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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.

Anonymous Referee #2

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General Comments

This paper describes simulation results by NIES-TM high resolution simulation of CO2 and CH4. Saeki et al., insist that the higher resolution of NIES-TM shows good performance for adopt a priori concentrations for use in satellite data retrievals. The experimental setting for meteorology seems to be reasonable (but I consider a combination of JMA meteorology and ECMWF PBL height is one issue). The authors should show more detailed transport processes (mass fixer, cumulus convection and so on). One major issue is that monthly mean CO2 and CH4 flux may be different from actual flux and I have some doubts about the validity of the model validation especially inland

sites. The merit of higher resolution NIES-TM is that the model could treat more realistic meteorological data and I think the authors should put more emphasis on this point. I consider this paper should be acceptable after some revisions described below.

Specific comments

Page 2220, line 7: The authors should show some description for a mass fixer, cumulus convection scheme and turbulent diffusion processes.

Page 2220, line 15: The height of the lowest model level may affect the CO2 and CH4 concentrations of this level. The authors should show it (all model levels are preferable).

Page 2221, line 9: As I mentioned in general comments, the combination of JMA analyzed meteorology and ECMWF PBL height is inconsistent. I consider it better to diagnose PBL height from operational meteorology. Could you show that there is no significant effect of this inconsistency?

Page 2221, line 20: One of the most uncertain fluxes is monthly terrestrial biosphere flux. The authors could compare with different climatology flux.

Page 2221, line 23: The annual global net flux of CO2 determines annual increase of CO2. This may affect annual mean biases. The authors should show each annual net flux of CO2. In CH4 case, such information is available for readers even if there are some chemical losses.

Page 2225, line 5: As mentioned by the first reviewer, GV data are not raw observation data. If the authors try to compare better condition, they should use monthly mean of raw observation data.

Page 2225, line 25: The problem may be CO2 flux climatology, not model resolution or meteorological field.

Page 2229, line 6: The authors should add some inland sites to show model performance precisely. The results may be worse, but the authors could show some merits

of high resolution model (synoptic scale variability).

Page 2230, line 7: The authors should mention the reason of this slightly large mismatch at SPO?

Page 2233, line 12: The authors should mention an effect of PBL height.

Page 2234, line 4: If the authors maintain lower horizontal resolutions' results, it could be nice to compare these results in section 3.3.

Page 2235, line 4: In conclusions, little mention of the advantages of a higher-resolution model. The authors should mention them.

Technical corrections

Page 2234, line 7: Replace 9 to 10.

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