs to be assessed for clarity. Showing the absolute differences between CTM2 and CTM3 would provide more information related to the

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changes introduced between both models.

Pg 1578, In 27: 'produce' -> produces

Pg 1578, In 29: Fig. 6 is not currently legible being much too small !!. Split into Fig 6a/b using the different months as the division criteria. This should really have been amended in the technical review before publication in GMDD.

Pg 1579, In 5: The statement regarding the stratospheric differences are insignificant between C3 and C3NIT should be moved forward to directly after where the simulations are introduced to avoid repeating this multiple times further through the paper.

Pg 1579, In 11: under-estimates "it". It being . . . ? In 22: "similar distributions in N2O".

Pg 1580, In 10: " although the amplitude " . . . amplitude of what ? Seasonal cycle ?

Pg 1582, In 3-5: This previous multi-model inter-comparison study allows the authors to place the performance of CTM3 in the context of other models which are used in the community. Some comment should be made regarding what deficiencies CTM2 exhibited and whether these have been improved upon.

Pg 1583, In 4-9: The scale used for Fig 9 means that changes in tropospheric profiles are not discernible. It appears as if CTM2 has a better performance around 250hPa across all profiles. This figure should be replaced by something more useful such as either the CO or CH4 surface comparisons.

Pg 1583, In 11-14: Remove short paragraph.

Pg 1583, In 22-26: By showing a correlation plot for the troposphere, upper troposphere a more quantitative discussion could be included. I would present a seasonal comparison for the 3 latitude bands used in the paper. You could add the various comparisons in an appendix if you feel that they are necessary. By using the correlation co-efficient a much stronger case can be made regarding improvements (or degradations). If there is any degradation in performance then one dominant term for O3 is

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photolytic destruction. Maybe this has degraded (become too efficient) in the upper troposphere ?

Pg 1584, In 27: More photochemically active because of the new solar spectrum or ... ? Is there a sun cycle imposed on the TOA flux ?

Pg 1585, In 15: Conversion of N<sub>2</sub>O<sub>5</sub> to HNO<sub>3</sub> is another important process that is not mentioned yet.

Pg 1585, In 18-25: There seems to be a wide range of opinion regarding the dry deposition flux of CO for use in global models. What is the total deposition flux per year in CTM2 and CTM3 ? Although high variable with respect to land type, neglecting dry deposition in arid and ocean regions could be another way of improving the CO distributions.

Pg 1586: A figure comparing surface monthly mean mixing ratios of CH<sub>4</sub> would give more confidence towards any improvements in the model performance and the subsequent discussion related to CH<sub>4</sub> lifetime.

Pg 1585, In 22: Rather than discussing the performance of other models (not relevant to this study) comparing the global CO distribution in CTM3 against measurements would significantly enhance the quality of the discussion regarding lifetimes considering it is a well-mixed gas which scavenges OH effectively.

P 1587, In 3: If you use a different set of anthropogenic and biomass burning NO<sub>x</sub> emissions then I am not so sure how, when comparing values, you can determine the 'correct' value for CH<sub>4</sub> lifetime. Maybe there are other studies that have used the RETRO inventory (without aerosols) which could be used for comparison which maybe more relevant.

P 1587, In 6: There have been other studies looking at this effect which need to be referenced such that the 8% effect exhibits an uncertainty range (e.g. 5-15% OH decrease). E.g. Martin et al, JGR, 108, doi:10.1029/2002JD002622.

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P1587, In 25-27. Another factor could be errors in the emission estimates which build up over time when there are no constraints applied towards climatologies in the stratosphere. This should also be mentioned in the text.

P1588, In 14: I do not agree with the statement regarding the definition of the tropospheric domain in Stevenson et al., 2006 for calculating O<sub>3</sub> burdens. It uses the 150 ppb O<sub>3</sub> contour taken from Prather et al., 2001 and states this. Whether this is the “correct” definition is another matter. Without a clear definition of what is considered the troposphere the comparison of the model values would not be valid. Please remove this sentence.

P1588, In 17-19: Surely it is the non-linearity in the O<sub>3</sub> production calculated by the chemical solver introduced when decreasing the time step which also plays a role?

P 1589, In 1: Associated uncertainty on mean model values for STE?

P 1589, In 18-24: As you have used a different definition to define the STE it is no longer directly comparable to the previous multi-model comparison values you quote on line 1. What are the CTM2 and CTM3 values using the Stevenson definition of  $STE = L + D - P$ ?

P 1590, In 3-4: More realistic compared to the previous model values or the measurements?

P 1590, In 8: You weaken the entire section by stating that the chosen isopleths may not be the correct threshold for use in your calculation of STE.

P 1591, In 8-9: Remove text after “1 January 2005”. Details are provided in the next paragraph.

P 1591, In 12 : typo: NHO<sub>3</sub>.

Pg 1591, In 18-20 and 29: What is the lifetime of HNO<sub>3</sub> (H<sub>2</sub>O<sub>2</sub>) given in the literature? Is CTM3 improving compared to what is commonly accepted regarding atmospheric

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lifetimes?

Pg 1592, In 15: Why the focus on HO<sub>2</sub>NO<sub>2</sub>? There are no recommendations for the scavenging parameters for this species so it is speculation regarding the potential importance of trapping on ice. In fact you could argue that a motivation for excluding it in CTM3 is that the evidence of trapping is weak. Other species such as HCHO and N<sub>2</sub>O<sub>5</sub> are scavenged on ice and do have recommendations but are not mentioned in the text. I would like a clear statement about N<sub>2</sub>O<sub>5</sub> here as most global models do include this heterogeneous surface process (on aquated aerosol and cloud droplets) whereas the HNO<sub>4</sub> loss can be considered to be much more exotic and less well measured. This will have implications for the O<sub>3</sub> burden, etc.

Pg 1593, In 8: Values of H<sub>2</sub>O<sub>2</sub> trapping on ice are available so I am not so sure excluding it in the model update is a good thing to be honest. Ideally it should have been implemented along with HNO<sub>3</sub> (although the competition for active surface sites on the ice surface has to be ignored).

Pg 1595, In 25-28: In that the time-step can be iterative and have different values a different latitudes raises the question of the usefulness of the 30 min time-step sensitivity test when constraining at global scale. If the reason is the poles why not limit the time-step at latitudes above 60 degrees? Also why not reduce the time-step to 15 minutes to prove that 30 minutes is sufficient for capturing the polar vortexes well? In an operational mode what is the typical time-step for the tropics and mid-latitudes?

Pg 1596, In 1-5: Suggests that the sensitivity study is far from complete and the final limitations/settings needed regarding capturing transport around the poles are yet to be determined in CTM3.

Pg 1596: Sect 4.2: The discussion related to the influence of changing the L-NO<sub>x</sub> profile is so short as to be uninformative, especially as it is a CTM3 vs CTM3 comparison. Without comparing model distributions against satellite NO<sub>x</sub> distributions and in-flight O<sub>3</sub> measurements it is not possible to determine if any improvement occurs directly

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from changing the Li-NO<sub>x</sub> profile.

Pg 1597, ln 4-5: Well there is an improvement regarding model speed-up for a start. The improvement regarding the distribution of trace species is a rather difficult to determine from the paper. This sentence gives doubt as to whether it was worth including extra complexity to the model (excluding the improvements to parallelization).

Pg 1597, ln 17: Too subjective.

Pg 1598, ln 27-29: Move this paragraph up to the beginning of the conclusions.

There is nothing in the conclusions about the age of air which should appear before the discussion on scavenging. Each section of a paper introducing results should have a sequential conclusion synthesized in this final section.

Table 1: The title states “improvements” but point f doesn’t sound like an improvement. I would use the word “model updates”. Many of these improvements are also based on previous work so remove the “New” (use “ Updated ..”) and provide the reference for e.g. the wet scavenging in a separate column next to each point.

Table 2: The last column doesn’t provide much information and could be included in the Table legend (All simulations use an operator time-step of 60 mins apart from C3\_30MIN).

A noted by the other referee the figures need to be improved in many instances to be used as evidence of an improvement due to the model updates.

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Interactive comment on Geosci. Model Dev. Discuss., 5, 1561, 2012.

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