<u>Review of: On the uses of a new linear scheme for stratospheric methane in global</u> <u>models: Water source, transport tracer and radiative forcing, by B.M. Monge-Sanz, M.P.</u> Chipperfield, A. Untch, J.-J. Morcrette, A. Rap, and A.J. Simmons

The paper presents a new linear parameterization for stratospheric methane and water vapor tendencies suitable for GCMs. The scheme is applied in a 3-D CTM and in the ECMWF GCM (IFS model). Good (fair) agreement for CH4 (H2O) is obtained in comparison to full chemistry runs and satellite-observations of the seasonal/latitudinal CH4 and H2O distributions. However, flaws in the H2O distributions are at most partly related to the water vapor tendencies produced by the scheme. Coupled to the radiation scheme the new scheme for CH4 leads to significant (up to 2K) cooling in the tropical lower stratosphere compared to the use of the default (GEMS) climatology. The potential of the new parameterization to diagnose transport is investigated by comparisons between the performance of the scheme in a free-running IFS model with its performance in the CTM driven by / IFS model nudged to ERA-40 and ERA-Interim reanalysis.

## General

The paper primarily documents a new parameterization for stratospheric methane together with some applications. The style in which the paper is written largely reflects a technical note more than a scientific paper. Therefore, I would recommend considering this paper as a technical note and suggest the authors to change the title following what is written on the ACP website: "Technical notes report new developments, significant advances, or novel aspects of experimental and theoretical methods and techniques which are relevant for scientific investigations within the scope of the journal. The manuscript title must clearly reflect the technical nature of the manuscript and should start with "Technical Note:". This review is further considering the manuscript as a technical note.

I think it is a very welcome study and it will be suitable for publication in ACP as technical note after a couple of modifications. It is mostly clear its objectives. However, I do have a couple of general and specific comments on the presentation. The paper is not very well structured. As explained below the presentation of the results should be harmonized. Some limitations are not made explicit and should be discussed (see below).

- It is clear that a good stratospheric methane parameterization for prognostic CH4 is needed in the ECMWF IFS model. It is needed for water vapor tendencies and radiative calculations and useful for transport diagnosis. A bit surprising is that climate effects are touched upon with radiative forcing calculations but then no reference is made to the recent use of the IFS in EC Earth (Hazeleger et al., 2010). Improvement of stratospheric CH4 an H2O is very important for climate and seasonal prediction applications of IFS. Also the need for stratospheric CH4 variability in GEMS/MACC emission inversions of

CH4 total columns from nadir-viewing satellites is probably an extra argument for implementing the new scheme in IFS. So, the relevance of the model improvement for IFS is not disputed. A fundamental question however is if the methane scheme is also suitable or attractive for other GCMs. I am not convinced. This point is claimed in the abstract and is important to distinguish this paper/technical note from an ECMWF technical report. If the authors have good arguments they should include these in the introduction and compare in the results with achievements/limitations of other schemes used in other GCMs. Most important argument to use this scheme in a GCM would be in my view (i) to improve radiation calculations and (ii) to prevent computationally expensive stratospheric chemistry. Diagnosis of transport in IFS is of course relevant, but could also be achieved in other ways (e.g. I think in IFS also ozone is transported).

- For the general usefulness of using CoMeCAT's CH4 and H2O distributions in GCMs it would be needed to see the radiative effect of the CoMeCAT fields (either in SLIMCAT or IFS) relative to using the GEMS CH4 climatology in the Edwards-Slingo radiation model. In this way the first order effect of the vertical CH4 profile is removed. Separate and combined radiative effects of the CoMeCAT CH4 and adjusted stratospheric H2O fields should be discussed for the GCM relative to using climatology.

- For the water vapor aspect it should be better explained that only tendencies from methane oxidation are provided. The new scheme should not be referred to as a full stratospheric water vapor scheme. The Austin et al. (2007) scheme is e.g. dealing with stratospheric water vapor and not with methane. Such distinctions could be defined much more precisely in the text. Evaluation/comparison of the new scheme for water vapor tendencies should prevail over a comparison in terms of H2O concentrations. There are limitations to the budget in equation (2) related to water sources and sinks, e.g. tropopause cold point temperatures, a mesospheric water source and polar stratospheric dehydration. These limitations hamper the evaluation of the water vapor tendency of the scheme compared to other flaws in stratospheric water vapor such as in the case of ERA40.

## Specific

- The introduction (Section 1) can be shortened, e.g. no repetition of other schemes which are presented in the literature elsewhere. I doubt that their equations are needed in this paper. Important differences and limitations can be explained in words. Possible limitations of equation (2), see remarks above, however should be shortly discussed.

- After the first sentence of the paper on radiance assimilation this aspect is not anymore covered in the manuscript. I had expected that in section 7 the radiative effects would not have been limited to a set of radiative forcing calculations but would have extended to the

effects of the improved CH4 distribution on the top-of-atmosphere radiances, potentially affecting stratospheric temperature adjustments in data assimilation. This point should preferably be covered. If this would not be feasible for this paper, the first sentence should be removed and a recommendation added at the end of paper that the impact of the scheme on radiance assimilation in IFS will need to be examined in the future.

- Section 2.1 is not needed as a separate section. It is just describing the water vapor tendency from the methane scheme discussed in section 2 and can be added to the text above.

- Section 3 presents the core results of the paper: the coefficients and the performance of the scheme against full chemistry calculations. If these coefficients were also made available to the interested user similar as e.g. the Cariolle coefficients for ozone, this would help to improve the general usefulness of this paper, which could then serve as main reference paper for such a public data set.

- Section 4 is just 'methods/tools' and could be incorporated in short in section 5.1, or should have been presented before the main results presented in Section 3.

- Section 5 could be merged with section 3 to include the evaluation of the coefficients with the full-chemistry model directly after their derivation. The discussion in section 5.2 should focus on evaluation of the water vapor tendency as produced by the scheme in the CTM. I am not sure if much is learned from the comparison with annual average H2O profiles from HALOE (figure 7). Remove here as the CTM results for H2O are also presented in section 6.4 and the paper focuses on the CH4 fields for scheme evaluation.

- Section 6 describes applications of the scheme in the ECMWF IFS model. The presentation order seems rather random. Section 6.1 compares the CH4 distribution of CTM and GCM. I suggest to link this comparison to the nudging effects and transport (6.3). Section 6.4 compares the H2O distribution of CTM and GCM and could include the discussion of the CTM results from section 5.2. Section 6.2 shows the impact of coupling the new scheme to the radiation scheme instead of using the CH4 climatology. This is an important result for climate/seasonal prediction applications. The radiative forcing discussion in section 7 (altered, see general comments and bullet below) could be linked to this result.

- In Section 6 p.496; l. 20) the statement is made that run 'fif4' uses 'the default ECMWF CH4 climatology' in the IFS radiation scheme, while run 'fipj' uses the CoMeCAT CH4 distribution. This is in disagreement with Table 2. Please explain the difference between the runs and include 'fipj' and coupling to radiation scheme in Table 2 with reference to Section 6.

- For Section 7 it is explained earlier (p. 496; 1.1-8) that the IFS version used in the present study with GEMS climatology is improved compared to an earlier IFS version with a constant tropospheric CH4 mixing ratio. For this paper on CoMeCAT the net RE change by CoMeCAT compared to the use of the GEMS CH4 climatology is more important than the differences shown in fig. 12 which are relative to a constant mixing ratio. I suggest removing fig. 12. The difficult discussion at the end of Section 7 can be left out. The reference to the radiative effect of contrails is a bit arbitrary. Remove also the two last sentences from the abstract (p.480; 1. 24-29).

## **Technical**

p.499, 1.19: In the tropics the 100 hPa level is most likely situated in the upper troposphere (TTL) and not in the lower stratosphere.

p.505, 1.5-8: Is it really needed to refer to two different issues of the same book?

p.510, Table 1: Expand Table caption. Tell what is included in the different columns

p.511, Table 2: Why CoMeCAT 'schemes' in plural? I understand CoMeCAT is being presented as one new scheme/parameterization for methane and water vapor tendency?

p.511, Table 2: same comment as for Table 1

p. 513, Figure 2: Why is the unit in days? This makes the numbers in the contourplot difficult to read. Better use 'years' for readability. Contour levels should be specified in the caption if these are not linearly increasing and/or unreadable.

p. 514, Figure 3: Contour levels should be specified in the caption if these are not linearly increasing and/or unreadable.

p. 515, Figure 4: Contour levels should be specified in the caption if these are not linearly increasing and/or unreadable.

p. 516, Figure 5: fig 5a small white feature. Maybe slightly off-scale in top of atmosphere above south pole (see also fig. 10)? How to interpret the white colors at the poles in fig 5a/b? Is the model grid up to 85 deg N and S or is it also a problem with the contour plotting program? Similar features in fig. 11.

p. 516, Figure 5: fig 5c explain in caption the latitudinal limits of the model-HALOE comparison

p.519, Figure 8: Text on top of the figure is incomprehensible. Remove and include relevant information in the figure caption below the figure.

p. 519, figure 8: Add after temperature the unit: '(in K)'

p.519, figure 8: I do not understand that the contour around 10 hPa at the south pole is white (meaning no temperature difference). Problem with contour plotting program?

p.520, figure 9: Again white coloring for most positive and most negative. Please prevent, although here it is not as confusing. Font of contour labeling could be reduced to improve readability otherwise indicate labels in caption.

## **References**

Hazeleger, Wilco, and Coauthors, 2010: EC-Earth: A Seamless Earth-System Prediction Approach in Action. Bull. Amer. Meteor. Soc., 91, 1357–1363. doi: http://dx.doi.org/10.1175/2010BAMS2877.1