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**Interactive
comment**

Interactive comment on "Diagnosing CH$_4$ models using the equivalent length in the stratosphere" *by* Zhiting Wang et al.

Zhiting Wang et al.

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We thank the reviewer for this critical and comprehensive review. We have taken the comments of the reviewer into account and modified the manuscript. Modifications in the text are written in RED.

RC: Usually I am pretty much in favor of short papers, provided they are written in a clear and conclusive manner and give all necessary information. Unfortunately, this is not the case here. In my view, the manuscript does not fulfill the basic requirements of scientific writing. From what is written in the text it is not at all clear to me what has been done, why has it been done, what is the goal of the study, what are the results, what is new, how do the methods and results compare to previous studies? For example,

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Discussion paper

section 2 does not include any details of the analyzed model simulations, except the resolution of the models. The presentation of the results in section 3 and 4 lacks a clear thread and line of argumentation. Furthermore, parts of the text are hard to read and would benefit from a better language. Some specific examples are given below.

AC: We followed the comments and Sect. 1, Sect. 3 and Sect. 4 in the manuscript are largely modified. In the Sect. 2 we added further details about the model simulations.

RC: Besides my criticism concerning the presentation quality I have some major concerns about the approach and applied method. The Le diagnostic is used as measure of isentropic mixing, although the authors state in section 4.1 that diabatic processes also have an influence on methane and, therefore, Le derived from methane mixing ratios. So in my view the described method is not appropriate to diagnose isentropic mixing and transport barriers. How do you want to make sure that the shown differences between MIPAS and the models are related to shortcomings in the simulated horizontal mixing and not to different representations of diabatic processes? Furthermore, from Wang (2016) I take that the models are not driven by the same meteorological data. As far as I understand the LMDz-PYVAR model is not at all driven be reanalysis data, but by the model's simulated dynamics. If this is the case, it does not make sense to compare individual years of observations with model output, but one would need long time series for a statistical analysis. In general, I have the impression that the whole analysis is mainly based on eyeballing, and I miss a quantitative analysis. This comes back to the question of the main goal of the study: Do you want to do a case study of a specific year or evaluate the model performance in general? Another question is related to the methane chemistry in the models. How is methane oxidation treated in the models? Are there substantial differences or shortcomings that could have an impact in the vertical gradient of methane in the stratosphere?

AC: The wave breaking induced mixing process is fast compared to the main diabatic processes like residual circulation and vertical motion associated with the semi-annual oscillation. These slow processes provide a background tracer distribution on which

the mixing process acts. In the extratropics the planetary-wave breaking has a dominant influence and is an adiabatic process. The surf zone and polar barrier indicated by Le are consistent with theoretical predictions. E.g. the surf zone develops in the winter stratosphere, large Le occurs and CH4 mixing ratio is meridionally uniform because of isentropic mixing. Zonal jets correspond to isentropic transport barrier and should have small Le, such as the polar barrier located at the polar jet. We state the diabatic infuence in Sect. 4.1 previously just as a possible explanation for the difference between calculated Le here and that in Haynes and Shuckburgh (2000) in the tropics. However, we made a mistake because their calculation is for altitude 400-850 K that is different from 450-2000 K here. Below 850 K our results are similar to their calculations in which the Le is calculated from adiabatically transported artificial tracer. This has been corrected.

The LMDz-PYVAR predicts the meteorology by the climate model LMDz through nudging to reanalysis data. So comparisons with the measurement can be done for individual years. In this study we want to evaluate the model performance in general. But model biases in specific years are important as well from the view point of the inversion. Methane oxidation is treated by prescribing monthly radical concentrations of OH, Cl and O(1D), without interannual variations. These radicals are produced by full chemistry models and certainly have influence on vertical gradients of stratospheric methane. It is not clear if there are substantial shortcomings and differences in the prescribed radical concentrations.

Specific comments

No abbreviations like Jun. or Nov. in the text (e.g. L123).

AC: These are corrected in the modified manuscript.

L125: replace ", Some. . ." with ". Some. . ."

AC: Corresponding changes have been done.

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Interactive
comment

Sect. 2: As mentioned above I think the description of the models and methods is insufficient. Some examples: How is methane treated in the models (chemistry, emissions)? Which meteorological data are used to drive the transport models? Which years are simulated? 2009-2011? Is the model output treated in the same way as the satellite data, i.e. also interpolated to 1deg x 1deg? How about the vertical resolution? Are the model data sampled at the same vertical levels as the satellite data?

AC: The methane chemistry is treated with prescribed radical fields, emissions are inverted though optimizing modeled CH4 against CH4 measurements at the surface. The applied meteorology data are ERA-Interim, ECMWF-IFS and predictions under nudging for TM3, TM5-4DVAR and LMDz-PYVAR. The TM3 and TM5 runs from 2005 to 2012, LMDz runs longer than the first two models. After interpolating the model outputs to the measurement time and location the later treatment is completely the same for the model and satellite data, including the gridding. The model outputs are sampled at the same vertical levels as the satellite data. The vertical resolution is different for each model and the satellite, and hard to define after the interpolation is applied.

L78/79: What do mean by this sentence? And which in situ surface measurements are used?

AC: The sentence has been changed to "The modeled CH4 4D fields are obtained through inversions of CH4 surface emissions with in situ surface measurements assimilated.". The in situ measurements mainly include background sites of NOAA and a few other sites which are different for each model.

L99-103: For calculating Le the MIPAS data are first binned into a 4deg x 4deg horizontal grid and then interpolated to 1deg x 1deg? Do I understand this correctly? What is the purpose of interpolating the data to a finer grid? You cannot create more information from coarse satellite data by interpolation. How does this affect the calculation of Le ? Please clarify. It might be helpful to add a schematic or an example of MIPAS

on a specific isentropic surface. Are the model data treated in the same way?

AC: Yes the MIPAS data are binned into a $4°x4°$ and interpolated to $1°x1°$ afterwards. The algorithm of central difference is applied to calculate the derivatives referred in the definition of Le. The function we built, q(A,t) (tracer mixing ratio as a function of the area bounded by its isolines) should be smooth for an easy calculation of the derivatives of the function. The interpolation to a finer grid avoids degradation of the actual resolution in the calculation of derivatives. The effects on the calculation of Le have not been checked but is not necessary since we just want to compare the model and measurements. The calculation procedure of Le is the same for the model and measurements.

L109: I think φe is supposed to read φe ?

AC: Yes, this has been corrected.

L116/117: I do not agree with this statement here. For example, the TM5-4DVAR model shows higher tropical methane mixing ratios at the top than MIPAS.

AC: Figure 1 we used to show that the tropical reservoir (about 400-1000 K) is not outstanding in the modeled CH_4 distribution compared to the measurement, e.g. the sharp boundary at the subtropical barrier in the measurements. The sentence now is "In the models the high CH_4 mixing ratio region in the tropics (compared to the extratropics), however, has a lower vertical extent and a leaking boundary compared to MIPAS."

L155: hpa -> hPa

AC: This is modified now.

L172/173: I do not understand this sentence. How is vertical mixing related to the fact that methane decreases with altitude?

AC: This sentence means without vertical mixing the CH_4 mixing ratio should decrease

with altitude. The sentence now reads "There should be vertical mixing in that region since CH4 mixing ratios would have decreased with altitudes in the stratosphere.".

L181/182: Here you state that the models do not capture the horizontally and vertically well mixed surf zone between 450-850 K and 60S-30S. How about the quality of the satellite data? How reliable are MIPAS data at these altitudes?

AC: Similar to MIPAS, the model biases relative to the ACE-FTS data also show a large negative value in this region (an indication of a small vertical gradient in measured CH4 mixing ratio). So both MIPAS and ACE-FTS reveal the existence of a region vertically uniform in CH4.

L235: Why do the models do a better job in 2009 and 2011? As mentioned above a evaluation based on three years makes only sense when the models are all driven by (the same) meteorological reanalysis data.

AC: It is not clear to us which factors give better performance for the models in 2009.

L254: Replace "sink down motion" by "downward motion"

AC: This has been corrected. L265: What is meant by "jet exit region"? The edges of the jet streams?

AC: The "jet exit region" means the area downstream of the maximum wind speed of a jet.

Discussion and conclusions: Your discussion of interhemispheric differences in gravity wave activity is not conclusive. Wave activity is expected to be stronger on the northern hemisphere than on the southern hemisphere. The same holds for the discussion of different transport schemes. Sensitivity tests using one CTM with the same transport scheme with different horizontal/vertical resolutions or the other way round would be helpful, but I see that this is out of scope.

AC: This section has been reorganized and a new conclusion section is added. The

discussion here is to explore possible reasons. It is true that gravity waves are stronger in the northern hemisphere and their breaking could occur mostly in the upper stratosphere and mesosphere. For us it is not clear which processes contribute to such uniform region. However, numerical model tests might give a definite answer but this is not the aim of this study.

Fig 1: What is here, zonal mean or zonal median CH_4 mixing ratios?

AC: It is the zonal median value to avoid extreme value of the measurements.

Fig 2: Black thick and thin contour lines are hard to distinguish. I would suggest different colors for CH_4 mixing ratios and zonal winds. Furthermore, it would be helpful to use a different, lighter color scale for Le . Furthermore, why are there missing values around 1850 K for TM5-4DVAR in February 2010?

Fig 3/4: Again, black contour lines are hard to read with a dark blue background.

Supplement: I would like to see all the figures, that are intensively discussed, in the main paper and not in the supplement.

Difference plots: I would recommend a different color scale, e.g. red-blue, for all figures showing differences. That makes it easier to identify positive and negative differences.

AC: The plots in the manuscript have not been changed. We tested different color scales, but decided that the current version is the best.

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