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**Geoscientific
Model Development
Discussions**

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

Anonymous Referee #1

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General comments:

This is a nice description of the substantial updates and improvements that are made in this version of the Oslo CTM relative to previous versions. I recommend publication after relatively minor revisions. I would like to see the authors distinguish more clearly between the model and the concepts on which it is built from the ECMWF winds that are input to the model. The computer construct could be perfectly adequate but some deficiencies (like STE) cannot be remedied except by improving the input to the model. General comments are below, and specific comments follow. There are numerous grammatical errors, some of which I point out, and I encourage one of the native English speakers to read the final version with this in mind.

Resolution – The discussion of spatial and temporal resolution could be much more clear. The long time step without an increase in transport error seems to contradict

statements about errors near the jets and the polar vortex. When high resolution simulations are discussed, I think this means high spatial resolution (because the longer time step allows it) but this needs to be clarified. I was confused by the prior statements about transport near the jets and the possibility to reduce the time step if needed when reading the description of advection and the internal computation of the time step.

The paper should summarize the reason for using pieced forecasts rather than the analysis fields and how this affects meteorology. There are clearly some questions about the meteorology and deficiencies in the current system compared to the previous system that need to be discussed more directly.

When comparing the versions of the CTM it isn't always clear whether meteorology is the same or different.

Do any observations compare sufficiently differently with the simulations that we can unequivocally tell the simulations apart (and pick one as better?). Even in the abstract – compares with observations 'well'. There are many improvements – is the new CTM objectively better or similar but faster?

The figures are often small and difficult to read even if enlarged. Perhaps they can be made at higher resolution so that enlargement makes them more legible.

Specific Comments:

1562 Abstract Line 6 – high spatial resolution Line 10 Use of the same meteorology to drive the two models shows that

1563 Line 15 first time the resolution issue. Even in the abstract – you are losing spatial information in regions of sharp gradients due to the long time step – so what do you gain from more spatial resolution? Need to be clear.

1565 L 13 'stratospheric losses' – I presume this means long-lived gases, and the radicals and reservoirs aren't considered at all? (which is fine for this application, but since the purpose of this paper is to describe the model I think the statement should

be explicit).

L 14 – I don't understand why the statement about the CTM2 climatology is here at all.

1567 L 15ff – This paragraph is confusing. The P2008 connection was severed? By the meteorology or by the implementation? The new meteorology is good or the new transport is good. The pie shape boxes have a problem in V2 (which is the new algorithm) – what is the difference between the new algorithm (V2) and the 'final algorithm'. Is the final algorithm an option or the new standard algorithm?

1569 L 8 the amount of tracer solved in rain? Technically correct I think (I looked it up) but not the most common usage (I checked). 1570 L 17 – what 'new' solar flux data are you using? L 24 there is a problem with the sentence that starts 'With the JPL-2010 update – should 'Sander et al.' be in parentheses and follow 'and more complex wavelength-pressure dependence"?

1575 – when talking about the lightning – 'the high flash rates are better modeled' – I am not sure where the 'better' judgment comes from. Physical basis of parameterization or some other comparisons with observations?

1576 age of air – Are CTM3 and CTM2 run with the same meteorology? It seems so but an explicit statement would be useful.

1577 – The age discussion – no data brought to bear anywhere, so I guess changing the transport timestep makes the simulations 'agree' but it isn't clear to me that anything is 'better'.

CCMVal (and other papers) show a more quantitative comparison for polar ozone loss. In general – ''reasonable polar ozone loss' – statements like these should be avoided in a model description when quantitative metrics are available.

Whey you say 'better' than Sovde et al. (2008) is this subjective or objective?

1579 – Is the HNO3 problem a lack of NOx or is it a lack of processes to convert NOx

to HNO3? (there is a paper by Kawa et al. discussing this in UARS data).

1579 – stagnant vertical transport – what about previous discussion of age of air?

1580 – N2O gradients matter at least as much as the N2O magnitude in developing the FRIAC. I expected a better explanation of why these results are not as good as discussed by Allen et al. for a CTM that is conceptually similar. It is likely that the 'stagnant vertical transport' matters to this also.

1584 Line 15 – drop 'carried out' (confuses the sentence and is not needed).

1585 CO emissions are too high, too low – how much OH difference can you account for by changing the emissions (within reason).

1586-87 The discussion of the OH lifetime could be much more clear. The 'literature' values are based on what? How does Prather get his estimate, and why is it different from the literature. It seems to me you need to say something about that if you are going to use these values as some marker for the difference in simulations. In some sense – literature and Prather differ by more than C3 and C2 – so is the difference significant at all?

1587 if stagnant meteorology makes the N2O lifetime long, won't it have the same effect on the stratospheric part of the CH4 lifetime?

1589 500 Tg O3/yr from Hsu et al. 2005 (among others). Is this the same method used here? The values here are much smaller. The big increase when removing PSCs is odd – the observational estimates don't make such a distinction – does the Hsu et al. estimate include PSCs? This is a confusing discussion with no conclusion.

1589 – if the meteorology has a 'stagnant Brewer Dobson circulation' no change in the ozone isopleths is going to give a better STE estimate – or a better age of air or appropriate mass flux. If the upward transport is slow, the downward transport (of mass and ozone) is also slow. This isn't speaking about the model – C2 or C3, it doesn't matter. There are issues with the met fields that make them inappropriate for many

studies – probably for example included the FrIAC, because no matter the numerics if the gradients are not appropriate in the middle stratosphere to begin with, the initial pulse of tracer that forms the FrIAC will have insufficient contrast with respect to the background and compare poorly with observations. A few well crafted and specific sentences on this point are needed – and along with this fewer general statements.

1589 – why would it make sense to take out the polar chemistry when calculating STE? The STE of ozone in the true atmosphere has an ozone hole (and dilution etc.) affecting ozone isopleths throughout the SH in particular.

1597 - the new model is as good as if not better than the old model – you have made changes to improve implementation (e.g., numerical transport) and to improve the physical basis of some parameterizations (e.g., lightning and convection). Even if the comparisons with observations were nearly identical, you could argue the new version is 'better' than the old version because the physical basis is improved, at least in the troposphere (which you finally do in the last two sentences of the paper). There is some confusion (in my opinion) about the stratosphere because of the new version of the met fields – that 'stagnant' meteorology depends on input and not on the model per se. However, some aspects of the comparisons with data are affected.

1597 – Discussion the OH – Present model should be improved, but OH is high. Then representing other processes (sulfates and nitrates), the 'bias' is reduced. There could be somewhat more cogent discussion on this point – the version with less physical representation of precipitation scavenging had 'apparently' better OH but clearly if the sulfates and nitrates were not included the good agreement would be for the wrong reason.

1598 – it is a serious point to discuss how to fix up the STE without fixing up the circulation – in the spirit of the preceding comment (right answer, wrong reason) I would say that it is not possible to have the correct fluxes of mass and ozone without having a realistic stratospheric circulation. Those other things that could increase the

amount calculated for STE are secondary to this point.

Figures

Figure 4 – if the point is pattern recognition – fine, although masking the output so it isn't shown where there are no observations would make pattern recognition easier.. Nonetheless – differences between TOMS and both CTMs are obvious, and it isn't clear one is better than the other.

Figure 5 – the feature you are discussing (the mini hole over Europe) is nearly invisible in this plot (in the observations).

Figure 6 is almost unreadable. Too small, and labels are not resolved and very difficult to read even if enlarged.

Figure 9 – a few random sondes don't reveal much about the model. Do you have chance of a sequence where you could see some feature? I don't see how this figure supports statements in the text about folds.

Figure 10 – also small, hard to read and pretty much impossible to interpret.

Some of the grammar problems 1563 Line 2 CTM2 resulted from (rather than 'was derived from') Line 7 better transport and diagnostics have been added – the core CTM does not 'develop' 1564 L 25 – no need for last word 'applied'. 1578 L6 delete the comma after available. 1568 L 21 no information . . .is used . . . 1569 L 25 'and therefore treats' – grammar issue. 'splitting it' – not clear what 'it' refers to. 1575 L 24 The C3 simulation produces 1578 L 4 – the models produce reasonable polar O3 loss – making a statement, rather than the weaker 'models are capable of' (the model actually has no choice). 1578 L16 differences are seen 1578 L 22 The CTM2 produces 1578 L 26 delete 'also' 1579 L 4 CTM3 reproduces 1594 L 3 somewhat larger? greater? Improvement 1595 L23 mainly increases computing time. 1596 L 13 profiles place (rather than places) 1596 L 23 giving a negligible lower tropospheric O3 burden (as opposed to negligibly) 1597 L 17 slightly less of e.g. HNO3 – what is HNO3 meant to

be an example of?

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