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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Diagnosing CH_4 models using the equivalent length in the stratosphere" *by* Zhiting Wang et al.

Anonymous Referee #2

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This study evaluates the performance of global Eulerian atmospheric transport that are used for studying the global sources and sinks of atmospheric methane using MIPAS data. The method makes use of a quantity called equivalent length, which is used to quantify the degree of mixing from observed and model-derived tracer distributions. The comparison between models and measurements provides interesting insights in the dynamical processes in the stratosphere, as well as the performance of the models in simulating those processes. This information is relevant since estimates of methane fluxes derived from inverse modelling of satellite data rely on the performance of transport models in the troposphere as well as the stratosphere. The application of equivalent length to this specific problem is novel and interesting, and therefore in principle suitable for publication in ACP. However, in my opinion the scientific significance is still

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too much limited for a number of reasons listed below, calling for further modifications.

In general, these concerns the following:

1) The quantification of differences between MIPAS and the models. The current description is very qualitative, which makes it difficult to verify statements that one model is performing better than another. Conclusions are drawn about the surf zone as a key region explaining shortcomings in the models. However, many differences can be seen in the figures. Therefore, without further quantification it is not clear whether there is any objective quantitative support for this conclusion.

2) The suggestion is made that model deficiencies in reproducing the stratospheric surf zone are important when using these models to investigate methane observations from satellite instruments such as SCIAMACHY and GOSAT. However, the impact of the identified errors for simulating total column CH4 as observed by these satellites remains unclear. Is this a significant factor explaining differences between models and data from such satellites?

3) The method to calculate Le is explained pretty much the way it was done in the previous publications that are referenced. Since this study focuses on its application, further information is needed how to apply the concept to actual model output and measurements. An attempt is made in this direction, but still too brief to fully understand how this was done. In part, this may also be a language problem. Therefore, I recommend that the description is checked by a native speaker.

4) In the text, several references are made to regions in the figures. However, they are something difficult to trace back looking at the figures. I'm hesitant to recommend adding arrows etc. in the figures, because several of them are rather busy already. However, nevertheless some further attempt should be made to make the connection between the figures and the text clearer.

5) Further specification of the offline global models is needed, distinguishes between

the driving meteorological fields that are used and parameterizations that are used for tracer transport. In the discussion section a deficiency in planetary wave strength or wave dissipation is mentioned as possible explanation for problem of the models reproducing the surf zone. Later the more favorable performance of LMDz is attributed to the scheme that is used for tracer transport. However, these are really different solutions to the problem: the first has to do with the dynamics inside the GCM which generated the met. fields, the second has to do with tracer transport in the offline transport model. Offline models as TM3 and TM5 can take meteorological fields from different GCMs (or different versions of the same GCM). That information is needed to properly judge the results. For example, it makes quite a difference whether TM3 used NCEP or ECMWF winds.

6) I listed a couple of grammar corrections that I encountered while reader, but the list could be much longer. Further effort is needed to improve the style of writing.

In addition, specific corrections are needed addressing the following:

p1, l25: 'In the stratosphere, ...'

p1, l30: 'in inverse modeling'

p1, l30: 'is smoothing CH4 gradients' ?

p2, l34: 'for stratosphere-troposphere'

p2, l34: 'using lagrangian . . .' The studies that are referenced indeed point to a better performance of lagrangian models. In this case, what is the reason for concentrating only on Eulerian models in this study? It would have been very useful to compare with a lagrangian model. To what extent would that solve the problems that are identified here?

p2, line 35: 'langrangian'

p2, line 39: 'SCIAMACHY'

p2, line 43: 'in' i.o. 'within'

p2, line 69: 'is' i.o. 'are'

p3, line 71: 'if the gradients in mixing ratio ... does not change along the isolines' Generally it will change along the isolines, at least within some finite interval. What happens in that case?

P3, line 91: What is the random and systematic uncertainty of the MIPAS measurements? How important are its uncertainties for this analysis?

p3, line 92: 'location' i.o. 'record'

p4, line 97: 'apply to' i.o. 'on'

p4, line 100: method is quite difficult to follow, for example here: how do you fit local areas to grid points?

p4, line 109: "Equivalent latitude" falls kind of from the sky here. As I understand now Le is a relative quantity compared to a reference for which you use equivalent latitude, right? These things need better explanation for others to be able to use the same approach.

p4, line 120: 'mixing on one'

p5, line 126: please spell out 'Jun. to Nov.'

p6, line 180: Some motivation is needed why the main text singles out comparisons with TM5-4DVAR, whereas the comparisons with other models have been moved to the supplement.

P7, line 205: Here, and in other places, wind directions are mentioned. It should be made clearer where this information comes from and that this information is not shown in the figures.

P11, line 310: The reference list is not in alphabetical order.

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