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Geoscientific Model Development Discussions

Interactive comment on "A 4D-Var inversion system based on the icosahedral grid model (NICAM-TM 4D-Var v1.0): 1. Off-line forward and adjoint transport models" *by* Yosuke Niwa et al.

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RC

This is a well-prepared manuscript with a good focus. It is ideally suited for GMD, since it describes the development of an off-line model suitable for inversions of GHG emissions. The focus is on errors due to the time resolution of the meteorological driver data, and on the general validation of the adjoint code. The validation sites are well chosen.

AC

We are grateful for you time to review our paper and for giving us fruitful comments and suggestions. Our replies to the comments are described below with line numbers/pages of the supplementary manuscript. The modifications we made are colored in red in the supplementary manuscript.

RC

There is only one major comment that I would like to make. The numerical errors due to the use of a flux-limiter and due to low temporal resolution of the driver meteo are typically in the order of $\sim$*1 ppm for* CO_2 *(and mostly smaller). It is difficult to place these numbers in a proper context. I advise the authors to provide this context by (i) report typical RMSD differences between different models, e.g. the TRANSCOM ensemble (ii) provide a typical "error" in the NICAM-TM that can be obtained by running in a different temporal or spatial resolution, or by other means (e.g. sampling error)*

AC

We agree in that the numerical errors are quite small in the most cases and it was difficult to place those in a proper context. According to your comment, we have added some discussions. For the suggestion (i), a typical error in NICAM-TM, we have performed an additional on-line simulation in which different wind data (JRA-55) are used for the nudging and calculated the RMSDs between the two on-line models (JCDAS versus JRA-55). Furthermore, we have added a table (Table 3) to show temporal correlation coefficients of synoptic variations between the models and the observation

[Page 9, line 25-28]. Comparing with the model error (JCDAS versus JRA-55), this also shows implications for the magnitudes of the data thinning errors (corresponding descriptions are from **Page 9, line 32 to Page 10, line 4**). For the suggestion (ii), typical difference between different models, we have cited Patra et al. (2008), which shows a correlation coefficient (not RMSD) range among the TransCom models **[Page 10, line 4-6]**.

RC

A more minor issue is the use of the word "underestimation" in several places. It should be absolutely clear that the "truth" is defined as the online model simulation. In fact, it turns out that the A6V6C6 version performs "better" over Russia. I have added some further textual suggestions in the attached pdf file.

AC

Accordingly, we have changed the word "underestimation" in several places **[Page 9, line 2-3; Page 10, line 12-13; Page 13, line 13]**. Furthermore, we have added further descriptions about the A6V6C6 performance at Karasevoe as below:

"This lower CO_2 of A6V6C6 does not necessarily cancel out the positive deviations of the on-line model from the observation (Fig. 4b) and hence is not closer to the observation than the on-line model. In fact, the correlation coefficient of the synoptic variation reduces from the on-line model to A6V6C6 (from 0.610 to 0.579), whose magnitude is relatively large compared to the other changes (Table 3)."

[Page 10, line 14-17]

Other minor issues

RC

Page 2, line 13: 20 years is hardly feasible, also because model transport errors start to play a role on longer timescales. More attention to model errors would improve the manuscript further.

AC

We agree in that a 20-year-long assimilation is very difficult. Here, we wanted to mention the need for an analysis on interannual variations (IAVs), which does not necessarily require one 20-year-long assimilation window. Consecutive several-months-long assimilation windows could be one way to estimate GHG IAV fluxes, but it still requires a number of forward/adjoint simulations. To clarify this, we have modified the text as follows:

"Moreover, a global inversion calculation of an atmospheric greenhouse gas requires a long time analysis ($\sim$20 years; e.g. Chevallier et al., 2010) to figure out interannual variations of surface fluxes, resulting in at least hundreds of years of model simulations in total."

[Page 2, line 13-15]

RC

Page 3, line 6: "The discrete adjoint is linear but reduces the accuracy of the numerical

scheme, while the continuous adjoint is non-linear but maintains the numerical accuracy." The fact that numerical wiggles as "fixed" does not necessarily mean that the numerical accuracy is higher, because it implies also numerical diffusion. See also page 6, lines 14-15.

AC

We agree in that a flux limiter suppresses numerical wiggles and does not improve the model accuracy in general. Accordingly, we have modified the text as follows:

"the continuous adjoint is non-linear but maintains the numerical accuracy"

=>

"... maintains the monotonicity" **[Page 3, line 8-9]**

and

"the discrete adjoint produces negative (or oscillatory) sensitivities and reduces the model accuracy to some extent."

=>

"... (or oscillatory) sensitivities." **[Page 6, line 22-23]**

Nevertheless, we think the flux limiter we use could improve the model accuracy, as shown in Miura (2007) (we have added a description in **Page 6, line 15-16**). In fact, we obtained larger correlation coefficients against the observations when the flux limiter was used, though they are minute changes (corresponding descriptions are added in **Page 10, line 21-23**).

RC

Page 7, line 18: 7 minutes: please specify which configuration (frequency of meteo input, I assume A6V6C6)

AC

We used A3V1C3 for this calculation. However, the computational time does not depend on the temporal resolution of the input meteorological data. Descriptions are added in:

[Page 7, line 31 – Page 8, line 2]

[Page 13, line 7]

RC

Please also note the supplement to this comment: http://www.geosci-model-dev-discuss.net/gmd-2016-231/gmd-2016-231-RC1- supplement.pdf

AC

We very appreciate your many suggestions. We have modified the text according to all the suggestions (colored in red in the supplementary manuscript).

Reference

Miura, H.: An Upwind-Biased Conservative Advection Scheme for Spherical Hexagonal–Pentagonal Grids, Mon Weather Rev, 135, 4038–4044, doi:10.1175/2007MWR2101.1, 2007.

Please also note the supplement to this comment:
http://www.geosci-model-dev-discuss.net/gmd-2016-231/gmd-2016-231-AC1-supplement.pdf

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