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Geoscientific
Model Development
Discussions

Interactive comment on "Global high-resolution simulations of CO_2 and CH_4 using a NIES transport model to produce a priori concentrations for use in satellite data retrievals" *by* T. Saeki et al.

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General comments

Saeki et al. describe NIES transport model (TM) simulations of carbon dioxide and methane. The simulations are at higher resolution (0.5 x 0.5 degree) than previous simulations using the NIES TM and use near real-time, GPV, meteorological forcing. A new nudging scheme is introduced in the stratosphere to improve upper level simulations. The authors argue that higher resolution and near real-time forcing is needed to provide better initial conditions for satellite retrievals of column CO2 and column CH4.

The abstract concludes that the new model set-up is 'adequate for use in satellite

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retrievals'. This is probably the case but I wonder if a stronger conclusion could have been reached if the analysis of the simulations had been more targeted? For example, apart from one illustrative figure, the analysis does not demonstrate whether the higher spatial resolution improves the model performance. Likewise, we do not learn whether the move to near real-time meteorological forcing degrades the model performance.

I have some concerns with the current analysis choices, which I detail below for the areas (Sec 3.1, 3.2, 3.3) that I am most familiar with.

Overall, I do not believe this paper is suitable for publication in its present form. As currently presented, the material seems to be more appropriate to a technical report.

Specific comments

Section 3.1 and 3.2: In these sections the model output is compared with the GLOBALVIEW datasets for CO2 and CH4, firstly for the 2008 annual mean and secondly for the seasonal cycle. I do not believe the analysis adequately addresses the fact that the GLOBALVIEW dataset is a data product; CO2 and CH4 observations are selected and fitted to give pseudo-weekly concentrations with gap-filled values also available. I assume that since the comparison with GLOBALVIEW has been made for 260 and 206 sites respectively for CO2 and CH4, gap-filled data is being used, i.e. the comparison of the 2008 annual mean includes sites that were not actually taking measurements in 2008. I think it would be preferable to do the analysis only for sites that are active in 2008; at least some of the sites that are noted as giving large errors (SCS, ITN, DAA) are those that are reliant on gap-filled data.

While there has been some attempt to select the model output by using afternoon data only, this selection will not be comparable to the GLOBALVIEW selection for all sites; mountain sites often use night-time data to avoid upslope flow, while coastal sites are usually selected to sample marine air only. The model error noted for LJO (p2225) is most likely due to a more rigorous marine-only selection of the LJO observations submitted to GLOBALVIEW compared to the model data selection. If this was the expla-

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nation intended in the text (p2225, line 19-26), it was not clear to me. With appropriate data selection, I would expect a global scale transport model to be able to simulate LJO GLOBALVIEW data. TAP and CRI (p2228) are other sites where I suspect that appropriate baseline selection is critical.

The annual mean at tower sites are noted (p2226, line 9) for being overestimated by the model. How sensitive is the analysis to the vertical model level at which the simulation is sampled? What is the vertical resolution near the surface? Another problem for these continental sites may be that the flux climatology used for CO2 has no diurnal cycle for the biosphere component. This should be stated more explicitly when discussing these results. I suspect this could account for much of the mismatch between model and observations in summer; afternoon selected data would be for the period of maximum uptake for diurnally varying biospheric fluxes, while the model would have less uptake from using daily mean fluxes.

In section 3.2, seasonal cycles from the model for a single year (2008) are compared with mean seasonal cycles from the full GLOBALVIEW period (1979-2008). Wouldn't it be better to make the comparison with the same year?

The observed seasonality at oceanic sites is described as 'reproduced fairly well' (p2227, line 11). However, I think this is difficult to establish for the southern hemisphere, at least based on Figure 4. Seasonal cycles in the southern hemisphere for CO2 are very small, so even a small Model-GV difference could imply a modelled seasonal cycle with incorrect amplitude or phase. This certainly seems to be the case for SPO, as shown in Figure 6, where the modelled seasonality for CO2 does not seem to be a good match to the observed seasonality.

I find the summary statistics provided in Table 1 and Figure 5 and described on p2228 to be hard to assess without any reference to compare them against. This section would be much stronger if equivalent statistics could be provided for the lower resolution version of the model. Then the reader would get a sense of whether the higher

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resolution model improved the simulation (or whether the simulation is mostly limited by the input flux climatology).

Section 3.3. is titled synoptic variations but the choice of sites, such as SPO and MLO, does not facilitate this comparison. The statistics calculated in Table 2 will be dominated by the trend and seasonal cycle, and consequently the analysis in this section currently adds little to that provided in Sec 3.1 and 3.2. It would seem to me that a better way to focus on synoptic variations is to detrend and deseasonalise the model output and observations before making the comparison and calculating the statistics. Focussing on HAT alone would be reasonable, or a second site with significant synoptic variations could be chosen. I understand that the early part of the simulation does not use analysed winds that correspond to that time period, but this could be seen as an opportunity. A comparison of statistics for HAT (e.g correlation) between the period with the correct meteorology and the period with incorrect meteorology should show a clear difference in correlation for synoptic timescales. Calculating statistics on a seasonal basis might also be useful in identifying where poorer correlations might be due to the flux climatology or to transport error.

This section could also provide an opportunity to compare simulations with GPV vs ECMWF or NCEP meteorological forcing. It would be nice to see that the model performance isn't significantly degraded by moving to near real-time meteorological forcing.

Technical corrections

p2217, line 10: Suggest re-write as 'In this TransCom continuous experiment, 25 transport models participated with two running at 0.5x0.5 degree resolution and the others running at ....'

p2217, line 17: Suggest re-write as 'However most model simulations of these greenhouse gases are still carried out at ...'

P2217, Line 18: Allen et al is 2011 in text but 2012 in references

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p2219, line 29: the GLOBALVIEW datasets should be cited here.

p2221, line 1: missing open bracket (

p2221, line 2: replace 'by' with 'at' 0.5x0.5

p2221, line 9: delete 'of'

p2221, line 9-10: presumably the boundary layer height is not available from GPV – do you have any idea as to the consequence of mixing two data sources for the transport model forcing?

p2221, line 14: in different places the 21 GPV pressure levels are mentioned or the model sigma levels. It would be helpful for readers to be clear about when you had to interpolate between different vertical coordinates. (also p2223, line 24)

p2222, line 23: is 2015 really meant here? if so, explain how you deal with future concentrations/IAV

p2223, line 7-8: I would suggest putting the CO2 correction with the CO2 description before CH4 is discussed.

p2223, line 13: delete reference to CO2 fossil emissions here since it is the CH4 trend being discussed?

p2224, line 18: suggest 'close to zero, indicating ..', to replace 'nearly the same zero, indicates'

p2225, line 6: replace 'opened' with 'available'

p2225, line 28: Peerez-Landa spelt with only 1 e in 2007a reference – but I'm not sure how relevant these references are

Sec 3.1: you might want to add a general comment that you might expect CO2 to fit the GLOBALVIEW dataset better than CH4 since the flux climatology used for CO2 included flux corrections from an inversion (which aimed to fit the data) whereas the

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CH4 flux climatology is an inventory only

p2227, line 6: it is not actually amplitude being plotted so perhaps should use ‘seasonal cycles’ instead

p2228, line 5: replace ’some’ with ’about’ – maybe note that for oceanic sites biases increase as move north

p2229, line 2: suggest ’may affect the model’s ability to reproduce the CO2 concentration ...’

p2229, line 10: the MLO and SPO WDCGG datasets should be listed in the references

p2231, line 14-18: I don’t really follow why observations are averaged while model is a single profile and why two different locations?

p2232, line 24: suggest ‘all at 13:00’

p2233, line 1: Europe and NE America look at least as large as Siberia and Asia

p2234, line 4: the description of fig 10 seems disconnected with the rest of the paper though it is a nice illustration of the impact of higher resolution

p2234, line 18: start a new paragraph to discuss CH4

p2234, line 23: delete ‘in equator’

p2235, line 1-2: If the polar vortex is not centred over the pole, would this contribute to what you are seeing?

p2236, line 1: replace ‘correction’ with ‘correlation’

p2236, line 9-10: ’resolve’ not ’resolves’, insert ‘better’ before ’than the 2.0o model does’. I am not sure that the one example given is enough evidence to make this claim.

p2236, line 15: the evidence for the model performance for synoptic variations is weak

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based on the analysis in this paper

Interactive comment on Geosci. Model Dev. Discuss., 5, 2215, 2012.