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

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We thank the reviewer for this comprehensive review. We have taken the comments into account and modified the manuscript.

RC: 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.

AC: The main text is largely modified and the main conclusions now include: 1) the

mixing strength in the surf zone is weak in the models compared to the measurements. There occurs a vertically and horizontally uniform region in CH4 mixing ratios in the lower part of matured surf zones in the measurement but not in the models; 2) the modeled polar and subtropical barrier is not as tight as the measured ones; 3) the modeled polar vortex break too fast. The remnants of the polar vortex are not resolved by the models. Because the difference between the measurement and the models is much larger than difference between the models for all these bias we do not state any more that one model is performing better than another. The mixing strength is represented as magnitudes of the Le. The transport barrier is recognized as large meridional gradients in CH4 mixing ratios in Fig. 4. Two new figures (Fig. 5 and S5) are added to diagnose/quantify evolutions of the southern polar vortex. With these modifications we hope the conclusions are supported by figures more clearly although we are not sure if the revised description satisfy the standard of quantification.

RC: 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?

AC: In this work we concentrate on studying the deficiencies of the chemical transport models in the stratosphere. So far, the influence of these deficiencies on simulated total columns of CH4 are not checked. However, the initial motivation was to help improving performances of the CH4 models in the stratosphere and finally obtaining more accurate inversion of CH4 sources. But the results should not be understood as to be limited to inverse modeling community. It is always important to find errors of the chemical transport model, even if the model is not used for surface emissions inversion of chemical tracers. The revealed model deficiencies should be applicable as well if the model is used to simulate other gases, such as OCS, N2O and CO etc.

RC: 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.

AC: The diagnostic tool of Le is developed and described in detail in Nakamura (1995, 1996) and Nakamura and Ma (1997). It might not be suitable to describe the calculation in too much detail in our manuscript because the goal of the work is to explore the measured and modeled mixing properties in the stratosphere instead of Le itself. Too much detail in analysis procedure will dilute and disturb the theme. The calculation of Le includes following steps: 1) obtain a CH_4 mixing ratio field on a isentropic surface. 2) construct the function q(A,t). Because CH_4 field is represented on grids whose area is easily calculated. For each value of mixing ratio we sum the area of all grids whose CH_4 mixing ratio is lower than this value. In this way we can obtain the function. We calculate the square of horizontal gradient of CH_4 mixing ratio at each grid. In similar way we get its areal integral as a function of CH_4 mixing ratio and then the A. 3) the Le is calculated according to the definition in the manuscript.

RC: 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.

AC: Considering this problem we have largely modified the main text in the manuscript. We believe that now the revised text is now connect to the figures in a better way and are more easily understood.

RC: 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.

AC: The TM3 and TM5-4DVAR are driven by reanalysis datasets ERA-interim and ECMWF-IFS. The LMDz-PYVAR is a climate model and creates its meteorology through nudging to reanalysis data. This information has been included in the revised text. It is true that the planetary wave breaking is largely related to dynamics inside the GCM. However the planetary waves seen in the simulated tracer field can be influenced by transport scheme. The favorable performance of LMDz is not attributed to the transport scheme any more in the modified text since we are not sure the extent to which the simulated planetary wave depend on the applied transport algorithm. We only mention that improving transport algorithm is an possible approach to improve the simulation of the planetary wave.

RC: 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?

AC: These grammar corrections have been incorporated in to the revised text. The Eulerian model is more computationally efficiency than the Lagrangian model. As a results the former is more widely used, e.g. inverse models usually use the Eulerian transport scheme because its high requirements for computational efficiency. Even if the Lagrangian model allows to solve the problem identified here it is still necessary to evaluate the Eulerian model and improve it.

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?

AC: These grammar errors have been incorporated into the modified text. With this sentence we tried to explain the physical content of the Le. If we assuming the gradients do not change along the isolines then the Le will be the geometric length of the isoline.

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

AC: There is not validation report available for the V6 data of ESA.

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?

AC: The grammar errors have been corrected. At each grid point we collect the satellite measurements that are located within 3° latitude and 5° longitude. Then a surface is fitted to the collected measurement and the CH4 mixing ratio at the grid point is taken as the value of the surface at this point. This procedure is applied to all grid points and in this way a CH4 field is obtained.

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.

AC: Yes, Le is normalized against the length of circle at specific latitudes. In this way the normalized Le will reflect the extent to which tracer contours are stretched. Without stretching Le should be equal to the circle length and the log normalized Le is zero.

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.

AC: The grammar errors have been corrected. We select the TM5-4DVAR as example because its deficiencies is most significant in terms of the polar vortex and surf zone. Comparisons with all models are moved to the supplement because it is appropriate to include too many figures in the manuscript.

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.

AC: In the Figure 2 and S4, the wind is represented by thin black lines with solid lines for westerlies and dashed lines for easterlies.

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

AC: The reference order has been adjusted.

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