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Interactive comment on "Atmospheric transport and chemistry of trace gases in LMDz5B: evaluation and implications for inverse modelling" *by* R. Locatelli et al.

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This paper presents an evaluation of the vertical mixing scheme in LMDZ. Three different configurations are tested. The newest version, named "NP", modifies the near-surface mixing, but also shows different interhemispheric transport. Moreover, the authors show the effect of vertical resolution on stratosphere-troposphere exchange.

The paper is very well written, and the results are presented in a well-structured way. I have mainly small textual comments, but one main issue needs to be addressed in the final version of this interesting paper.

Main Issue

The paper starts with an analysis of near surface mixing, by comparing to a LES case for shallow cumulus convection. The case is made here that the plume parameterization improves the comparison of LMDZ with LES. Hereafter, the authors start to analyze 222Rn. Here it is shown that the schemes (NP v.s. TD/SP) differ mainly in the stable boundary layer (e.g. figures 4 and 6). Differences are not driven by the plume model, but by the diffusion scheme, and possibly by an interaction with near surface temperatures (i.e. the cold bias) that stabilizes the nocturnal boundary layer. However, not much emphasis is placed on the vertical diffusion and nocturnal boundary layer heights. It would be very good to analyze nocturnal boundary layer heights (text mentions that NP NBLs are shallower), and associated K diffusion profiles (i.e. Louis v.s. Yamada). In that respect, it would also be interesting to study a "clear" boundary layer, without clouds and to compare LES with the column model using the different versions. This will highlight the effect of diffusion.

One of the main reasons not to include night-time observations over land in inversions is the poor representation of these observation in models. Figure 4 shows a clear improvement, but figure 6 shows a clear deterioration using NP. So, inclusion of night-time observations in inversions remains tricky, I guess. Besides, the near surface vertical resolution plays a role in the representation of (very) stable temperature gradients.

Apart from this, the remainder of the paper (focusing on the rectifier and the CH4 lifetime) completely ignores the fact that the night-time mixing has been identified as a main issue using 222Rn. Thus, all-day averages of CO2 and CH4 are taken at the surface (or between surface and 850 hPa), mixing in the night-time issue. The discussion, however, ascribes the differences mainly to convection and plume transport, while it would be interesting to know how a day-time only comparison would look like. Day-time mixing is more strongly related to convection and plume transport, while night-time mixing is dominated by the diffusion scheme. So, I suggest to analyze also day-time only averages, especially in the CO2 and CH4 analysis.

Minor comments:

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Page 4995, line 23: “equilibrium value” unclear without context

Page 4996, line 20: Two new papers (Houweling & Bergamaschi) appeared recently.

Page 4998, line 15: “which have been ..... TM5 model” → “which is underestimated by the TM5 model, as shown in Patra et al. (2011).”

Page 5002, line 11: at → in

Page 5003: Maybe explain a bit better that LES resolves (dry) updraft and downdraft motions. Also, I miss the horizontal resolution that is used in the LES. It is very unclear to introduce “KE”, can that not be avoided?

Page 5004: The NP scheme strongly underestimates the cloud fraction (5% compared to 20% in LES). Please discuss how this may affect vertical mixing.

Page 5004, line 25: “in an opportunistic way”?

Page 5005, line 5: “at” → “in”. Also look at the referencing (use brackets).

Page 5005, line 23: degrees N is missing.

Page 5005, line 27: “The Table” → “Table”

Page 5007, line 12: “are much better for NP (1.13) than for TD (0.42).” Not obvious to me why 1.13 would be better if NSD represents the standard deviation of the (model-obs) values normalized by the mean.

Page 5008, line 1: “concentration at” → “concentrations at the”

Page 5008, line 24: “are probably very different”, explore, see main issue.

Page 5009, line 2: “diurnal” → “the diurnal”

Page 5010, line 17: “one can wonder how much”: rewrite.

Page 5011, line 26: Here it would be good to mention how large the adjustments were, because this links also to mass-balance and stratosphere-troposphere exchange

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(more for 19 layer version).

Page 5012, line 14: higher → steeper

Page 5012, near line 20: Vertical exchange cannot be directly linked to the IH gradient, unless you explain how this would work.

Page 5013, line 8: "transport in the PBL". I think that the seasonal rectifier is not restricted to PBL mixing, but pertains to the vertical mixing in general (e.g. more convection in summer).

Page 5013, line 23: exposes → displays

Page 5013, line 25: "emissions" → "exchange", also elsewhere.

Page 5013, line 27: how much is this correction? Does is differ for the different configurations?

Page 5014, line 18: uses → use

Page 5014, line 25: "can explain" → explains. Please analyze also day-time only, see main issue.

Page 5015, line 3: cannot → do not

Page 5015, line 9: validate → to validate

Page 5015, line 11: contribute → contributes

Page 5015, line 18: done → made

Page 5016, line 6: exhibited → presented

Page 5016, line 16, 28: SF6 → the SF6

Page 5016, line 24: same → the same

Page 5017, line 15: "resolutions ....that", "resolution impact the location of the

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tropopause height much more than". In the line below you claim that the 39 layer version performs better. Without a validation, you cannot make this statement.

Page 5018, line 16: I think this statement (role ozone) is out of context for this "off-line" chemistry with only OH. As explained in the "main issue" you should explore the effect of near-surface mixing (on the yearly means), in order to separate this from the temperature effect. If temperature-effects on the k (CH4 + OH) are a dominant effect you should quantify the lifetime better (how calculated?).

Page 5018, line 23: NP/SP ?? NP is different, right?

Page 5018, line 29: units are missing.

Page 5019, line 3: CH4 → the CH4. You could quantify the effect of T on k by evaluating integral (k.OH.CH4)/integral (OH.CH4)

Page 5020, line 1-5: Given the results of Lutjewad, this seems an overoptimistic statement.

Page 5020, line 14: PYVAR → the PYVAR

Page 5021, top: Why do you conclude that inversions have a bias? We do not know the CH4 emissions. I think you should carefully remark here that the conclusion is based on biases found for e.g. SF6.

Page 5041, "three letters" → three-letter

Page 5044, Italics in caption?

Page 5047, "resulting in biosphere" → "resulting from biosphere"

Page 5048: If you have 39 level simulations, it would be interesting to include these, to investigate the effect of stratosphere-troposphere exchange on the long-term methane budget.

Interactive comment on Geosci. Model Dev. Discuss., 7, 4993, 2014.