MS No.: acp-2011-837: *'On the uses of a new linear scheme for stratospheric methane in global models: water source, transport tracer and radiative forcing.' Monge-Sanz et al.*
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Response to Reviewer #2

We thank the Reviewer for his/her comments and suggestions, which have raised points requiring improvement or clarification in the ACPD manuscript. The final manuscript for ACP will address these points as explained here below.

Please note that our responses to the Reviewer's comments are in bold font, while the original comments are in normal font**.**

Review of: On the uses of a new linear scheme for stratospheric methane in global models: Water source, transport tracer and radiative forcing, by B.M. Monge-Sanz, M.P. 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 CH_4 (H_2O) is obtained in comparison to full chemistry runs and satellite-observations of the seasonal/latitudinal CH_4 and H_2O distributions. However, flaws in the H_2O distributions are at most partly related to the water vapor tendencies produced by the scheme. Coupled to the radiation scheme the new scheme for CH_4 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.

The style and organisation of the paper will be significantly modified already in response to the comments made by Reviewer 1. Also the scientific relevance of the results shown in the paper will be emphasized, clarified and further explained (see also responses to comments raised by Reviewer 1).
The paper not only introduces the development of a new scheme, it also uses the scheme to obtain results with implications for climate research, seasonal prediction and

atmospheric transport of chemical tracers. The experiments we have carried out prove the relevance of the stratosphere in model simulations from seasonal to annual scales, including impacts on temperature and radiative forcing; the relevance of stratospheric H2O for radiative calculations and temperature trends is being actively discussed at present (Solomon et al., 2010; Ravishankara et al., 2012; Wang et al., 2012).

For these reasons we think the manuscript should be published as scientific article.

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).

-The new CoMeCAT scheme is suitable for any global model that includes the stratosphere but does not fully resolve stratospheric chemistry in the region. As pointed by the Reviewer, it is a halfway step compared to the computationally expensive full-chemistry alternative. The fact that the CH4 scheme also performs well in the CTM (validated against the full-chemistry CH4 field from SLIMCAT and HALOE observations) backs up the suitability of the scheme for global models other than IFS.

- The relevance of the scheme for MACC will be briefly mentioned in the final manuscript as, although out of the scope of this paper, it is part of ongoing research.

-Regarding climate effects, we will include the reference to the use of IFS in EC-Earth, clarifying that also the Earth System Model would benefit from an improved stratospheric description in the ECMWF GCM.

-For stratospheric transport diagnostics at time scales relevant for seasonal and climate prediction a tracer with longer lifetime than ozone is required. Ozone is not a good dynamical tracer, especially in the higher stratosphere. An age-of-air tracer, CH4-like or N2O-like tracers are better candidates than ozone to evaluate stratospheric transport. The advantage of a CH4 tracer over an age-of-air tracer is the existence of CH4 observations to compare with.

- 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.

The radiative effects of using the GEMS climatology will be calculated with an additional run of the E-S radiative model.

Also, regarding the effects in the GCM, results from one additional IFS run will be shown that allow the comparison of separate and combined effects of CH4 and H2O when interacting with the radiation scheme.
The comparison with the climatology can only be done for CH4 as IFS uses, by default, an empirical scheme based on CH4 lifetimes to produce stratospheric H2O, not a 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.

In Section 2.1 (page 488, lines 6-7) we specify that the CoMeCAT scheme is used "...*to obtain H2O tendencies in the stratosphere*" we did not introduce the scheme as a full water vapour scheme. We will revise the text to make sure there is no confusion in other parts of the manuscript. The Austin et al (2007) study implements their H2O parameterisation in a model that already includes a methane field, which is one main difference with the IFS model, we will clarify this in the existing discussion on this point (Introduction - page 485). On rewriting the Introduction we will discuss more specifically the differences between our scheme and other existing schemes.

Effects at the tropopause due to the use of ERA-40 and polar dehydration effects are already discussed in the article when showing the results validation (Sections 5.2 and

6.4.2 in the original manuscript). In the final manuscript we will also mention these aspects when discussing equations (1) and (2) in the Introduction.

<u>Specific</u>

- 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.

We will edit the Introduction keeping these comments, as well as the comments by Reviewer 1, into account.

- 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.

To quantify impacts on the assimilation of radiances, data assimilation runs with IFS would be required, which were out of the scope of the study, and which are not feasible at present. But we will briefly discuss this aspect in the final manuscript, including a recommendation for future research on this.
The first sentence was intended to show the relevance of an improved stratospheric description, pointing towards model aspects and applications that will benefit from the improvement. We will rephrase that sentence to avoid misunderstandings.

- 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.

We think it is clearer if we keep it separate. It is not redundant, and (related to a previous comment by the Reviewer) it helps to show that the main scheme is developed for methane concentrations, while for water vapour we use the scheme to obtain tendencies in the stratosphere. We will change the title of the subsection to "Scheme for water vapour tendencies" to make it more consistent.

- 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.

The coefficients are, of course, available for research on request. We will include a sentence making this clear in the paper.

- 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.

Yes, we will include this as part of 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.

We will take into account this suggestion about sections 3 and 5 for the reorganisation of the manuscript (the comments made by Reviewer 1 also need to be accommodated in this respect).
As for the water vapour tendencies, if the focus were on H2O tendencies we would not be able to compare model results against observations, there are no observations for stratospheric H2O tendencies but for concentrations.
Differences between model runs at the tropopause level are more clearly seen in Fig.7 (as pointed by the Reviewer earlier, the tropopause is a key level on dealing with H2O distributions); we will edit the main text to clarify what is learnt from keeping this figure.

- 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.

We will reorganise the paper, considering the suggestions made by both reviewers.
In particular, nudging effects and radiative forcing sub/sections will be edited to accommodate the comments by both reviewers and to make clearer the scientific relevance of these two aspects of our study.

- 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.

It is not in disagreement, the runs included in Table 2 did not couple CH4 to radiation. That is why the 'fipj' run was not included in that Table, as in this case CH4 is indeed interactive with the radiation scheme. We will add one sentence in the main text clarifying that in the runs included now in Table 2 CoMeCAT CH4/H2O are not interactive with the radiation scheme.

- For Section 7 it is explained earlier (p. 496; l.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; l. 24-29).

Net radiative effect obtained with the GEMS climatology in the E-S model will be shown in the final manuscript (see also our response to related comment by Reviewer 1, as well as to previous remark by Reviewer 2).
Figure 12 shows the differences between a constant CH4 value and the CoMeCAT CH4 field in the radiative E-S model. The use of a constant CH4 profile is still the default CH4 option in off-line radiative models like the E-S model; since these models are widely used for climate research, the scientific message contained in Fig.12 is appropriate to show the importance of an improved stratosphere in climate simulations, with respect to the standards currently used. We will explicitly mention this in the final manuscript.
The reference to the effect of contrails puts our results from the E-S model in a quantified context, providing a comparison with a radiative effect, obtained with the same model, that is relatively small but still it is acknowledged that needs to be taken into account in climate simulations.

Technical
p.499, l.19: In the tropics the 100 hPa level is most likely situated in the upper troposphere (TTL) and not in the lower stratosphere.

OK, we will edit this.

p.505, l.5-8: Is it really needed to refer to two different issues of the same book?

We will edit this to refer only to the most recent version.

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

OK, we will do this.

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?

Yes, CoMeCAT is one parameterisation for both CH4 and H2O. But, for every model run, it can be chosen whether to activate both or just one of them. We think it is clearer to keep this caption as it is. We will include a brief explanation.

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

OK, we will expand the caption.

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.

OK, contour units will be changed and values clearly specified.

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

OK, contour values will be clearly specified.

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

Same as above.

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 fig5a/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.

Values plotted were for centre of grid cell latitudes, that is why the last value appears at around 85 degrees in these figures. We will avoid the white features in the final plots.

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

OK, we will include a brief explanation on the latitudinal range of HALOE data.

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

The caption already included that information; we will delete the text above the figure to avoid confusion.

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

OK, we will do this.

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?

As the colour scale has been used here, white means either close to zero difference or beyond the maximum values in the scale (see colour bar). We don’t think this case is confusing because the white region around 10hPa is clearly contained within a region of maximum positive differences.

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.

OK, labels will be edited and white colour will not be used for both extremes.

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

Thanks, we will add this reference as well as the ones listed below, regarding the points discussed in the responses given above.

Additional references:

A. R. Ravishankara (2012), Water Vapor in the Lower Stratosphere, *Science*, 337 (6096), 809-810, DOI:10.1126/science.1227004

Wang, J. S., D. J. Seidel, and M. Free (2012), How well do we know recent climate trends at the tropical tropopause?, J. Geophys. Res., 117, D09118, doi:10.1029/2012JD017444.