Review of “On the uses of a new linear scheme for stratospheric methane in global models: water source, transport tracer and radiative forcing” by Monge-Sanz, Chipperfield, Untch, Morcrette, Rap, and Simmons.

The paper describes a new linear parameterization of stratospheric CH4 chemistry, CoMeCAT, which is designed for global models that resolve the stratosphere but do not include full photochemistry. The parameterization is similar in form to earlier linearized ozone photochemistry parameterizations, in that the loss rate is described in terms of a basic state value plus perturbation terms describing the effects of local variations in CH4 and temperature. The perturbation terms are evaluated using the TOMCAT box model, which is initialized using output from a fully interactive SLIMCAT model simulation that was forced with ERA-40 and ECMWF operational winds. The box model calculations were carried out for the period Jan-Dec 2004. The coefficients computed with the box model, and the reference climatologies for CH4 and T computed with the SLIMCAT model, are then used to specify a CH4 loss rate, which can be assumed to approximate a H2O production rate throughout the stratosphere where total hydrogen is constant. The COMECAT parameterization is then tested in both the SLIMCAT model and the ECMWF GCM, and differences in CH4, H2O, temperature, and radiative forcing are evaluated.

While I appreciate the level of work involved in carrying out and analyzing these simulations, in its present form the manuscript does not represent a substantial contribution to scientific progress within the field of NWP, data assimilation, or global modeling. This may be in part due to the nature of the problem (i.e., CH4 chemistry in the stratosphere), and in part due to the presentation of the material and methodology. My comments below address each part in turn.

Regarding the nature of the problem: The motivation for developing and implementing CoMeCAT is not clear. What scientific problem will this work address? The Introduction (p 481, lines 1-20) discusses the need for NWP/DAS models to accurately depict radiatively active trace gases such as H2O and CH3. This is not a disputed fact. However, the authors then claim that the description of such gases is “in many cases still too simple for current stratospheric purposes”. What does this mean? Are there studies that can be cited in support of this point? Further down, it is stated that “One of the current problems is the poor representation of CH4 found in most GCMs…”. Again, what references can be provided to support this statement? If by GCMs one means NWP models, this is probably true. If it means GCM’s such as coupled chemistry climate models, it is probably not true. For NWP purposes, one could argue that including CH4 chemistry is unnecessary because CH4 chemistry in the stratosphere (i.e. then net production of H2O due to CH4 oxidation) is very slow compared to transport times, for example. This can be seen in Figures 2, 3, and 4, where the photochemical lifetime of CH4 is on the order of years, and the sensitivities of the CH4 loss rate to changes in CH4 and temperature are extremely small. What exactly is the problem that CoMeCAT will address?

Later in the Introduction (p 486, lines 5-18) it is mentioned that CoMeCAT has the potential to improve radiance assimilation and to provide a tracer for diagnosing transport in a GCM. If these two points are in fact the main motivations for developing CoMeCat, they should be discussed in more detail at the start of the Introduction. The results of the paper should also, consequently, clearly show how the CoMeCAT parameterization may contribute substantial new results in these areas of research.

Regarding the presentation of the results: The paper needs to be better organized around the main significant contributions of CoMeCAT. Currently, it lists the results of many different applications of CoMeCAT in different models, but the overall significance of the results is unclear. For example, Figs 8 and 9 discuss the differences in temperature between ECMWF GCM runs using with CoMeCAT or GEMS CH4. Outside of the winter extratropics, the differences are small, and it is difficult to determine what, if any, relationship exists between the CH4 differences in Fig. 9 and the temperature differences in Fig. 8. More importantly, it seems very likely that any differences in zonal mean temperature will be due to the radiation scheme using different zonal mean CH4 distributions rather than any photochemical effects parameterized by CoMeCat. In this sense, Fig. 9 seems to be testing differences between GEMS and SLIMCAT CH4 and could in fact have nothing to do with the linearized photochemistry scheme, which is the subject of this paper. The same seems to be true for Fig 12, which is really evaluating radiative forcing differences between an assumed global 1.8 ppmv CH4 value and a reference CH4 state (as a function of latitude, altitude, and season) determined from SLIMCAT.

It would help if the results presented in the paper could clearly show that (a) CoMeCAT CH4 chemistry can improve NWP/DAS treatment of radiance assimilation and (b) CoMeCaT CH4 helps diagnose transport in GCM's, as these seem to be the main motivations for developing CoMeCAT. As part of this, the paper should clearly show that CoMeCAT is producing CH4 or H2O tendencies (or increments) in the stratosphere that are large enough matter for these applications. As a start, one simple way to do this would be to plot the size of the tendencies from each of the individual terms in CoMeCAT (equation 16) as a function of altitude for a realistic profile of T and CH4.

Overall, I would recommend more effort in rewriting the Introduction section to motivate the research, and refocusing the analysis of model results to clearly show how the photochemistry in CoMeCAT has the potential to improve radiance assimilation for NWP and/or transport diagnosis in GCMs.

Specific Comments

1. The abstract is probably too long, and lacks focus. Rather than listing every result in detail, it would help to summarize the key results and their significance. If possible, reducing the number of acronyms in the abstract would improve its readability.

2. The Introduction needs to be re-organized. As mentioned above, it seems as though the main motivations for CoMeCAT are not discussed until the very end of this section. It would help if these were discussed much sooner, and previous work on the subject should be cited where relevant in motivating the current research (i.e., why is CH4 photochemistry needed, why are current methods unstatisfactory, etc. )

3. Are longitudinal features in CH4 really important (p. 482, line 18)?

4. Equations 2-6 are not really equations. The way this is written, it is very confusing. Can these be condensed and written as proper equations?

5. Why do NWP models need CH4 chemistry? (p 487, line 8) They need a good background CH4 climatology, but do they need CH4 chemistry unless they are assimilating CH4?

6. As mentioned above, the entire discussion in section 6.2 seems inconclusive as to whether or not CoMeCAT CH4 chemistry impacts temperature in long GCM simulations. I would recommend eliminating this section.

7. Why do Figs. 6 and 7 plot these latitudes? It's not clear from these figures what the CH4 chemistry is contributing.

8. I didn't follow the discussion on p. 498 regarding the nudged experiments. What exactly is CoMeCAT's role in helping to explain the differences here?

9. The Figure 1 caption should state that the CH4 and T reference terms come from SLIMCAT.

10. The contour labels on Fig. 2, 3, and 4 are illegible for the most part. Please consider revising the units to make the plots easier to label (e.g., Fig. 2 could be plotted in years), and reducing the number of contour lines.