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***Interactive comment on “Simulations of column-average CO<sub>2</sub> and CH<sub>4</sub> using the NIES TM with a hybrid sigma-isentropic (sigma;–theta;) vertical coordinate” by D. A. Belikov et al.***

**Anonymous Referee #2**

Received and published: 2 August 2012

**1 General Remarks**

The paper compares columns of long lived trace gases simulated by a CTM driven by reanalysis data with observations. A new coordinate system in the model is introduced. Unfortunately the text is often not unique because of sloppy expressions or missing definitions. Uncertainties due to the use of climatologies for chemical sinks and precalculated heating rates are not enough addressed. Several Figures and Tables cannot be understood from the captions, but only after carefully reading the text

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(even that is not sufficient for some). The paper needs a lot of clarifications to be acceptable.

## 2 Specific Comments

**Abstract:** Give full expression for  $\text{XCH}_4$  and  $\text{XCO}_2$ . Is this surface mixing ratio (like in other papers) or the quantity in the title? Please give locations and some reasons for discrepancies already there instead of words like 'bias' or 'contrasting performance'.

**Introduction:**

**Paragraph 3:** I suppose 'column averaged' implies a pressure weighting and the quantity is related to the total column as seen for example by nadir viewing satellites. Please give a more detailed definition.

**Paragraph 4:**  $\text{CH}_4$  is oxidized by OH and Cl mostly in the troposphere. The stratosphere has only a minor effect on surface mixing ratios. The total column as referred to in the reference is indeed dependent on tropopause height because of the decrease in mixing ratios above the tropopause. However, it is also dependent on the surface elevation which is not mentioned. Please reword paragraph to avoid misunderstandings.

**Paragraph 7:** A lot of the problems discussed here arise from artifacts in the ERA 40 reanalysis.

**Section 2.1:** Please define all variables used in equation 1 and give the region where the transition is applied. Refer to Table 1.

**Section 2.2:**

**Paragraph 1:** The use of precalculated heating rates based on climatologies of radiatively active gases ( $\text{CO}_2$ , ozone,  $\text{CH}_4$ ) and the meteorological reanalysis introduces errors in the vertical transport due to inconsistencies. Especially ozone and its radia-

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tive interaction with clouds can be critical in the tropical lower stratosphere, but also in high latitude spring (ozone hole!). Some error analysis should be given.

Paragraph 5: 300K is in most cases in the troposphere, except near the poles where normally no ascent occurs (last paragraph). This 'fudge factor' is confusing. There is also a contradiction to Eq. 1.

Section 2.3: Please mention how the reanalysis data are converted to the model grid. Refer to Table 1 earlier. How often are the heating rates calculated? Every timestep and at every gridpoint or just climatological as said in section 2.2?

Section 2.5:

The use of the old OH climatology by Spivakovsky et al., (2000) in the troposphere and 2D-models in the stratosphere without any interannual variation can cause deviations for CH<sub>4</sub> at different sites. Cl from seasalt is not mentioned. The sinks should be implemented in a better way.

There is now also EDGAR 4.2 available. The use of constant emissions after 2007 causes larger deviations from observations. It might be better to show results only to the end of 2007.

Section 3.3.1, paragraph 1: The bias should be due to wrong emissions and/or sinks. Fudging is no solution.

Section 3.4:

Paragraph 3: Are the site specific profiles not seasonal? To include seasonal effects is very important at these sites. Is there a problem with the polar vortex at Sodankyla? This can easily explain biases by 5 to 10% as seen in a CCM which shows a clear anti-correlation between stratospheric CH<sub>4</sub> column and potential vorticity at about 70hPa.

Paragraph 5: Again troposphere and stratosphere are messed up. One reason for offsets might be indeed the not specific (or outdated) sinks in the troposphere and

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stratosphere.

Section 3.4.1, last paragraph: For most sites the variability of the model results is much less than for the observations. Especially at Sodankyla this appears to be due to the use of climatologies for calculation of vertical motions.

### 3 Technical Corrections

Page 8058, line 29: height-coordinates!

Page 8070, line 17: Typo.

Page 8071, line 24: bad wording.

Page 8072, line 21f: Separate sentences.

Page 8088, Table 1: remove horizontal lines in part on upper troposphere and stratosphere. The numbers in the table are only valid for an ocean surface. Say something on mountains in the caption or give  $p/p_s = \sigma$  instead of  $p$ .

Page 8093, Figure 3: There is something wrong at the poles (kinks).

Page 8094: Figure 4 should be given with  $\log(p)$ , however it also might be omitted. Is an average over all stations shown? Clarify in caption, refer at least to text (better repeat sentence in caption).

Page 8095: Add 'surface volume mixing ratio' in caption. Is this meant here?

Page 8098: A table would be better here.

Page 8099: Shown is volume mixing ratio, not concentration, please correct caption.

Page 8101 and 8104: Typos in station names in legends. Please write out XCH4 and XCO2 in captions.

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Page 8103 and 8106: Use more other symbols than circles to allow for better distinction between stations.

## 4 References

Spivakovsky, C., Logan, J. A., Montzka, S. A., Balkanski, Y. J., Foreman-Fowler, M., Jones, D. B. A., Horowitz, L. W., Fusco, A. C., Brenninkmeijer, C. A. M., Prather, M. J., Wofsy, S. C., McElroy, M. B.: Three-dimensional climatological distribution of tropospheric OH: update and evaluation, *J. Geophys. Res.*, 105, 8931–8980, 2000.

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